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BY THE
UNITED STATES NUCLEAR REGULATORY COMMISSION'S
ADVISORY COMMITTEE ON NUCLEAR WASTE

APRIL 22, 1997

The contents of this transcript of the proceedings of the United States Nuclear Regulatory Commission's Advisory Committee on Reactor Safeguards Nuclear Waste on APRIL 22, 1997, as reported herein, is a record of the discussions recorded at the meeting held on the above date.

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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91st MEETING

ADVISORY COMMITTEE ON NUCLEAR WASTE

(ACNW)

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TUESDAY

APRIL 22, 1997

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ROCKVILLE, MARYLAND

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The Review Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Paul W. Pomeroy, Chairman, presiding.

COMMITTEE MEMBERS:

PAUL W. POMEROY, CHAIRMAN

B. JOHN GARRICK, VICE CHAIRMAN

WILLIAM J. HINZE, MEMBER

GEORGE M. HORNBERGER, MEMBER

ACNW STAFF PRESENT:

John T. Larkins, Executive Director
Michele Kelton, Technical Secretary
Richard K. Major
Howard J. Larson
Lynn Deering
Andrew C. Campbell
Richard P. Savio
Michael Markley
Carol A. Harris
Sam Duraiswamy
Theron Brown

ACNW CONSULTANTS PRESENT:

Bruce Marsh
Michael Ryan
Kenneth Foland

ALSO PRESENT:

John Trapp
Chuck Connor
Brittain Hill
Tim Sullivan
Kevin Coppersmith
Gene Yogodzinski

ALSO PRESENT (cont.)

Tim McCartin

Abe Van Luik

Mike Bell

Wes Patrick

A-G-E-N-D-A

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

(8:37 a.m.)

CHAIRMAN POMEROY: The meeting will now come to order.

This is the first day of the 91st meeting of the Advisory Committee on Nuclear Waste. During today's meeting, the committee will discuss the status or studies related to igneous activity at the proposed Yucca Mountain Repository.

Ms. Lynn Deering is behind me there. Standing up is the designated federal official for today's session. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

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

It is requested that each speaker use one of the microphones, identify himself or herself, and speak with sufficient clarity and volume so that he or she can be readily heard.

Before proceeding with the first agenda item, I would like to cover some very brief items of current interest. The first item of interest is, of course, as many of you know, on April 15th the Senate passed the

Murkowski Substitute Amendment to Senate Bill Number 104 of the Nuclear Waste Policy of 1997 by a vote of 65 to 34.

That vote is two votes shy of the required number to overcome, override, a Presidential veto, if that should happen. And on April 10th, Representative Upton introduced House Resolution 1270, the House version of the high-level waste legislation. So, that's moving forward.

The second item is that Undersecretary of Energy, Tom Grumbly, announced his resignation in late March. Grumbly will join his former boss, Hazel O'Leary, at ICF Kaiser in Fairfax, Virginia, where he will become president of the Federal Programs Group.

The third, DOE told the Nuclear Regulatory Commission that it believes NRC can take over the regulatory responsibility for certain DOE nuclear facilities at an annual cost of roughly 75 million dollars.

In late 1995, NRC estimated regulatory costs to be around 300 million and an increase of work force of 1200 to 1400 employees. According to DOE projections, the NRC will, at the end of a ten-year transition period, assume regulatory responsibility for approximately 200 DOE nuclear facilities.

Those were all the items of current interest I have. Do any of the other members wish to make any

opening comments? If not, let's turn to the first item of our agenda. Namely, the discussion of the status of igneous activity related to the proposed Yucca Mountain Repository.

Dr. Hinze is the lead member for this discussion. And he will chair the sessions throughout the day today. Bill.

MEMBER HINZE: Thank you, Paul. It's finally arrived.

CHAIRMAN POMEROY: I'm appreciative of that.

MEMBER HINZE: To experts or laymen who visit Yucca Mountain, volcanism certainly is one of the more visible of the potential hazards to the repository site. And this has been identified by the NRC in their program of the vertical slice where volcanism igneous activities is a KTI.

The problem of volcanism and igneous activity in general as a potential disruptive effect is exacerbated by the infancy of the science of the volcanic prediction.

The resulting uncertainty and the interpretation and the incompleteness of the data sets available has made this into one of the more contentious issues of Yucca Mountain and also has led to the consideration of this item in a probabilistic manner.

The potential problems and the prediction of

igneous events and the potential serious consequences of an event has made this one of the most highly studied and certainly highly debated of the Yucca Mountain project.

Several groups have been investigating the volcanism issue. Bruce Crowe, Greg Valentine and their cohorts at Los Alamos National Lab have been studying this problem for nearly two decades and have been advising the DOE on this.

They have summarized their work in a final report that has been made available to us. The state of Nevada, Gene Smith, Gene Yogodzinski all have made intensive geological studies and probability studies of the potential for igneous activity at Yucca Mountain.

The NRC also has been very active and has spent a considerable amount of resources, time and money, in this study of the volcanism issue. The Center for Nuclear Waste Regulatory Analysis and particularly Chuck Connor and Bruce Hill have spent a great deal of time looking at the consequences and also the probability of volcanism.

Finally, and most recently, DOE has conducted through Geomatrix and Kevin Coppersmith an expert elicitation on the probability of an igneous event. Their report has recently been completed and is being acted upon, we understand, by the Nuclear Regulatory Commission

staff.

Today, we will be hearing from all of these parties. And it is appropriate and timely that we do so because the DOE wishes to close this issue out and has terminated the data acquisition phase.

The NRC, we understand, is also considering producing a staff technical resolution paper, that may not be the exact wording, on the probability of the volcanism at Yucca Mountain.

These studies have led to probabilities that are converging, probabilities of occurrence of volcanism that seem to be converging. Nonetheless, there are differences. And one of the things that we wanted to determine today is what is the significance of these differences to the dose that may be received by the critical group?

The use of consequence models to predict the hazard from volcanism that had been carried out by the Center suggests a very minimal risk to the critical group of the Yucca Mountain Repository. We need to understand the assumptions, the sensitivity, the robustness of these models.

In terms of the objectives of the working group of our meeting today, frankly, I don't expect that we will be hearing much in the way of new information.

Perhaps some small parts here and there.

What we do hope to accomplish is we hope to receive a comprehensive overview of the results and what they mean to the bottom line, to the exposure of the potential critical group.

What we do hope to learn is the basis for any differences remaining among the cognizant groups that I mentioned, their importance, and what is being done, what can be done to minimize these differences if they are important.

I should also point out that the ACNW in its next two meetings in May and July will also be conducting brief sessions on the volcanic activity, the igneous activity. In May, the committee will be taking up expert elicitation.

And the probabilistic volcanic hazard analysis undoubtedly will come up at that time. In July, the committee will be looking at reviewing the performance assessment in the high-level waste and the Nuclear Regulatory Commission.

And we understand that the volcanic activity, the igneous activity, will come up at that point. For the presenters, Lynn Deering has prepared an excellent background document for this meeting, which I hope everyone has received.

And in that, she has included a long, long list of excellent questions. I'm not going to repeat those questions now, but I do want to emphasize a few that I would like to have us all be thinking about.

First, what are the principal causes of the differences in the probability obtained by the different methods? Can these differences be reconciled?

What is the significance of these? If a range of probability is the appropriate result, how will the range be folded into the performance assessment?

Second, dealing with consequences. Consequences is an important element in determining the risk from igneous events. How are the DOE and the NRC investigating the consequences and what are the results?

Is the Suzuki model which deals with tephra dispersion appropriate for this study? Are there any other types of models that can be used to judge the Suzuki model?

Third, at what point will the NRC close out the study of igneous events? And what steps will lead them to this?

And finally, how are indirect effects of igneous activity being studied and the risks determined? I think that if we can answer all four of those questions with a reasonable assurance, whatever that word means,

then we'll be in pretty good shape.

We have a full schedule. And we're going to be hearing from the four different areas of study today. We will conclude this afternoon with a round table which we hope that everyone will be able to bring their questions so that we can have a good interchange.

That does not mean that we won't be asking questions as individual presenters move along. With that, I would like to introduce the consultants that we're pleased to have with us today.

We have three distinguished geoscientists with us. At the far end is Dr. Bruce Marsh, professor of geosciences at Johns Hopkins; Dr. Mike Ryan, a research scientist with the U.S. Geological Survey in Reston; and Ken Foland, professor of geosciences at Ohio State University.

We are very pleased to have you. And I'm sure that you will want to feel like an integral part in asking questions and making comments. With that, do any of my colleagues, with or without ties, have anything that they would like to add before we call upon our first speaker?

Okay. With that, we are going to be introduced to this topic by the NRC lead on igneous activity, John Trapp. John, the floor is yours.

MR. TRAPP: I'm always glad to come here to

these nice, intimate gatherings with a few hundred of my friends and go from there.

CHAIRMAN POMEROY: Continue if you feel that way for the next 30 seconds, John.

MR. TRAPP: I was asked by Lynn to give a few introductory comments and basically to cover three basic points, kind of give an overview of what to expect for the rest of the day, give a few comments on some of the technical feelings we've got on the PVHA report.

And I'll also talk a little bit about the effect of the change in the, I'll call it the, EPA standard and how that's affected our work.

Basically, most of what you're going to hear today is going to be a summary of what was presented during the meeting between DOE, NRC, the technical exchange which took two days.

As such, because of recovering this material in about three-quarters of a day followed by the round table, we're going to be going through an awful lot of material awful quick. I hope that most of you have had a chance to read the NRC yearly report, etc.

A lot of the information that you're going to hear today is summarized in that report. And I hope you've also read the PVHA report from DOE.

In addition to this, you will hear some

summary statements about work that's being planned by both DOE and NRC, where we're going as far as this whole thing. One of the big differences in this meeting from the technical exchange is we will get a chance to hear some of the views of the state.

This was not present, and it basically offers both the political and technical opinion which needs to be brought into consideration. We will not be talking about a lot of the generic concerns with expert elicitation or questions, this type of stuff. That's basically going to be handled in your May meeting.

MEMBER HINZE: Right.

MR. TRAPP: And we will go a little bit into TSPA, but not to a great deal because TSPA again will be covered during your July meeting. So, a lot of these questions actually will be partially covered today.

And hopefully, between now and July, we should get most of the questions at least answered to a reasonable extent that were raised by Dr. Hinze.

MEMBER HINZE: John, is it appropriate to ask what are the staff's plans? Are you going to get to that in terms of --

MR. TRAPP: The staff's plans will be covered in basically the last presentation I'll be making today.

MEMBER HINZE: All right.

MR. TRAPP: So, I'll be going through what we're doing both technically and otherwise.

As you know, the EPA standard has been kind of in flux for several years now. If you take a look at the old standard, it really was very strongly probability-based, probability in relation to the accessible environment.

And if you take a look at a lot of the probabilities that have been tossed around in igneous activity, you end up with an awful lot which are sitting right at this magic number in the old EPA standard, 10^{-7}.

Because they sat right on that point and because there were different opinions as to whether we're above and below this point, it meant that we were having a very, very strong focus on our probabilistic aspects.

If you take a look at the proposed recommendations for the new standard and some of the things that are in the Senate bill, what we're now talking about is release, transport, and dose to population. When I say population, all my health physicists get all upset.

As such, the focus on probability has been much less. But because before all we had to do was worry about getting the material on up into the accessible environment, we weren't worried about the transport aspect.

And we weren't worried about going through all of the heath physics calculations. As such, since we started getting the feel as to where the standard is going, we have spent quite a bit of time going into the transport which is part of the Suzuki code, which was mentioned, and trying to calculate how this would affect population in some critical group.

In PVHA, I'm only going to bring up a couple major points. We do have an ongoing concern on what is the effect that new information is going to have all the way through.

An expert elicitation really is kind of a slice in time. And there will be new information. Now, it's very easy to take this new information and analyze it from a mathematical standpoint.

There's a question, what also does it do to things like the underlying conceptual model that the people had? This is something that really cannot be analyzed. And it's kind of hard to figure out how these affects would be.

From a technical standpoint, our biggest concern really is the zonation models which were placed or used by many of the experts. Trying to basically not just talk about their zonation model, but if you take a look at most of the work that was done by the state, there also

was a zonation model there.

And the point is, by taking these different types of zonation models, the assumptions that are laid within these zonation models can really, totally drive the probability obtained.

This last statement on here really is kind of self-explanatory except, like I said, we do have a full range including the state numbers. Now, if you take a look at the range that you've got that is in some of the literature, you end up with things down to 10^{-10} which comes out in the PVHA up to some numbers which almost get up to 10^{-5}.

We consider the PVHA basically as one input. We consider the state's stuff that was published in various technical journals as another input, and we consider the work that was done by the Center as another input. We've got to take a look at all this to come up with the final conclusion.

CHAIRMAN POMEROY: John, before you leave that, let me ask a couple of questions. First of all, with regard to new information, we all, I think, recognize that that's both the generic and the specific issue for this particular elicitation.

Isn't the first thing you need to talk about or think about whether that information is of any

importance or significance to the calculation? For example, would one additional aeromagnetic anomaly have any effect on this study, for example?

How do you propose to treat new information that comes up as you get into looking at this? Do you really see it merely as calling attention, calling DOE's attention, to new information and say, "Find out whether this is important?"

Or do you intend to actually make some studies? Does the staff intend to make some studies to determine its importance?

MR. TRAPP: It's more the latter. What we'd be doing was if we get new information, take a look at it, try to determine if we think it has some significance.

Of course, all of the information would be passed onto DOE, but the emphasis that we'd put on it really would be based on our opinion as to its relevance, yes.

CHAIRMAN POMEROY: But you recognize clearly, of course, the problem of getting inside somebody's head to see whether it actually is important to a particular expert or group of experts.

MR. TRAPP: It's almost impossible. And that's really in some ways our concern.

CHAIRMAN POMEROY: Right. And I guess the

heart of the question I think is will every expert elicitation run into this same basic problem as far as NRC is concerned?

MR. TRAPP: I'm not going to answer it for the NRC because it's really too broad a question. And if I did, I'd probably have the managers down here all over the place.

CHAIRMAN POMEROY: Plan to have several questions they're going to try and do that.

MR. TRAPP: But, yes, I do see it as a problem because there are several areas of characterization which basically we felt were not really fully carried out by DOE. It's mainly the reason why we went into some of the geophysics work, some of which has been reported in the last EOS article.

With the fact that this characterization probably has left things in a much more uncertain state than we'd like, there may be more of a problem in this one than in some of the others.

MEMBER HINZE: Bruce, please.

MR. MARSH: In this regard, how do you plan to keep abreast or understand theoretical developments? In other words, you get new information or the existing information may take on a different complexion based on more understanding of magnetic transport, magnetic

processes, which is a rapidly developing field.

So, I just wonder have you thought about melding these together and what impact this will have?

MR. TRAPP: I think we're blessed by having a very good support staff at the Center. Chuck and Britt have been keeping well up on all the new material. Very honestly, the best answer I can give to it is I depend on those two to take care of it. I'm not sure how else to answer your question.

MEMBER HINZE: Paul, did you have some follow-ups?

CHAIRMAN POMEROY: Yes. John, let's turn for a second to concern with the geologic basis of zones. I'd like you to help me out a little bit there. What is the nature of the concern, that you don't think the geologists knew, or the volcanologists knew, what basis --

MR. TRAPP: The simplest form, if you take a look at the PVHA, and this will come up in some of what Chuck will be presenting, and it will all definitely come up in some of the stuff Kevin Coppersmith will be presenting.

There is a question as to the nature of the transition from Crater Flat to Yucca Mountain. If you attended the DOE/NRC meeting on structural geology, exotectonic models last year, the thought processes that

came out of here is that Yucca Mountain is part of the Crater Flat basin.

If you take a look at the DOE synthesis report in geophysics and DOE synthesis report, I believe, Seismic Tectonics is the correct title, both of those come up at the same conclusion that Yucca Mountain is part of the Crater Flat basin.

If you take a look at the zones that were drawn within the PVHA report by many of the experts, there was a boundary either hard or soft, etc., that was drawn basically on the topographic difference between the two.

We see no geologic or geophysical reason to draw this type of boundary. It's basically the reason that Chuck and his models have gone the way they are.

So, from a technical side you will see how we're handling it right now. And the concern, very honestly if you take a look at it, is not that the numbers are different, but the meaning of the numbers that are in PVHA versus the numbers that we've got are slightly different.

What we're talking about basically for our probability numbers is volcanic eruption and dispersion through the repository. If you take a look at the DOE numbers, they're really talking more secondary effects, dikes, etc., and this type of thing.

If you get down to a number that would be very close to our number, it would be something like about $6x10^{-9}$. So, basically, you're talking about maybe an order, order and a half magnitude lower than ours.

So, it's a disagreement in the geologic interpretation, but it's really what this interpretation means to overall risk.

CHAIRMAN POMEROY: Certainly. And we're going to get to that bottom line question also. I do want to just get into this a little bit further. Do you feel that, therefore, the ten experts on the PVHA panel did not consider data?

I think there's a specific statement in there someplace that they decided that structural control was not the issue that they were going to consider, particularly in reference to that boundary.

MR. TRAPP: No, they considered all the information that they had. Geomatrix did a very good job of giving them the information that was available at that time.

But from a technical, both from that and a regulatory perspective, we're going to take a look at all the information, etc., and try to figure out exactly what it means, how we're going to take a look at it from a regulatory perspective.

We are basically being more cautious towards the public by having these types of numbers. From an overall standpoint, when you get to overall risk, I am not sure that most of it really makes that much difference.

We need to find out. But we're talking, no matter how you do it, extremely low probability numbers. And if you're going to have risk, you got this multiplication factor. And there's a limit as to how far you can go.

One in a million is probably about as far as you can go in probability. It may be higher, but that's reasonable. If you compare that to something that's got uncertainty, you're going to have a hell of a lot more consequence to make the risk equal.

CHAIRMAN POMEROY: Let me ask you one last question, John. It's a stated and avowed purpose of all of these elicitations to try to bring forth the full range of uncertainty that the community would express with regard to any specific issue that the elicitation is addressing.

And presumably, the experts were asked to try to do that. Does the 10^{-7} to 10^{-8} range that the NRC feels is appropriate express the full range of uncertainty of the community?

I note, for example, of course that it doesn't

include the 10^{-9}, 10^{-10}, nor in fact does it include the states of higher probabilities.

MR. TRAPP: But if you take a look at the number, it basically includes the mean value of the PVHA, and if you take a look at the values that come out of the states, especially the last ones which are basically ranging from $1x10^{-7}$ to I believe --

There was a value quoted in the report of $8.8x10^{-6}$, but I think the actual number that they were talking about was $3x10^{-6}$, but the best estimate of just over $1x10^{-7}$. So, our number basically includes the mean values.

And really when you start putting these type of things into a total system performance assessment, it's the mean value which really drives the whole curve. Therefore, if you start going from the 10^{-8}, 10^{-7}, I think we are covering the mean of all areas that I've talked about.

But no, we haven't covered the total range of uncertainty. That would be, like I said, about 10^{-5} to 10^{-10}, five orders of magnitude.

CHAIRMAN POMEROY: Yes, I'm sure that, for instance, I know Atomic Licensing and Safety Board would want to look at that whole range of uncertainty, would want to at least know about the whole range of

uncertainty. Thank you, John. That's all I have.

MEMBER HINZE: I think these first two items will be coming back to haunt us as we move throughout the entire day. At least I hope they will. Is that it then, John?

MR. TRAPP: That's it for me. And, therefore, with no further ado from Chicago and on his way to Vienna, as you may know, I'll let Chuck cover it.

MEMBER HINZE: Chuck, you're going to give us a summary of the work on geological setting and probability, right?

MR. CONNOR: Right.

MEMBER HINZE: And there is a handout.

MR. CONNOR: Thanks. Is that on?

MEMBER HINZE: It's always good to have you here. And we're interested in what you have to say.

MR. CONNOR: Okay. As you mentioned, I'll be trying to condense a couple of presentations that a few of you have heard before. I'd like to talk about the geologic setting and probability of volcanism at the Yucca Mountain Repository based on some studies that we've been doing at the Center.

And, of course, I'm not going to discuss the entire geologic setting, but just those that are particularly relevant to volcanic hazard assessment and

also some data that we've been gathering out ther[illegible] the past year or so which is roughly new information

Here's an outline for what I want to go through. I want to talk about the regional structural setting of basaltic volcanism near Yucca Mountain.

People have recognized for a very long time that small volume basaltic volcanism structure has a relationship. And it's important to identify that relationship and hopefully try to use that in constructing probabilistic models for the volcanic hazard.

Second, I'm going to talk a bit about some recent ground magnetic work that my colleagues at the Center and I have been doing in the area around Yucca Mountain. Yucca Mountain is an area not only of basaltic volcanism, it's also an area of a lot of tectonic activity.

And there are subsiding basins there which tend to obscure some volcanic features. I think it's important to understand patterns in basaltic volcanic activity between from the miocene through the pliocene and quaternary.

In order to do that, we need to rely on some geophysical data in order to identify the locations of buried cones in the alluvium.

And third, I'll summarize some of the geologic

factors and go through some of the probability models that we've been developing to try to get a handle on our particular range, which has already been mentioned.

It's a critical step, I think we could all agree, it's a critical step to understand what we're talking about looking at this volcanic field. The Yucca Mountain Repository, proposed repository, is located here.

This is a rather complicated map for this presentation. But I just want to point out a few features on it. Here is the quaternary Crater Flat alignment. Lathrop Wells volcano is down here, also quaternary volcano.

Several aeromagnetic anomalies located in the Amargosa Valley first identified by Langenheim and her coworkers at the USGS. There are five here. We've recently done some surveys in here which I delineated three volcanoes at this location.

MEMBER HINZE: Are any of those not shown on the aeromag map?

MR. CONNOR: Sorry?

MEMBER HINZE: Which of those are not shown on the aeromag map?

MR. CONNOR: Well, of these anomalies, they are all identified by Langenheim on the aeromag map. I'll go back to it, but this one in particular is a little

obscure on the aeromagnetic map because of its relatively low amplitude.

This one, for example, Anomaly B is actually a big volcano. And that covers about 16 square kilometers, pretty thick accumulation of lava flow for this kind of system.

So, these are the large volcanoes that we can readily identify in the alluvium out there from the aeromagnetic data. There is pliocene volcanism and some quaternary volcanism up to the Northwest, the funeral formation, pliocene volcanic field, 20 volcanoes or so down here.

I think Gene Yogodzinski is going to talk a lot more about this, the geochemical and isotopic setting of this volcanic field. But he and Gene Smith discuss the Amargosa Valley volcanic isotopic province, which basically encompasses all of this basalt to the east and north of Death Valley.

I'm going to zoom in here a bit and take a look at again the location of the proposed repository. This is the area of Crater Flat with the quaternary basalt and so on. And I want to point out that people have tried to identify the system over time in a couple of different ways.

I think it was in 1989 that Frank Perry and

Bruce Crowe defined the Crater Flat volcanic zone which is this north/northwest trending zone which encompasses the quaternary pliocene basalt -- in Crater Flat, heading up toward the quaternary little cones in Black Peak.

And that's later been extended to the south to encompass these anomalies. So, the idea is that the volcanism is concentrated in a long, thin zone like this. And of course, defining a zone in that way tends to decrease the probability for volcanism at the repository site.

The state of Nevada proposed something called the Area of Most Recent Volcanism, which was extended to include Buckboard Mesa, and thus the repository location itself.

And Gene Smith and Ho paid a lot of attention to this kind of prominent northeast trending alignment here in quaternary Crater Flat, also the northeast alignment of Little Black Peak and Hidden Cone and suggested that this aspect of things was important.

They proposed a structural zone extending, very narrow extending from Lathrop Wells up to the repository which yields high probabilities of volcanism.

So, between the, I'll just call it the Crater Flat volcanic zone model in this northeast trending model, defining the structural zones gives you a lot of variation

in probability.

I think it's quite important to look at the geology relative to these zonal models. And these are three cross sections through Bare Mountain. Let me put this one back up for a second.

These are cross sections basically taken through Bare Mountain and across the proposed repository through Crater Flat. These two are balanced sections put together at the Center at the USGS.

This one is an internally deformable crustal block model put together by DOE. And basically, up here, we've got the Bare Mountain fault bounding at half -- and which encompasses Crater Flat and Yucca Mountain and extending east into Jackass Flat.

This model is essentially the same except that it sort of bounds the depth of this detachment fault here. In this case, it's about ten kilometers. Down here, it's about five kilometers. There's some debate about that.

And then, again, the deformable block model is a little bit simpler. But I think for our purposes today, we can say that these models are similar in that they're relatively high-angle faults close to the surface.

At any rate, extending down to some depth, maybe five kilometers, maybe deeper beneath Yucca Mountain and Crater Flat. It's basically an extensional setting,

robin or half robin kind of situation.

Maximum extension must be somewhere over in Crater Flat. And there isn't really much evidence for some prominent structure between Crater Flat and Yucca Mountain, prominent structure.

That is, crustal structure, which would prevent magma from rising through Yucca Mountain rather than Crater Flat, something like that. So, that's as far as I want to take that structural model, but we can come back to it if you want.

It's also possible to look in -- view of this structural setting. And I'm going to use some of the gravity data put together by the U.S. Geological Survey and others in the Yucca Mountain area, and NTS, the NTS area over the last 40 or 50 years.

There's a tremendous amount of gravity data. Maybe the best data set in the world is collected in this area. This map consists of about 8,000 gravity, individual gravity stations.

The -- quaternary basalt in Crater Flat is shown here in yellow. The Amargosa Valley anomalies are just shown by the yellow dots here. And again, the repository is shown there in red with gravity varying across the region, of course, due to lateral density variations in the crust.

What you can see here is that the Bare Mountain fault is quite a prominent feature on this map. And that volcanism, quaternary volcanism, is tending to occur east of that fault.

There is some debate about where the Bare Mountain fault goes as it trends into, actually out of southern Crater Flat. You can see a relative gravity low extending south of the area.

This feature has been called the Amargosa trough. And the pliocene basalts, actually all their ages aren't known. But these aeromagnetic anomalies in Amargosa Valley fall within that trough.

On the west side up here, that's bounded by something termed the Gravity Fault in the alluvium, and its extent north is somewhat more difficult to sort out what's happening here. And if you recall, the structural cross sections, certainly the west side of the system, was pretty well defined by the Bare Mountain fault.

And that series of faults out here make the eastern boundary a little bit less distinct. This huge anomaly up here is the Timber Mountain Caldera Complex.

MEMBER HINZE: Have you ever considered -- the alluvial anomalies?

MR. CONNOR: I'm sorry?

MEMBER HINZE: Have you ever considered

stripping off the alluvial anomalies to really get at the bedrock gravity?

MR. CONNOR: Yes, I have. And in fact, I had a conversation with Vicky Langenheim about this. And she mentioned that they are doing that, or at one point were doing that on a regional case.

Not only for the subset of data I've been using, but for the entire region. So, that's something that would be really worth doing of course. And it's apparently in progress. But I haven't done that myself.

MEMBER HINZE: Is that part of the Yucca Mountain project of the U.S. Geological Survey?

MR. CONNOR: It's not clear to me because my understanding is they're basically not funded to do that work right now. So, I don't know if she and her colleagues are doing that on their own somehow or what.

But that was something she planned to do. And she actually published a paper about that and the high-level management waste meeting notes which was sort of getting started on that.

MEMBER HINZE: Well, let me ask you. Do you feel that you have sufficient amount of data on the alluvium to prepare a strip map?

MR. CONNOR: I think so. There's a lot of well data, of course, in Crater Flat. And the gravity

data, of course, are great. So, I don't think that's going to be too bad a problem to do that.

MEMBER HINZE: It certainly would make your arguments I think a lot stronger because you have these superimposed effects -- surface is killing you or accentuating.

Before you take that off, let me ask you another question. The first of your conversation here is towards the faults and the volcanic centers. There is, to my knowledge, only one instance in this area where there is any igneous activity in a fault.

That's the Solitario Canyon up there at the north end of Yucca Mountain. I think that there are structural geologists that will tell you that more important than faults to this structural control on volcanism are the joints.

Can you justify the concern here about the steep gradients and about the faults that they represent in this discussion of the geological setting?

MR. CONNOR: Yes. I think that joints and faults are both important in transporting the small volume basaltic magma in the shallow crust. And as some of you recall, some of the work we've done involves the dilation tendency of faults through joints to a particular stress orientation.

So, in the Yucca Mountain area, minimum horizontal compressional stress oriented about like that, so I would imagine the faults or joints of that orientation would have a slightly higher tendency to dilate than faults of other orientations.

MEMBER HINZE: If they're vertical. Right.

MR. CONNOR: Yes, exactly, if they're vertical. So, that's an important consideration, the orientation of these faults.

Obviously, maybe not obviously, but I think there's less of a tendency for a fault like the Railroad Valley fault which is an orientation like that to dilate and host magnetism perhaps than faults of other orientation.

MEMBER HINZE: Is there a decent map, is there an appropriate map, of the tectonic joints of this area?

MR. CONNOR: Not that I know of. I don't recall anybody -- maybe I'm sure on Yucca Mountain itself there is a map, joint sets in the Yucca Mountain block itself.

MEMBER HINZE: Well, I was wondering if in considering the dilational aspects of the faults which trivets through and around Yucca Mountain, did you consider the joints at all?

MR. CONNOR: No.

MEMBER HINZE: Should they be considered?

MR. CONNOR: It's possible, but I think what -- for instance, Paul Delaney has done a lot of work on joint sets and dike intrusion on joint sets. And this is something that's happening on a scale of hundreds of meters.

And once these probability models we're dealing with are dealing with scales of kilometers, I hesitate to say that joint patterns, say in the Paintbrush tuff, are going to have an important influence on whether volcanism is going to occur here or over here.

MEMBER HINZE: But if the action is in the hundred meter level range, if that's what really is controlling it, maybe it has to be broken down into that kind of a scale in order to solve the problem.

MR. CONNOR: Yes. I mean, you know --

Let me back off and say that I agree with you that joint or fracture orientation is important with respect to the regional stress field. There are some good examples of that.

We were out in the San Rafael volcanic field with Paul Delaney a few weeks ago and saw great examples of that. I am not certain how to incorporate that into a probability model at this point. But we do to a certain extent, looking at this dilation tendency on the faults

themselves.

Yes?

MR. MARSH: Yes, Chuck in this context of what Bill is asking about, the joint density across the map may be very interesting to look at.

MR. CONNOR: Right. I'd like to get into this a little bit later when I am showing some specific probability models, but a serious problem in incorporating structural data into the hazard analysis is alluvial cover.

If I made a probability model based on mapped joint density all the volcanism is going to occur right here because that is where the mapped joints are. There is this big alluvial basin over here.

MR. MARSH: But the difference is that you do hav· a structural map and you know the basic units. In over words, pre-Cambrian sediments, et cetera that run through the area.

So, you know basically the jointing characteristics in these from the bedrock and the areas with outcroppings. So, it actually may give you by having some joint density map -- as you are talking about it now, you are assuming sort of an equal uniformity in texture and joints probability across here. But it may not be true. And that may be specific to the various rock types.

So, in other words, since you do have outcroppings, you do have a decent structural model, you could make a first attempt at this probably.

MR. CONNOR: Yes, especially comparing something like Bear Mountain to the Yucca Mountain.

MR. FOLAND: Chuck, just one more point before you move on from the gravity.

MR. CONNOR: Sure.

MR. FOLAND: What is the scale? What is the dimension of the smallest young basaltic feature that shows up in terms of gravity anomaly.

MR. CONNOR: In terms of gravity, the gravity does a very poor job of identifying basalts and I wouldn't use it for that task.

There are 8,000 data points over here which is a lot of back-breaking work but that is not going to begin to resolve any of these kinds of things.

And I am not using the gravity data to help identify basalt.

MR. FOLAND: Okay, in terms of magnetic, then.

MR. CONNOR: I will get to the magnetics in a second. But the magnetics, it turns out, is pretty good at identifying the basalts in the alluvian core based on the magnetization contrast.

MEMBER HINZE: I am not going to let you get

that off yet.

For the benefit of the Committee and the consultants, while you have that map up there, could you tell us, if I am not asking too much, what we have in terms of mantle velocity anomalies or any indications? I think this is an appropriate topic to make certain that we are all together.

MR. CONNOR: Sure. That is a good point.

I guess it was several years ago, at the suggestion of Linda Kovach who was a project manager at the time in research, suggested that we take a close look at the tomographic data for the Yucca Mountain region. Also, Evans and Smith had published a paper about tomographic data in the Yucca Mountain region.

I wrote a report with Chris Sanders who was then at ASU on tomographic anomalies and their use in this kind of problem.

Evans and Smith basically identified several scales of tomographic anomalies in the area, though they admitted that they weren't using the best data sets; they used what they could.

They had a relatively large, deep anomalies which I think would cover most of this map area, cutting out a little bit of the NTS over here. That was one slow zone in their model.

They tried to identify an anomaly. They thought they identified an anomaly, if I recall correctly, at sort of a mid-crustal level beneath the crater flat area and I believe that has been disputed to a certain extent based on some data coming out of the University of Nevada at Reno.

They also pointed out some features of the small scale structures beneath Yucca Mountain at the edge of Crater Flat and pointed out that Crater Flat structurally tended to extend beneath the repository based ont he shallow tomographic data.

Recently, there has been some work presented. For instance, at the last GSA meeting, there was a paper presented but I cannot remember the author's name unfortunately, from the University of Nevada at Reno. By Glen Biozzi.

He had more data of course and was looking at several tomographic slices, level slices through the area.

What I thought was interesting, and I asked him a couple of questions at his talk, what I thought was interesting was that he was identifying a fairly broad, what I would call north-turning but it was somewhat diffuse, zone at 40 to 60 kilometers. So, a slow velocity zone at 40 to 60 kilometers extending across this region.

He felt that the data weren't sufficient to

completely resolve that anomaly; specifically, he was worried about slow velocities creeping up into that layer depth in his model.

But I thought that was an interesting point because based on Fran Perry's work that is the depth of last equilibrium for these basalts is 40 to 60 kilometers. So, [illegible] you have a slow velocity zone that is kind of nice.

On the other hand, I would say that zone was broad compared to this map. It was hard to do that comparison exactly and I am not going to do it here, but it basically covered this zone.

MEMBER HINZE: But he had a lot of data, a good coverage.

MR. CONNOR: Yes. He felt that the anomaly identified by Evans and Smith at mid-crustal levels for example, was probably a near-surface effect. I carried that away for his discussion, also.

When I questioned him about it he was fairly tentative about this 40 to 60 kilometers thing because he thought some of the slow velocity zones depth were leaking up, and I am not an expert on that data processing technique. But that was something that he was running into.

I want to point out one thing about the tomographic and that is there has been some work done, for

example in the East African rift by a German group who identified some really nice little velocity zones associated with active basaltic volcanic fields.

There has been some slow velocity zones identified beneath, say, the San Francisco volcanic field.

But I am not convinced that the kinds of volumes of partial melt that generate this kind of volcanism are necessarily going to correlate well with a seismic tomographic anomaly.

MR. RYAN: I was wondering if you recollect what the difference was in velocity at 40 to 60 kilometers.

MR. CONNOR: I am not going to tell you, I can't remember.

MEMBER HINZE: I think the velocities are very poorly constrained. It is just a time delay of .2 to .25 seconds.

MR. CONNOR: One per cent comes to mind but don't hold me to it. It was small compared to what you see beneath Caldera or something.

Okay. I just want to quickly go through. I think we have beat this to death a little bit but, this is the horizontal gravity gradient and what I have shown here is the amplitude of the horizontal gravity gradient. High amplitude gradients are dark here and the light areas are

low gradients.

I have also plotted on here the outcrop extent of Miocene basalt. One of the things that intrigued me about the gravity data and the structures associated with these data is that a lot of the basalt does occur near relatively high gravity gradient areas.

For example, at Crater Flat we have this Miocene basalt here up against the Bear Mountain fault. A normal polarized magnetic anomaly that I will talk a little bit more about in a minute.

The Little Cones are up here against the fault. One of the things that I have speculated about is that if the dip of the Bear Mountain fault, the shallowing northward, that may explain why these volcanoes, if they are breaking out of that fault zone at a given depth, it would explain the displacement of these volcanoes away from that fault zone.

In the Miocene there is also good correlation with gravity gradients with, for instance, the Beatty basalts up along Fluorospan Canyon fault. There is a pretty good correlation, I think, between these large crustal features revealed by gravity data and some of the basaltic volcanism.

Of course not in every case though. For instance, you have Miocene volcanoes out here away from a

steep gravity gradient area. So, it is a geophysical data set that I think should be considered.

MEMBER HINZE: Any questions regarding this? If not, then I would like to ask a question.

What is the significance of the pink area that extends south from the proposed repository and then shifts over towards Lathrop Wells?

MR. CONNOR: This area? That is a relatively high gravity gradient area and down here it correlates roughly with the Stagecoach Fault. You can see that it extends up through here.

Well, it is a slightly higher gravity gradient area and I think it fits rather well with the structural edge of the eastern part of the basin, being a little less distinct than the west edge, but extending beneath Yucca Mountain and the site of the proposed repository.

I guess some of the structural geologists have called it the Bow Ridge Fault, which would be the eastern extent of the basin. I am sure there is disagreement about that, if anyone wants to comment about that.

But there is a high gravity gradient there. And that, to a certain extent, shows up in some of the aeromagnetic data that is on the deconvolution of that data that was done by DOE.

MR. MARSH: Is this kind of information

entered into the probability calculation?

MR. CONNOR: Well, in a minute I will show you some attempts that I have made to do that.

MEMBER HINZE: If we could strip off the alluvium in Crater Flat, would the density of faults be approximately equivalent to that of Yucca Mountain area?

MR. CONNOR: That would be my guess. I think that would be approximately equivalent to Yucca Mountain.

MEMBER HINZE: There has been an attempt to map some of those, not just by seismic, which raises all kinds of hackles.

But in terms of the magnetic data, you have looked at the magnetic data of this area probably more than anyone else.

Does the magnetic data support in any way, the occurrence of faults in Crater Flat?

MR. CONNOR: Sure. They're good.

MEMBER HINZE: How about that for a straight line?

CHAIRMAN POMEROY: Awfully good, awfully good.

MEMBER HINZE: It does support it though?

MR. CONNOR: Yes.

CHAIRMAN POMEROY: Chuck, when you use the terms, and I am trying to quote exactly, "a pretty good correlation, I think," --

LAUGHTER

MR. CONNOR: You are going to nail me down here, Paul. Okay.

CHAIRMAN POMEROY: What does that imply to you and is that a conclusion that say all volcanologist would derive from that kind of a map?

LAUGHTER

MR. CONNOR: There are --

CHAIRMAN POMEROY: Answer the first part.

MR. CONNOR: Well, a pretty good correlation -- I'll drop the 'I think'. It is a pretty good correlation. It means that some of the volcanoes lie along these faults and they don't in every case.

So, the data should be considered but it is not the silver bullet. We can't say that all volcanism will occur on these high gradient areas.

MR. MARSH: But you could quantify that by distance correlations from gradient, et cetera.

MR. CONNOR: Right. And you can quantify that and I could talk about that a little bit more in a minute.

But basically, if you take these data into account when you put the probability models together, the locations of past volcanic events are better predicted by that model than if you don't consider the locations of the faults. Okay?

MR. RYAN: Is there any reason to think that in the alluvian covered area that the faults would differ in density significantly or in orientation from their more northerly neighbors?

MR. CONNOR: I think I would speculate that they don't change density or orientation locally. In other words, I feel pretty comfortable over here and actually the aeromagnetic data over here support the idea that the faults are north/south trending and seem to have a comparable density.

I think it becomes a lot more speculative, for instance, down here.

I would like to talk about some ground geophysical data that we have collected at the center in May, July and August of last year. And I have a few reprints available from an EOS article related to these, if you are interested.

I want to talk about a ground magnetic survey we did at Northern Cone, that is the closest volcano to the site. And Southern Crater Flats in the Little Cones area and close to a magnetic anomaly identified by the DOE and the USGS. And then a survey we did over at Amargosa Valley, Anomaly A, also identified by the USGS.

This is a map of the area around Northern Cone. I kind of stretched this image a little bit because

the amplitudes around Northern Cone itself are quite large. So, this is the extent of Northern Cone.

Again, I changed scales on you; this is two kilometers just to reference you and we are about 8 kilometers west of the center of the repository block to Northern Cone.

I made this map because we are interested in the relationship between structure and the basaltic volcanism near Northern Cone.

As you can see some of these prominent faults coming through the area and extend on into the map area in the northern part; these are taken from a Frizzel and Shulterz map.

To make a long story short, we basically identified these gradients across here, I believe, are associated with normal faults, extending through Northern Cone and Northern Cone is a good correlation with the north/south trending fault.

So, I would say in a shallow crust, this basalt exploited a structure on its way to the surface and that structure happened to be north/south trending which is a good, high dilation tendency orientation in this area.

I guess a second point is that there is no evidence from the ground magnetic data that there is a

shallow northeast trending structure along the alignment itself. Remember there is the quaternary Crater Flat alignment at least in the shallow crust doesn't come to light, even though the north/south trending structures are quite clear.

So, one possibility is that there is a master dike at some depth like 10 kilometers and these things are exploiting structures above that master dike of different orientations. Another possibility is that there is no master dike; these are individual events.

MR. MARSH: What about these lines going off in the northeast direction?

MR. CONNOR: These guys over here?

MR. MARSH: Yes.

MR. CONNOR: What I see on this map is basically a fault basically defined by these inflection points. But I am not sure exactly what you are referring to.

MR. MARSH: Those little bulls eyes.

MR. CONNOR: Oh, these guys?

MR. MARSH: Yes.

MR. CONNOR: It is pretty noisy alluvium up there and it could just be a tuff block, a larger tuff block sitting at the surface or something like that. I wouldn't attempt to interpret those beyond that.

Okay. I am going to take you down to just south of Lathrop Wells, Amargosa Valley Anomaly A.

We got interested in mapping Amargosa Valley Anomaly A because it is very close to Lathrop Wells which is the youngest volcano in the area. This map is about three or four kilometers south of Lathrop Wells itself and is an area identified by Langenheim as a possible basalt basin aeromagnetic data, but it was a very complicated anomaly, so we decided to take a close look at that.

We mapped this over a three day period and there are about 33,000 data points on here on something about 60 kilometers of survey line. You can see here that we actually identified three prominent reversely magnetized bodies.

I got nice positive on the north side and large negatives and interpret those to be shallow, reversely magnetized rocks well-explained by a plate-like model, i.e., small lava flows or edifices.

I think the important thing about this map is it has this nice northeast trend to it. This distance is about 4.5 kilometers.

It looks like Amargosa Valley Anomaly A really consists of three very volcanic structures. That fits in with the idea that northeast trending alignments are important and ought to be considered in probability

models.

MR. MARSH: What kind of surface here?

MR. CONNOR: It is a flat alluvial surface.

MR. RYAN: Pardon me.

MR. MARSH: How deep is it?

MR. CONNOR: Two hundred meters, plus or minus 50, something like that. They have magnetizations of about one to three ampmeter is what I am using to derive that depth.

MR. RYAN: If 200 meters is the depth to the top, what about the depth to the overall?

MR. CONNOR: I am modeling them as thin blocks.

MR. RYAN: Okay. And what is the sense of crustal rotation in this part of the study area, clockwise or counter-clockwise, if it could be reckoned?

MR. CONNOR: People have argued for some clockwise rotation.

MR. RYAN: This pattern is consistent with that.

MR. CONNOR: Yes.

CHAIRMAN POMEROY: Just so I understand that, Chuck. I read it but --

MR. CONNOR: Excuse me.

CHAIRMAN POMEROY: The total extent of the

three anomalies is something in the order of 1,000 meters?

MR. CONNOR: This is about 1,000 meters here. It is about 4.5 kilometers.

CHAIRMAN POMEROY: Okay, so 4.5 total?

MR. CONNOR: Yes. One of the anomalies is about 1,000 meters across.

MEMBER HINZE: The dilational tendency when those volcanoes were produced was in what direction?

MR. CONNOR: Well, do you know how old these are? I don't. They are at least 7,000 years old, right? And if you correlate them with aeromagnetic anomaly B, then they are on the order of 3.5 million years old.

So, if they are indeed Pliocene, then it is essentially the same orientation, is my understanding.

MR. MARSH: Well, Bill, aren't the positive in a little peculiar position? Are you getting at the fact that they may have been rotated from where they were placed originally?

MEMBER HINZE: They're much more off to the northwest than you would anticipate.

MR. CONNOR: No, I actually disagree with that. I think the positive wraps around like that.

MR. RYAN: To get at Bill's question, the dilational tendency could actually be a function of depth and three dimensions. One could make a case for a north-

northeastward dilation at the depth at which these anomalies express themselves. And perhaps a north-south or north-northeast, south-southwest dilation at very, very shallow levels.

MR. CONNOR: Well, these are very shallow.

MR. RYAN: Even more shallow than the top of the anomaly?

MR. CONNOR: Well, yeah. I don't know Bill; I don't really think there is much evidence of rotation there.

MEMBER HINZE: Well, you would have to look at the paleomags before you could sure.

MR. CONNOR: Sure, yeah.

MEMBER HINZE: But it does seem to be tilted. We could argue.

MR. CONNOR: Well, if you look at the map, for instance, what I did was I compared this with the anomalies that I saw at Red Cone, for example. And as you know the positive associated with Red Cone is sort of messy and it is out there somewhere; I wouldn't push it too hard.

The third area that we mapped through May and July last year is this area around Little Cones. The area around Little Cones are two volcanoes located in Southern Crater Flat.

If you have been there, you probably noticed that these are really small-looking features. It turns out that because Southern Crater Flat is actively subsiding, Little Cones lava flow have been buried since they erupted about a million years ago. This lava flow is revealed extending about two kilometers south of Little Cones, beneath the alluvial pavement, it is at a depth that varies from about 15 to 20 meters beneath the surface, which John Stematikos used to compare with other estimates of subsidence in the area.

There are also some other features on this map. This is a normal polarity anomaly located south of Little Cones and we interpret that to be a salt body buried at ta depth similar to those at Amargosa Desert on the order of 150 to 200 meters.

I think it is important to realize that that is a normal polarity anomaly which means that although we don't know it's age it is certainly a different age that Little Cones basalts and others in the area which means that there has been volcanism through time in this area and possible along the Crater Flat alignment.

Finally, there is a third volcanic feature here. This is what we interpret to be a shallow dike extending north-northwest of a outcrop of Miocene volcanic basalt at the very southern end of the map.

Although it is quite short, only about 500 meters long, it is parallel to the trend or suspected trend of the Bear Mountain fault in this area.

So, that is what we gleaned from those data.

MEMBER HINZE: Were you able to get a dip on that?

MR. CONNOR: I haven't actually modeled it, I have to admit.

MEMBER HINZE: Let me ask you a question. Are you suggesting that if we went and did the high-density survey on the ground throughout Crater Flat and Amargosa Valley, that we would end up with a number of hits that would in some way change the probability? Is that what you are suggesting?

MR. CONNOR: First, I wouldn't wish it on anyone to make a map of that kind; it's a lot of work.

But I guess my answer to that is, based on these and similar data, what I think is, is that there are something like 10 to 20 aeromagnetic anomalies that bear further investigation. And this is a very efficient way to do it.

So, if I had my druthers, I don't see any reason not to make those maps; it seems like a good thing to do at this point. You can do the calculation yourself, but I imagine that would affect probability.

But I think there is another aspect which John Trapp alluded to and that is what we really need to be careful about here is that we understand the process. The geology process is controlling volcanism in the Yucca Mountain area.

It is not completely understood, it is not completely understood anywhere in the Western Great Basin. We have an opportunity to learn more about the structure of volcano interaction here, using these relatively new techniques; this very rapid data collection kind of technique.

I think it is worthwhile exploiting that, but not in the whole region. I would instead, choose these 10 to 20 anomalies that are pretty clear from perusal of the magnetic map of being potential targets.

MEMBER HINZE: Have you conducted surveys of any of those target areas?

MR. CONNOR: We conducted surveys about a week and a half ago at one of those areas and that data set is pretty preliminary right now.

MEMBER HINZE: What does that mean? It is not going to change, is it?

MR. CONNOR: Well, we are in a formal meeting and I hate to present data that I collected a week ago that hasn't been reviewed.

MEMBER HINZE: Let me ask this question. Is it worthwhile looking at these anomalies on the basis of the most recent results?

MR. CONNOR: Absolutely.

MEMBER HINZE: Okay, that answered my question.

CHAIRMAN POMEROY: Well let me -- can I pursue that?

MEMBER HINZE: Sure.

CHAIRMAN POMEROY: Why -- Certainly it is important, everybody would agree, to understand as much of the geologic process as you can. Yet in a regulatory environment you are probably never going to have the funds and the time to do that, I suspect.

So, my question would be, we have talked about three different volcanic areas here. Is there something in any one of these that the more detailed aeromagnetic mapping has provided to you --

MR. CONNOR: These are ground magnetic maps.

CHAIRMAN POMEROY: Sorry.

MEMBER HINZE: Okay.

CHAIRMAN POMEROY: Is there information contained here that, in your opinion, is not contained within the PVHA uncertainty?

In other words, the uncertainty that people

have given in terms of occurrence and the numbers, it would certainly not, perhaps, be the mean number that they used but is there an importance to this?

MR. CONNOR: You have two questions there. One is there information contained in here that is not in PVHA and the answer is of course, yes, they didn't have all this information. There are three anomalies here. No one in the PVHA treated those as three volcanoes.

Now, the question is, does that make any difference in the long run and I will reiterate.

This teaches us about process and just to give you sort of an indication of that, when this came out in EOS, George Walker who is on the PVHA panel wrote me a letter and said that this is very important because it demonstrates that the northeast trend is very critical in the model.

Now, he didn't say that he changed his model or that his model would be any different or his answer would be different. But it certainly was an indication that these data are helping us close in on the geologic process. Because I believe that, ultimately, it is the geologic processes that are going to drive up our comfort level in deciding whether to put this thing at Yucca Mountain or not or whether volcanic hazards are important or not.

Now, does it change a probability number? Well, of course that depends on the model that you are using. I would think that if it turns out that these ten anomalies are actually Pliocene volcanoes or can be interpreted to be Pliocene volcanoes based on this then that would have a small impact on probability, on the order of maybe a factor of two or three, would be my guess based on some very preliminary things that I have done.

I would be surprised if it changed more than an order of magnitude. I am speculating about that.

Whether that is ultimately important in the context of PA or total system performance is not a problem that I have addressed at this point in time. My hope is that we have the opportunity to better understand the processes before having to reach that decision.

But of course, that is just my perspective.

MR. FOLAND: Chuck, you mentioned a few minutes ago, rapid data collection. Could you characterize how much time is involved in doing one of these surveys?

MR. CONNOR: Yes. This particular survey we did in three days. Basically what we use is a cesium vapor magnetometer manufactured by Geomatrix Geophysical Instrument Company. That is an adjustable sampling rate, but we tend to sample every two seconds or four seconds.

We walk across the ground and if you are moving at a meter and a half per second, that gives you an idea of the amount of ground that we cover.

Interfaced with that is a differential GPS. The differential GPS has a resolution of 20 centimeters, something ridiculous like that. The survey accuracy due to time updates and stuff like that might be on the order of two meters. Of course, in high gradient areas, that is 10 or 20 nanotussels and in low gradient areas like this one that is a couple of nanotussels.

I guess we did that in three days, something like 60 kilometers to traverse across there.

Because we get feed back very quickly or actually while we are collecting the data in terms of graphical output and that sort of thing, we tend to survey these areas more densely. You find out that there is nothing going on out there, it is a long way to walk through the sand, so we tend not to collect data as densely as we do anomalies we can identify.

A good example of that is this survey because we knew that we wanted to understand the extent of this lava flow but we didn't want to map all the high frequency variation in this lava flow. So, there is lava here but you can almost begin to pick out some tracks here but we didn't want to map this noise on top of the flow.

We also changed our survey design when we identified this anomaly. We focused in on that anomaly and mapped that much more than some other areas. Okay?

So, it comes out to be something like 30 to 40 thousand data points. They are all drift corrected and the IGRFs removed and that is all the processing that has been done to those maps.

MEMBER HINZE: Bear with me one moment. I would like to follow up on Paul's question.

It is my understanding from listening to some of the experts involved in the PVHA that they were driven away from Yucca Mountain, I think that is the proper term, by the recurrence of volcanoes in the southerly portion of Crater Flat.

Finding another few volcanic events down in that area will not have a profound affect on what is happening in terms of probability up in the Yucca Mountain area.

As a result of that, it seems to me that the critical anomalies to investigate are any that are in the proximity of Yucca Mountain. Are there any of these 10 to 20 subtle anomalies that you have observed in the Yucca Mountain region, Midway Valley, et cetera?

MR. CONNOR: Again, all I want to do here is speculate because you are asking me about stuff we are

doing right now which is fine. But I want you to understand that we are doing it right now and I haven't had a chance to review it and that sort of thing.

But, yeah, of those 10 to 20 anomalies there are two if, I recall correctly, in Jackass Flat for example, that I would imagine if those turned out to be Pliocene basalts that would have an impact on probability models that drew a hard boundary between Crater Flat and the repository. Okay?

MEMBER HINZE: Yes, sure.

MR. CONNOR: So, I agree that there are definitely anomalies that have a higher priority than others. But I would also reiterate that if we are going to understand the process, more data gives us more confidence, I think, in understanding not only where the volcanoes are and when they erupted but also changes in volcanic activity in the region through time. It also increases our understanding of interaction of structure and say rates of extension and episodes of volcanism.

I think that given that it is relatively straight forward to collect these data, it is worth that kind of attention.

CHAIRMAN POMEROY: But just to clarify it in my mind, Chuck if you can, every incremental bit of data helps us understand the process. At some point you have

to say either that I have sufficient data to make a decision or I don't.

MR. CONNOR: Sure.

CHAIRMAN POMEROY: It is my understanding of this problem that the geologic processes may not be fully resolved in my life time and perhaps not in yours. So, how doyou determine where the cut-off is? Where should we decide if we have enough data or not? How do we make that decision? How do you cut off at some point and say I have sufficient data to make a decision?

MR. CONNOR: Yes.

CHAIRMAN POMEROY: That is something that scientists usually don't like to do.

MR. CONNOR: Sure, sure. I would answer your question this way, and this is only my personal view, I have studied the Yucca Mountain system for five years now and these data improved my understanding of volcanism in the area.

They gave me confidence in understanding the northeast trending alignment which Gene Smith talked about in 1990 and it has been dumped on and what not. But basically, it reaffirms the importance of that.

I think it is important that we see a clustering of volcanism in Southern Crater Flat; that is certainly not the repository site. The more volcanoes

that you identify there it reaffirms our understanding of volcanism clusters.

I think it is interesting that there is a normal polarity anomaly along that alignment, the quaternary Crater Flat alignment. All the other volcanoes are reversely polarized which makes me suspect that that feature may have hosted volcanism over time and that is an important consideration in the probability models.

Some of the probability models coming out of PVHA used changes in volume eruption rates through time to get a handle on recurrence rates.

Well, if you don't know what was going on in the Pliocene because half these volcanoes are buried or something like that, then you have trouble understanding the change in recurrence rates through time based on volume model in a volcanic field like this one where the eruption rates are extremely small compared to a lot of basaltic volcanic fields.

So, we do learn a lot about it. And, if I may be so bold, you are interested in the bottom line. That is something that I am really interested in, too.

It is not such a great exercise to trudge 90 kilometers through the desert to get the data and --

CHAIRMAN POMEROY: I understand that.

MR. CONNOR: And I guess that is an indication

of how important I think it is to get that kind of data. I think that will help us understand issues.

MR. HILL: I would like to add a point, if I may. This is Britt Hill at the Center.

One of the real well-defined characteristics for other volcanic fields in the Western Great Basin is that when they're long-lived, five or six million years of volcanic activity, there tends to be a regular spatial shift through time.

If you look at the Coso field, Reveille Range lunar crater field, Big Pine, just about any big field out there, and the locus of volcanism displays a regular spatial shift.

We have not uncovered that kind of spatial shift on the Yucca Mountain field. There doesn't seem to be a real change from Pliocene to quaternary. None of the probability models that have been developed for the Yucca Mountain field have tried to accommodate a regular spatial shift through time in the locus of volcanism.

It may be, and I am just speculating and one of the things we are going to do is we are going to evaluate the significance of this for that kind of a probability model, is that if we had ten additional Pliocene volcanoes in the Armagosa Desert, we may have to think about models that account for that spatial shift

from a Pliocene locus in the Armagosa Desert to a quaternary locus in Crater Flat.

Now, whether that will make a bottom line difference in the absolute value of probability of the repository site I can't evaluate because our models are not capable of dealing with that spatial trend through time.

But I do think we have an obligation to test that hypothesis and then move forward if we feel that a class of probability models needs to be accounted for to accommodate this new data and the potential spatial shift.

MR. RYAN: What is your sense of shallow to intermediate crustal storage regions for these magmas as you might reconstruct them, and what would the evidence be for that.

MR. CONNOR: I guess I would let the geochemist answer that question. Go for it, Britt.

MR. HILL: It is indirect evidence right now based on the mineralogy, about three per cent olivine is about the only phenocryst in there. There is an absence of plagioclase in part due to what I believe is a fairly high water content in the magmas, about 2.8 per cent water.

There are phenocrysts of tightened partosite to carosotitic amphibole in some of the Crater Flat magmas

in trace amounts. But they are texturally phenocrysts not xenocrysts or megacrysts.

So, the interpretation would be that we are dealing with something that is probably 10 kilobars or greater as the last equilibration depth.

There is very little, if any, evidence for crustal assimilation. Buckboard Mesa, the 2.8 million year volcano has some granitic xenolith to it but it is a small contamination trend. By and large we are not seeing any evidence of shallow mineralogy, eight to shallower kilobar mineral assemblage or crustal assimilation as a significant process.

MR. RYAN: So then, Britt, are you suggesting that these magmas came directly from depths on the order of 30 kilometers?

MR. HILL: I would push it more to 30 to 60 kilometers was there last equilibration depth and pretty much rammed up.

MR. RYAN: That might be higher than 10 kilobars then.

MR. HILL: Well, it certainly would be higher than 10 kilobars. Ten kilobars to me is a petrologic convenience about where the experimental data usually are.

MR. MARSH: What are you basing this water estimate on?

MR. HILL: Experimental work by Green back in the early Seventies on Hawaiite magmas of almost identical composition to this where he had 2.8 per cent water at 10 kilobars and did not have amphibole on the liquidous, but did find a tightened partosite with 5 weight per cent water on the liquidous.

MR. MARSH: That is quite a high water content.

MR. HILL: We have had a lot of discussion and speculation on interpreting very indirect data. We tried to get some ion probe data on melt inclusions but we were unable to find any suitable melt inclusions in a reconnaissance study to get direct measurements.

MR. RYAN: I think I would agree with Bruce that this is a surprisingly high water content.

MR. HILL: I think in the last --

MR. RYAN: If it is systematic.

MR. HILL: If it is systematic. But I think we have seen quite a bit of work in the petrology over the last five years going for hydra-basaltic magmas, especially when they derive from a lithospheric mantle.

MEMBER HINZE: What is the implication of that if it is high?

MR. MARSH: Well, if this whole magma was an amphibole, you could crystalize it all with two per cent

water into an amphibole and none of the basalts that we know in any detail gets anywhere near two per cent water in basalt.

MR. HILL: Certainly some of the work of Grove and Sissom up in the Medicine Lake area would support that.

MR. MARSH: Yes, but there is a lot of contaminated stuff in those lavas.

MR. HILL: Sure.

MR. MARSH: Lots of junk in it so it is very hard to know what you are dealing with in those things. And they have big crystals that are probably restitic chunks from the wall rock and things. If you look at their crystals they have a real history.

MR. HILL: Sure.

MR. MARSH: What I have seen of these, they look pretty clean as you said.

MR. HILL: Very much.

MR. MARSH: It would be very nice to have ion probe.

MR. HILL: It would be nice but we tried in reconnaissance but we didn't have the resources to go a full blown study.

The important point for this question would be that if we have lower water contents, the absence of

plagioclase would imply that we are seeing a deeper magna system rather than a shallower magma system, wouldn't it?

MR. MARSH: Well, the major implication I think would be that the magmas would be a lot more explosive. When you think about the delta V, the change in volume from in solution to out solution can be a factor of 10,000 sometimes.

So, on the surface they look rather normal in terms of cinder cones and the way the craters are spread around. I would expect to see them much more explosive and almost diotrene eruptions with a lot of xenolithic material in the flows coming out near surface.

MR. HILL: I can quickly draw from direct analogy at Cerro Negro volcano in Nicaragua where we do have ion probe data with 3.5 weight per cent water in the melt inclusions in the olivine and, for all intents and purposes, looks like Lathrop Wells.

MR. MARSH: And you have to realize that these melt inclusions in the crystals have been stripped of a lot of their major elements and these actually often happen from melt being struck between crystals and they have been annealed together, so that is an absolute upper limit.

MR. HILL: Sure.

MR. MARSH: So originally, if you take into

account the point of crystallization, it may have only been a half of a weight per cent of water.

MR. RYAN: With the possible exception of Kimberlites I would be very surprised, frankly, to find magnas of this composition ascending directly without pause from 40 kilometers depth. One would expect, actually, some development of shallow and intermediate hostel storage prior to eruption. This is the garden variety case worldwide.

MEMBER HINZE: Is there any consequence to this in terms of probability or the consequences of the effects?

MR. RYAN: Well, depending on the subsurface structure there could be consequences in terms of the combinations of dynamics of dynamics and pathway. If, for example, the subsurface storage region is substantially offset in plan from the ultimate erupt event, and if that storage region is at, let's say three to six or three to eight kilometers depth, and it will offset I suppose 20, 30 kilometers, or 10 kilometers, interesting and perhaps as yet unanticipated consequences for the overall dynamic system and the overall pathway.

MR. MARSH: Especially for degassing. Because it doesn't have to stay there a long time but it has to stay long enough to nucleate and coalesce bubbles and

degas itself to come out in a more normal eruptive fashion. If it came up in one shot, I think as Mike says, you would see a much more dramatic effects on the surface.

MR. HILL: But doesn't that depend on the ascent rate?

MR. MARSH: Well, these things are accompanied -- in terms of the small volumes they have to come fairly rapidly in your mana, if you are going to come up with that chilling entirely.

MR. HILL: Right.

MR. MARSH: But you can hold them at intermediate depths in the crest when the crest is still fairly warm then they can form a chilled margin and protects them from thermal and degass there. But, so those are limitations also.

MR. RYAN: If one accepts the very large water contents you are suspecting, then the exolution of volatiles is going to radically change the momentum environment in which magnas would then reaccelerate once exolution takes place.

MR. HILL: Sure.

MR. MARSH: And retrodive, of course, would be carbon dioxide release and water doesn't usually come out until extremely shallow pressures, as you know. But the CO_2 reconstructed contents would also be of interest. Do

you happen to have a feel for that?

MR. HILL: Not at all.

MR. MARSH: And the other factor that you release this much water is the liquidous and solidous actually rise up through the system very fast and you increase crystallization rapidly. And the fact that these are low crystal entities, the phoenecous contents, crystal entity contents in terms of pre-eruptive crystals looks really low. It means to me that the system is not degassing strongly in terms of water. And so the water -- probably doesn't saturate until shallow depths. Otherwise the liquid would sort of sweep right up in the system very fast and you would get incredibly strong nucleation trends, leaves of nucleation through the whole body, as Rutherford and Kashwin have shown in experiments on the Mount St. Helen's eruptive.

MEMBER HINZE: I think we will go back to this discussion at a round table.

MR. HILL: I'd like to have a sidebar with you on that and how we get anthro liquidous then on these mats. But that's --

MEMBER HINZE: Yes, we can pick --

MR. HILL: -- something we can do around lunch.

MEMBER HINZE: It appears to me, Chuck, that

you are at a reasonable stopping point. What I would like to do is I would like to take a ten minute break and we will reconvene to hear what else Chuck has to say. I must admit that it's unbelievable that Chuck has taken a great deal of time to make this presentation.

MR. CONNOR: Me? I'll take as much time as you want. I'm willing to talk about this in five minutes or 15, it's up to you.

MEMBER HINZE: What time is your plane to Vienna?

MR. CONNOR: Six thirty.

CHAIRMAN POMEROY: We'll reconvene at 20 minutes after 10.

(Whereupon, the foregoing matter went off the record at 10:18 a.m. and went back on the record at 10:28 a.m.)

CHAIRMAN POMEROY: Let's reconvene. Please take your seats.

MEMBER HINZE: Chuck, let's -- as soon as you get wired up let's move on. Thanks for taking the break, Chuck.

MR. CONNOR: So, just to summarize very briefly, I think that from this geologic setting we can glean some information useful and probability model development. One, volcanoes cluster in Southern Crater

Flat. That's been clear from the map distribution of volcanoes. It's also clear from centers identified through magnetic mapping.

Probability models should wind up with higher values than Southern Crater Flat, for instance, than in, you know, at the Yucca Mountain Repository, or something like that. That's an important aspect of the models that needs to be considered.

Second, there is an association between volcanoes and faults. As evinced by the Northern Cone Magnetic Dataset. It's important to consider that somehow in probability models.

Third, is a prevalence of this northeast -- of northeast trending vent alignments. There is, again, two maps and we've now identified a third based on the geophysical dataset.

And four, there is a low and persistent rate of volcanism in the area. Apparently, through time and, of course, additional identification of volcanoes for instance, may change that. But basically this is a volcanic field that has been going for eight million years at a very low rate of magnum production. I think that's safe in anybody's book.

Okay, I'm going to try to move through this. This is stuff that most of you have seen at least twice

before. Basically, I try to incorporate this kind of data in probability models in a -- using a series of steps. In the first step, basically these boxes, excuse me, these boxes represent maps and in the first step I tried to map a probability distribution based on a non-homogenous model.

There are several different kinds of models that can be used to do this. Near neighbor models, colonel models like the Knott Epanechnikov Kernel, is basically estimating some sort of a special recurrence rate based on where volcanism has occurred in the past, and perhaps when that volcanism occurred. It could be a spaciotemporal model.

I want to fold into that dataset an estimate of how structure, for example, faults revealed by the gravity data or map faults may influence magna ascent and hence the distribution of volcanoes. So I'm going to try to fold some structure into that somehow.

Third, you can look at an estimated dike geometries that can intersect the repository, dike links along which vents may occur. So, for example, you know, what's the distribution of buds along the dike or something like that.

Finally, we need to fold in an estimate of the regional recurrence rate of volcanism. So for the

volcanic field, if you need to find isotopically or whatever, what's the regional recurrence rate? And these things give you some estimate of the hazard.

Okay, I'll just do this fairly quickly. This is an example of a non-homogenous probability model superimposed for the Yucca Mountain area. This particular model is a near neighbor model. It's based on eight near neighbor volcanoes. Blue here is where cyan is relatively low probabilities, increasing to red to relatively high probabilities.

What I've done is I've normalized this map so that the probability of an event in this map region is equal to one. Okay? So if a volcano occurs, where will it occur in the Yucca Mountain area, for example. And this is a result based on the timing and distribution of past volcanic events. Certainly in Crater Flat you get the highest probabilities. Out here, this border between green and yellow is about 1×10^{-5} per square kilometer. That's the probability of volcanism in a given location given a volcanic event in the area. And, of course, it drops off with this from Crater Flat.

So, this kind of captures a sort of, think of it as a big filter. It captures some aspects of volcano distribution. But, this is a model that I would guess we first proposed in 1993, it doesn't capture some things

that may be important in volcano distribution. For example, Bear Mountain here is an uplifted paleozoic block. Many people think that the probability of volcanism at Bear Mountain, because of its structure and possibly it's elevation, some people would speculate would prevent volcanism from occurring there. Volcanism probability is high there, say as high there as it is over here or over here.

So, you know, there are things that you could speculate about that may change that distribution. This is just a straight model.

MR. MARSH: What's the background values? Just in the countryside, numbers?

MR. CONNOR: Out here it drops off to about 10^{-7}.

MR. MARSH: So that's a baseline.

MR. CONNOR: Yes.

MR. MARSH: So you have worked from there up?

MR. CONNOR: Yes. Given an event in this region, not folding in anything with a recurrence rate here. Yes, the highest values are about 10 -- a little higher than 10^{-4} in this region. So we are getting orders of magnitude variation across the plot.

MR. MARSH: Over what period of time would that be?

MR. CONNOR: Well, remember this is just if a volcanic event occurs.

MR. MARSH: Given an event, okay.

MR. CONNOR: Given an event, this is --

MR. MARSH: Spatial?

MR. CONNOR: This is only spatial. Okay. Now, as a way of addressing this possible structural control on volcanism, what I did was I actually folded together the horizontal gravity gradient data with the probability map. So, and there are various ways to do this. I'm not going to go through it in a lot of detail.

But essentially what we've done here is said that if I normalize the horizontal -- amplitude of the horizontal gravity gradient as a probability density function, then I can multiply that together with the near neighbor map, renormalize that and look at the resulting PDF.

The idea there is that as we saw earlier, the horizontal gravity gradient was giving us amplitude of the horizonal gravity gradient and it was giving us a good indication of where prominent structural features are based on volcano distribution. Sometimes we think -- or at least often, it looks like volcanoes correlate with the locations of those faults. So fold those two datasets together and see how that changes the probability map.

Well, one thing it does it is makes it a little bit more complicated, of course, areas of higher/lower probability. I wouldn't pay too much attention to the noise out here. But what it really does is it elongates this anomaly in a west/northwest direction parallel to the Bear Mountain fault. Okay?

So, if you think that Bear Mountain Fault is a major feature bounding the west edge of the grabin and you think that volcanism is related to extension of this grabin, then it seems like -- it seems reasonable that this kind of influence would take place. So the whole probability surface is extended in a west/northwest -- northwest direction. And to a certain extent mimics the extent of the Crater Flat Volcanic Zone proposed by Krone-Perry.

Okay, there is --

MEMBER HINZE: Are you saturating -- excuse me, but are you saturating your maximum values? It appears -- if you look at the gradient map, it appears that the horizontal gradients are much more intense along the Bear Mountain Fault, the southern portion, and then --

MR. CONNOR: In the center of the map. That's right --

MEMBER HINZE: Well, and also along the eastern boundary of that anomalous area. So, why do they

show up as the same?

MR. CONNOR: Well, I haven't changed the scale here. So this boundary between yellow and green is still 1 x 10^{-5} per square kilometers. Is that what you are asking?

MEMBER HINZE: Well, it appears that what I'm concerned about is that you may have saturated the coloring on this, have a low cut off so that you pick up the eastern margin of that and also the intense values in the center of Crater Flat where you don't have a high horizontal gradient.

MR. CONNOR: Well that's true. But the -- actually if I draw a profile across here, the high values are still in the center of Crater Flat.

MEMBER HINZE: Okay.

MR. CONNOR: Okay? There is no doubt about that and actually, now that you ask that question I realized I planned to bring that profile, but I didn't. But I -- the highest values remain in the center of Bear Flat. In fact, what we've done is looked at horizontal slices all along this thing and basically this axis is the highest probability zone, decreasing as you go northwest, okay?

The contour plot for this is kind of, you know, gets a little messy out in here where people tend to

focus. So I, you know, think this is a reasonable way to show it.

MEMBER HORNBERGER: How does your weighting factor affect this?

MR. CONNOR: Yes, let me -- I'll address that point in two slides.

MR. MARSH: Is this normalized to one?

MR. CONNOR: This is normalized to one, exactly.

MR. MARSH: It looks like -- the proportion of rate area is very large, compared to the last one.

MR. CONNOR: It's normalized the same way.

MR. MARSH: So the value changed, basically. The background value, I guess, is lower.

MR. CONNOR: That's a good question. I'm not positive. But I don't think it's changed significantly. I think the proportion actually is about the same. So we look at that.

MR. RYAN: Chuck -- seemingly, depending on the combination of the radial distance from the Crater Flats area and the proximity of gravity gradients to that area, the yellow regions essentially leak off, if you will, from this red dominated area, despite the fact that they might actually be normal to the map fault system on the surface. Just as an example.

MR. CONNOR: Yes.

MR. RYAN: This is not necessarily a criticism, it's simply an observation.

MR. CONNOR: Okay, right.

MR. RYAN: Okay, could you comment on your selection of the radius of influence that -- or your R factor --

MR. CONNOR: Sure.

MR. RYAN: -- basically your area of Crater Flats in relation to local structure?

MR. CONNOR: Yes. The -- well, there are several levels to your question. First is in a near neighbor model you choose the number of near neighbors you use to make your estimate. I tend to use models with relatively large number of near neighbors, eight, you know, which is a relatively high proportion of volcanoes in the field, to smooth thing out. Because I feel like the few number of events would even, particularly with this few number of events, we should look at a relatively smooth picture.

And something like an Epanechnikov model you actually have the smoothing distance parameter, it's been called, I think amply during the PVHA study. And that is a very important influence because if you choose a very short smoothing parameter like --

MR. RYAN: So the radius itself is coming into the rating factor?

MR. CONNOR: Right, right. Then if you use a small smoothing parameter, then you lump your probability next to where volcanoes have occurred in the past, of course, very tightly. If you use a large smoothing parameter, you smooth that out and spread that probably density function out over a bigger area.

Now, the -- I can think of a -- I did some cluster analysis work to try to sort out, get an idea of what kind of cluster radius we are dealing with and that came out in the Yucca Mountain area to be on the order of 16 to 20 kilometers, something like that. So, I prefer using models with smoothing factors of about that scale. Other people use tighter smoothing models. Tighter smoothing parameters are used in PVHA.

And, finally, how does that compare with the density of faults, for example? The fault density tends to, you know, where there mat at Yucca Mountain and not an alluvian, fault density is quite high compared to those kind of smoothing parameters. You know, so over hundreds of meters of fault spacing.

Okay, this just -- I'm just going to show this to show that there are a variety of ways to fold these data sets together using different assumptions about the

structure. This is the set of maps I just showed you where I took the near neighbor model and multiplied that together with the horizontal gravity gradient. Here, giving these two maps equal weight to produce this probability map.

On the other hand, you can do things like try to use fault density. And you know, it's tough using fault density because, obviously the map fault density changes depending on how much alluvial cover is present. If you assume that the density of faults in Crater Flat is the same as it is as Yucca Mountain, just make that assumption, then I count through the density of faults this way. And actually folded in some additional information. I was really concerned with the distribution of high dilation tendency faults, that is faults that have -- or the proportion of high dilation tendency faults. That's faults with a north or northeast trend through the area.

So that tends to shut off areas like uplifted paleozoic fault blocks or the Timber Mountain caldern in terms of fault density. So even though there are a large number of faults there, they also have very little to do with current stress data.

And so if you fold that together you get a probability surface like that and what I point out is that

these are comparable in shape and so using different assumptions about the structure I've come up with a relatively similar looking model. Again, it indicates this north/northwest trend is important and that the probability is highest in Crater Flat.

Okay, so if we get specific to the repository --

MR. MARSH: What other kinds of -- have you done this using other fields? For example, topographic stress from just topographic that --

MR. CONNOR: Yes, I did it with topographic data. I'm not a big believer in topographic control on cinder cone distribution. But I went ahead and did it anyway.

If you make a PDF from the topographic field and you use the elevation of the crest of the mountain, then the probability of disruption after a repository changes very little. It goes down by, you know, from -- I can't remember the exact number, but from say, 2×10^{-8}, or 1×10^{-8} or something like that. You know it drops by a factor of two or less.

If you use the elevation of the repository itself, rather than the crest of the mountain, it tends to go up a little bit. But again a trivial amount.

MR. MARSH: And when you do your fault

probability, do you, because you have alluvium over a lot of things, but you know the bedrock basically what's there from the structure models.

MR. CONNOR: Right.

MR. MARSH: Can't you propagate through normal fault densities that you would see in various kinds of bedrocks over various ages, because you know the structural history, and have a, you know, have a map for the whole area. Is that what you've done here?

MR. CONNOR: I haven't quite done -- gone quite to that extent. What I've done is I've looked at the distribution of alluvium using the GIS covers from fizzel & Shulterz where there is dense alluvium, and of course there is a few faults. What we've done is we've taken a look at increased -- or changed fault density in those alluvial cover areas from basically the distribution of map faults, in which there is no faults in alluvium, to the density of faults is about twice that of the maximum density of faults on Yucca Mountain.

So rather than make the assumption I've just explored, the effect of that change in the parameter on my bottom line, which is the probability.

MR. MARSH: But you have put something in for Crater Flats, I guess, or --

MR. CONNOR: Sure.

MR. MARSH: And so you weighted it, I guess, to make the worst case. Probably assuming there is a certain fault density there.

MR. CONNOR: Well, as you increase the fault density in Crater Flat, that tends, and it is in a structure model, that tends to decrease the probability at Yucca Mountain.

MR. MARSH: Right.

MR. CONNOR: So, yes, that's right. And we've, in this particular model, I've assumed the fault density in Crater Flat is the same as the maximum density of faults in one square kilometer area on Yucca Mountain.

MR. MARSH: And you've normalized all these to one again?

MR. CONNOR: Yes, it is all normalized to one.

MR. MARSH: So, once you know a regional probability, you have to plug this into a regional probability to actually get absolute probability, right?

MR. CONNOR: Yes, yes. Which is what I've done in the next couple of slides. Here, for example, is a range of probability of intersection at the repository for a regional recurrence rate equal to one that just means that if the volcano occurs, it has a probability of intersection with the repository.

What I've done here is I assumed a uniform

distribution of dike half link from 100 meters to 2,000 meters. Okay? Assuming that if an event occurs that close to the repository, then that's a disruptive event. So that's going to be important when we start comparing apples and oranges between this and PVHA. So please keep that in mind.

And here is the kind of curve that develops for a number of different near neighbor models or kernel models in different weights assigned to fault density. Okay? So if you weight structure data quite heavily, you are out here on the graph. If you give structures no data, you are back to the straight non-homogenous Poisson model, you are back on this axis.

And you see that all these curves go through a maximum between, you know, if you assign a little weight on the order of one up to a factor of two, for these structural models, you tend to go through a maximum. And that's the kind of range and probability we get with, it shows these curves to represent the range in probabilities we get for intersection given a volcanic event in the region for these different models.

So I want to point out that at this point I don't think there is a very good geologic basis for distinguishing between a lot of these models. And that's one reason we are trying to do this kind of a bounding

analysis.

Another set of curves, not using map structures but rather using the horizontal gravity gradient data, you get a very similar kind of distribution. And then finally if we just cast that as a probability, this is given a regional recurrence rate varying between two volcanoes per million years and ten volcanoes per million years. Okay?

So that's -- some of the uncertainty in here of course is what the regional recurrence rate of volcanism is going to be. And you can see this is the kind of range we are talking about here. None of these models gave me values of less than 1×10^{-8} annually. And I would say that you only get into the upper part of this curve, up above 1×10^{-7} annually with some pretty strong assumptions, worst case models. So, I tend to call the upper limit 1×10^{-7} where you can get up to 2×10^{-7} in the worst case models.

Okay, so I just want to summarize a little bit with some conclusions. I guess the language in the first conclusion is probably a little vague for you, Paul, but, you know, this is the best I can do right now, is that plio-quaternay volcanoes near Yucca Mountain correlate well with high dilation tendency faults, and regionally with vertical displacements in basement indicated by the

gravity data.

What does that mean? It means that some of the volcanoes lie along these high gravity gradient areas. We can find faults going through some volcanoes. Certainly not always the case, as far as I can tell.

Second, when include structural control in volcanic hazard models, probability tends to increase. And that increase is by about a factor of two or so. Rarely more than that. So, for example, if we follow one of these curves, this is not including structural control. As I give increasing weight to structural control, the probability goes up a bit.

I don't see order of magnitude changes in probability by including or excluding structure. Okay? Things change --

MR. MARSH: Why is there a maximum in that? Why does it come down so fast?

MR. CONNOR: Well, actually it doesn't come down as fast as it goes up. But what happens is, let's take a look at the -- if I can find the slide, let's take a look at the horizontal gravity gradient data. For example, when I give structure a lot of weight compared to volcanism, then stuff, for instance way over here, is going to start, you know -- if I only included structure and not the past distribution of events, this fault would

have a lot of weight compared to this fault.

MR. MARSH: So you keep taking it away from --

MR. CONNOR: So I take it away from the Yucca Mountain area. Right.

MR. MARSH: Concentrated in the --

MR. CONNOR: Yes, and it just happens to go through a maximum which is convenient --

MR. MARSH: But you have to net sum to one, so it --

MR. CONNOR: Right, right. Okay. And these models indicate a range of probability in volcanic disruption from about 1×10^{-7} to 1×10^{-8}. And again, if you assume very low regional occurrence rates, if you assume that the recurrence rate of volcanism is one event in a million years which is lower than it has been in a region, or if you assume very high recurrence rates of volcanism, you know, you can get above that value.

Based on these models, this is the range we've come up with. There are some differences in how events are defined in this case compared to PVHA, so it's important to keep those clear in your mind. But, I guess I would say there is overlap in the mean are near overlap in the mean value with this range.

And these models are a little low compared to those values proposed by the State -- by Ho and Smith.

MR. MARSH: But yet this doesn't take into account the probability of volcanism occurring in this area of Nevada or the western United States?

MR. CONNOR: Well this does, this final value does because I fold it in as a last step that regional recurrence rate. Right so --

MR. MARSH: So how big is the regional rate? I mean, when you say regional, what is that?

MR. CONNOR: Well, if we go back to --

MR. MARSH: Oh, you can just tell me.

MR. CONNOR: Okay. Well, I can't just tell you. It's a little tricky because if -- what I'm doing here is looking at volcanoes, the aeromagnetic boundaries on the south, sort of the southbound of the map, going up north to about Buckboard Mesa, going from the Bear Mountain Fault over to Yucca Mountain. I'm taking that slice and I'm saying well, if the volcanoes I see now represent the amount of pliocene and quatery volcanism in this region, then there are, say, 30 volcanic events in that area over the last five million years.

MR. MARSH: But you compare that to the whole state, this area that you chose right there, for example, is it higher or lower for example, than a volcanic --

MR. CONNOR: Well, sir, if you choose a very large area, what you are saying -- this is kind of like a

homogenous model. I'll choose my area and get the recurrence rate in this large area. If you choose a large area then you, of course your recurrence rate goes down.

MR. MARSH: Well, if you choose the whole earth.

MR. CONNOR: Right.

MR. MARSH: But if you actually choose, you know, if you choose an area where we've had --

MR. CONNOR: Well --

MR. MARSH: -- cinder cones through basin range or an area of province, I'm just interested in how the base level is set, actually.

MR. CONNOR: Well, if you look, this is a calculation that I did a while back. But if you [illegible] at the area as a whole, I come up with about 10^{-10} per year as sort of a generic number that if you just wanted to throw a dart at the Western Grade Basin, that's the probability of volcanism over about a eight square kilometer area, which at the time I was using for the footprint or repository.

So these are on the order of, you know, two -- maybe two orders of magnitude higher than those kinds of numbers. And if you, you know, look at a map like Lutken-Smith, for example, you know, the locus of volcanism in the quarternery has occurred in very specific areas in the

Western Great Basin and it's had a high tendency to occur where pliocene volcanism occurred.

MR. MARSH: So if you add that into it, what happens then?

MR. CONNOR: Well, with a non-homogenous model, and a nice advantage of a non-homogeneous model is I think I've gotten away from that edge problem which is an issue you are dancing around. So that's one reason to go with a non-homogenous model.

This model is not going to change as I go to bigger and bigger areas. The map might be, of course, become more complicated on the edges. But at Yucca Mountain the probability surface won't change a lot.

MR. RYAN: You mentioned that, when you were discussing the graph on page 17, that you had selected dike half lengths of 100 meters to 2 kilometers. Did I hear you correctly?

MR. CONNOR: That's right.

MR. RYAN: Can you talk about that? Can you explain why you selected those numbers?

MR. CONNOR: Yes, I was looking, I basically used, I was interested in disruptive events. So, I used a value -- uniform random distribution sampling in there. And it actually, if I cut the half length down to 1,000, it decreases my values by about a factor of two. Okay?

What I was concerned about was the area likely disruptive, disrupted and with the potential for extrusive volcanism as a result. And if you look at, for instance, eruptions of Perikoten volcano, Horruio volcano, Tolbachik volcano, modern cinder cone eruptions, extrusive vents associates with those vents are distributed up to several kilometers away from the main vent. Horruio is a good example where an alignment of something like seven cones formed over a period of 15 years extending about five kilometers.

MR. RYAN: So you, if, am I correct in thinking then that you have used the surface expression basically in analogue settings --

MR. CONNOR: Right.

MR. RYAN: -- as a guidance and rule of thumb for selecting dike dimension of depth.

MR. CONNOR: And I'm not particularly enamored with this approach. I think it's a good -- you know, there is merit in it. As I mentioned earlier, I was recently in the San Rafael Field where Paul Delaney and Ann Gardner have done a lot of mapping. And there the buds expend over comparable or probably slightly smaller distances. And you know, you have these beautiful intrusions eroding out of the Entrada sandstone, so it's very obvious what these geometries look like.

I'd like to go back and do some of these calculations using those kinds of geometries. And, again, I think that what that does it is makes the models robust but it's likely to change the final answer by a factor or two or less, using that kind of thing.

MR. RYAN: Do you think that these analogue dike dimensions that you have used have, one can translate them down in the third dimension some considerable distance?

MR. CONNOR: No. These might correlate with dike segments, for example, rather than a large dike. And let me give you an example. During the 1975 Tolbachik eruption, there was eventually an eruption about nine kilometers from the first breakout, they call it the northern and the southern breakout. And you know, that indicates to me some dilation and intrusion. Possibly a master dike. But those would be separate events. If I used a dike segment model where I had, you know, a four kilometer upper limit.

MR. RYAN: The other think I was wondering about is with respect to the integrated volcano structure probability model on page 15, particularly plots C and E. Plot C you derive by looking at the gravity gradient dataset as input.

MR. CONNOR: Right.

MR. RYAN: Plot E you derive by looking at mapped fault distributions as input. There seems to be an apparent rotation of the major axis of that red high probability region clockwise somewhat, maybe five, eight, ten degrees, possibly pushing it. Is that rotation apparent or real?

MR. CONNOR: I think that I would prefer not to push these models that hard.

MR. RYAN: Because the gradient that seems to be influencing Plot C has a north/northwesterly trend, but the mapped structures that seem to be influencing Plot E have a distinct north by northeast or north/south trend, if I'm correct in recollecting.

MR. TRAPP: Chuck, just a point. I don't know if you've seen the literature on some of the rotational values that they have mapped out at Crater Flat, but there is going from northern part of Crater Flat to the southern part, a very definite clockwork rotation in the structural field.

MR. RYAN: In what sense?

MR. TRAPP: Clockwise.

MR. RYAN: Yes, okay.

MR. CONNOR: Yes, in terms of the probability map, I wouldn't want anybody to try to extract that kind of detail. But in terms of the basis for what emerges,

you know, from what goes in comes out, that not using it as part of a hazard analysis. And after this consideration I haven't really looked at it in that respect. That's a nice idea.

MR. MARSH: Are there any signs of dikes and sills of similar age in the mountains around?

MR. CONNOR: No.

MR. MARSH: No dikes and sills in the mountains?

MR. CONNOR: Not that I know of. If you go over to Nye Canyon and Pahute Mesa and Piaut Ridge, there pliocene dikes and sill complexes, but not at Yucca Mountain. For example, there is the 10.5 million year old Solitario Canyon dike which is basically half a kilometer, the southern end is about half a kilometer from the repository. But's 10.5 million years old, not pliocene.

MR. MARSH: But, what did you say, Nye Canyon or something?

MR. CONNOR: Yes.

MR. MARSH: The same age?

MR. CONNOR: No, actually I think those are six -- six or seven, I'm sorry.

MR. MARSH: Okay, so there is no tendency that style of emplacement of sills, for example, at this age of one, two million, in the area.

MR. CONNOR: That we can see.

MR. MARSH: Right. But that's significant, also, the fact that you don't see dikes, transport dikes in the mountains around, the structural highs. That may be a feature to put in, a small tweaking feature to put in.

MR. HILL: We don't see the -- what you are saying the transport features at Yucca Mountain. But certainly up in the Sleeping Butte complex the igneous activities located on the topographic high in that region and very clearly controlled by structure based on the work of Scott Minor and his colleagues survey.

MR. MARSH: But there is still no sills anywhere?

MR. HILL: Not exposed, but we are still seeing the four and a half million year old original flow surfaces preserved up there. A very low erosion rates and uplift rates relatives to coming down 100 meters below the surface.

MR. MARSH: Why I ask about sills is that it tells you something about the, perhaps, temporal storage, intermittent storage on the way to the surface. Sills are very, very common in complexes that are erupting in intermittent depth. They are very easy to place, especially in trains like this where you go from bedrock

into an alluvial region and they are starting to lose their hydrostatics. It's very, very common all the way through thousands of kilometers in Antarctica and all over.

MEMBER HINZE: John, you had a question?

MEMBER GARRICK: Yes, one of the issues that's always involved in probabilistic calculations is the issue of the end states and a well set of defined end states. Are these results that you presented in view graph number 19 associated with a specific definition of disruption, or a specific definition of an end state? And is that translated into a result in terms of consequence?

MR. CONNOR: Well, first I haven't presented anything here taking this to consequence. This is just hazard.

MEMBER GARRICK: Well you do talk about disruption. So you --

MR. CONNOR: Well, right.

MEMBER GARRICK: So you must have some sort of a definition in mind as to what constitutes the reference --

MR. CONNOR: Right.

MEMBER GARRICK: -- for disruption.

MR. CONNOR: Right. So this would be an igneous intrusion which has a center, an orientation and a

length to it. And that length is -- that half length is 500 or excuse me, 100 to 2,000 meters. So, and that results in disruption of the repository. Is that --

MEMBER GARRICK: Well, yes --

MR. CONNOR: Well, now the answers here change depending on your definition of those events.

MR. GARRICK: That's right.

MR. CONNOR: And PVHA, the different experts, I think it's fair to say, use different definitions of events. As I mentioned before, I'm not wed to any particular definition of the event. I tried to develop this one based on analogy where volcanologists have seen these things form, you know, what the areas disrupted look like.

But, it's an important issue to explore and again, we've explored it to a certain extent. We've varied the length quite a bit and so forth. But, you know, geologic analogy I think is the only way to go on that. Right?

MEMBER GARRICK: Okay, well, probably be more on this later.

MEMBER HINZE: Paul?

CHAIRMAN POMEROY: Chuck, we are going to be comparing, in a few minutes, I hope, the apples and oranges that you referred to earlier. And I just wanted

to clarify, for the record essentially, this model that you presented is one of several. But was this model presented to the PVHA experts? And was it available to them for their consideration?

MR. CONNOR: Not entirely. This model was presented to the PVHA experts and at that time we hand folded this other information in. But I did present to them, I guess in two talks, some information about the fault dilation tendency, how I thought that fault orientation and dilation tendency might affect the probability of distribution in a qualitative way.

So, these maps were never presented. Numerous variations of this one were, and again, this idea of, or a concept on how to fold instructural control was.

CHAIRMAN POMEROY: Okay, thank you.

MEMBER HINZE: Chuck, there are many conclusions I think we can reach from your presentation, your excellent presentation. One of the conclusions that I would draw is based upon the second bullet to your nineteenth transparency, you've got instructive control in a factor of only two. I would conclude that the issue related to the PVHA raised by John Trapp concerned with the geological bases of zones can be certainly modified or eliminated.

MR. CONNOR: Actually I disagree with that

because what I found is that if I take my -- this conclusion refers to comparison of a non-homogenous model with a non-homogenous model with structure folded in. Many of the PVHA models, and Kevin can probably put a number on this, put a barrier, or a zone definition between Crater Flat and the repository. And what that does is it essentially creates a step function or some variation on the step function of high probability on the west and lower probability on the east based on the occurrence of a structure, or a structural zone of some sort.

I don't see any evidence in the structural data that I've got or the kinds of processing I did that you can decrease probability at the repository site based on structural control.

So what I'm saying here as it increases it, it increases it frankly by a fairly trivial amount.

MEMBER HINZE: That's what I'm saying. That it increases it certainly, but not by an amount that is --

MR. CONNOR: In comparison to the near neighbor models. Now the event center, if you just look at the event center, I believe that, at least in, I guess it was the third PVHA meeting a number of models were presented -- a number of histograms were presented. The probability of an event center at the repository was zero

in many of those models. Is that correct?

PARTICIPANT: No.

MR. CONNOR: Okay, no, what was it?

MR. COPPERSMITH: (Off mike. Inaudible.)

CHAIRMAN POMEROY: I'm sorry, but you can't do this without getting to a microphone. Kevin can --

MR. CONNOR: Well, Kevin will come up, but the point is that many of the models have a step function across a zonal boundary. And I'm not particularly satisfied with the idea or use of structural zones with step functions across them in a probability surface. You know, probably surfaces, you've got to be able to differentiate them, I think. You've got to be able to -- you have to be very careful about how you use that.

Certainly in this case, in my view, there is no geologic evidence for such a zone boundary, even if you wanted to use one. And hence, you know, structure does not decrease probability.

MR. TRAPP: There is a very important point and I disagree with you very much, Dr. Hinze. Because if you take a look at the models that we are dealing with, what we are trying to do is concentrate on those models in which we are talking about an extrusive component. If you take a look at -- and I'm going ahead to the first slide that Kevin will be talking about, you notice that his

models and the probability he is talking about have got both the intrusive and extrusive component.

Therefore, an awful lot of his models basically end up with a probability -- and it's really just a probability, of a dike, not an extrusion. Therefore, you are talking a tremendous difference in possible consequence when you take a look at the actual numbers that you are dealing with. I've stated, as close as I can figure, from the PVHA report, the comparable number would be about 6×10^{-9} for their mean value for the model which would compare to ours.

MEMBER HINZE: Okay, then another conclusion which obviously I'm going to be incorrect about is that adding another dozen hits to the aeromagnetic study or to the ground magnetic study in Amargosa Valley or southern Crater Flat is not going to have any effect, any significant effect upon the probability.

Is that --

MR. CONNOR: I really don't want to speculate about that, but of course if you add --

MEMBER HINZE: Have you run sensitivity studies to determine how robust this is?

MR. CONNOR: By adding 10 or 20 suspected events? No, I haven't done that yet.

MR. HILL: That's something we're planning on

doing fairly shortly to come up with a reasonable location, evaluate possible age scenarios and look at the impact of those on existing probability models.

MR. CONNOR: I would -- I hate to speculate about data that hasn't been collected, but in the interest of moving forward, what I would say is that the probability will, of course, increase if you add events to the system and the probability at the repository site will increase.

Of course, the distribution of those events is quite critical to answer your question.

MR. MARSH: One question. In that context, is that if you have more from the aeromag or the ground mag, if you have more areas that are possibly eruptive centers and you have your probability normalized to one in this area, wouldn't you draw the probabilities over to that area and heighten it and reduce it with the repository or do you increase the probability just for this geographic region for the state as a whole? That way it's a little bit artificial because they're not doing a ground survey for the whole state also or the whole region. How do you actually handle that?

MR. CONNOR: Well, the nonmodulous models, I think, are advantageous because they don't require that you define some region.

MR. MARSH: Right.

MR. CONNOR: And that's nice. So if I find some -- I mean obviously the regional occurrence rate may increase, if you find more events, right, because our estimate of the regional occurrence rate is based on past events, so in that sense, of course, it will increase.

And of course, there might be some change in the structure of the probability surface itself based on past events.

MR. MARSH: If you find more in one place in some area in your math, does it reduce the probability of the repository or not?

MR. CONNOR: Well, it would reduce the probability in some areas of the math and increase it in others, definitely. What it does at the repository I don't know.

MR. MARSH: Well, if they're far enough away.

MR. CONNOR: Yes.

MEMBER HINZE: Well, we thank you very much, Chuck, and we'll take a modicum of blame for wrecking the schedule of your presentation.

MR. CONNOR: Well, thanks, Bill.

MEMBER HINZE: It was extremely interesting. Well, let's see, we're now at 10:15 and Tim Sullivan and Kevin are up to discuss the overview and status of DOE

activities and summary of probabilistic volcanic hazard assessment.

Tim? We appreciate you and your colleagues joining us today. We really will find it extremely interesting and useful.

MR. SULLIVAN: So good morning. This is two-part presentation shared by Kevin and myself. Based on what we've seen in the last couple of hours, I frankly don't expect we're going to catch up on any schedule.

(Laughter.)

CHAIRMAN POMEROY: We'll help you with that.

(Laughter.)

MR. SULLIVAN: I'm going to give you an introduction and some background and the status of the volcanism program at DOE.

I'm a geologist with the Department and have been the lead for our vulcanism program for the last couple of years.

You should have copies of the handouts. The volcanism status report was issued in 1995. This was a document produced by the project, by Los Alamos, that provided the most comprehensive synthesis of data available at that time.

I noticed it's not in your workbooks here, but I know the NRC staff has that and I'm sure the ACNW has

that as well.

It turns out that was the first of several synthesis or summary reports that the Department has produced. The LBL part of the M & O has produced the geophysics synthesis report which was referred to by Chuck and John and I believe you have that as well. And the USGS has produced the seismotectonic framework report which is a comprehensive discussion of our tectonics program and seismic hazards program.

An update to that report is planned for later this year which will incorporate additional data analysis which was completed in 1995 and 1996.

And then finally, at the end of 1997, a part of a larger document called the Project Integrated Safety Assessment, the PISA, Chapter 2 called the site description will be completed and this will provide an integrated discussion here of tectonics, seismic hazard, regional and site geology, geophysics and igneous activity. This should be available to you early in 1998.

MEMBER HINZE: Did you say when the vulcanism report --

MR. SULLIVAN: End of the fiscal year.

MEMBER HINZE: End of the fiscal year.

MR. SULLIVAN: Right. DOE decided early in 1996 to close out site characterization data collection

for the volcanism or igneous activity program.

This was based on several factors. The low hazard probability results from DOE's earlier work and from the PVHA which you'll see in a moment.

In our view, the PVHA results are insensitive to new data collection and we'll support that view later today.

DOE performance assessment results, which will also be reviewed in a later presentation, have consistently shown that system performance or risk is not sensitive to volcanism. In fact, in our view, volcanism is not a key technical issue for performance at Yucca Mountain.

MEMBER HINZE: So there.

(Laughter.)

MR. SULLIVAN: The Department recognizes its responsibility to evaluate new information and to conduct additional analyses as appropriate to insure that we maintain a sound, defensible position in the license application in 2002.

Additional consequence analyses are planned as a part of an upcoming total system performance assessment and these will be discussed in a later presentation.

The main focus of our presentation today will be on the probabilistic volcanic hazard analysis. This

was an expert elicitation that involved ten subject matter experts in volcanism that was conducted over approximately a one year period.

The experts formulated their judgments based on information provided by DOE, provided by the Center, provided by the State, provided by the USGC and based on their own experience both in the U.S. and world-wide.

The final report was completed in June of 1996 and I presume you all have that as well.

Following that, in the fall of last year, we held an informal meeting with the NRC to discuss the expert elicitation process that was used in PVHA and then in February we had a two-day technical exchange to discuss the PVHA results and we're going to discuss those again here this morning. They're reproduced at the bottom there and Kevin will provide a full description of those results.

At the technical exchange conducted in February, the NRC and the Center presented data and analyses that were conducted after completion of the PVHA. DOE agreed to evaluate this new data through hazard sensitivity studies, specifically we have evaluated the significance of the increased number of events associated with Anomaly A and we have evaluated the significance of the revised volume estimate for Little Cone through hazard

sensitivity studies. Kevin will present those results here shortly.

At the technical exchange and again here this morning, Chuck presented the basis for the NRC's conclusion that the probability of future volcanic events ranges between 10 to the minus 8 and 10 to the minus 7. As Chuck and John have acknowledged, the information that they have used to formulate these models was also available to the experts for their use in determining appropriate source zones at Yucca Mountain. This range differs from the PVHA results, but is included within the bounds of the full probability distribution.

It remains DOE's position that the PVHA results, the meaning and the uncertainty distribution provided defensible basis for characterizing the disruption probability.

At the technical exchange, DOE committed to providing a description to the NRC of how we will use the results of the PVHA in performance assessment and Abe will provide that for you early or later this afternoon.

This should support closure of the hazard probability subissue.

Finally, as I think we'll hear again later this morning, at the technical exchange, the NRC and the Center presented dose calculations for volcanic eruption

through the repository with associated tephra and radionuclide dispersion.

The risk result using the hazard probability, the upper bound hazard probability of 10 to the minus 7 you heard described earlier, not DOE"s mean value of 1.5 times 10 to the minus 8 indicated an average annual dose of a half a millirem per year.

We view this as a low result. We have also concluded that the risk of volcanism is not significant. It appears that DOE and NRC results are converging which should lead to closure of consequence subissues.

Kevin Coppersmith, under contract to the Department of Energy has been the manager and facilitator for the PVHA process. As I said earlier, this extended over a year over most of calendar year 1995. He'll describe both the process and the results of the PVHA hazard probability results for you now.

MEMBER HINZE: Thank you, Tim. I think we're missing at least I'm missing the last several slides that you had and I'm sure the Committee and the consultants would be interested in having a copy of those.

(Pause.)

May I ask a question in terms of the clarification. Your second overhead or your third overhead concluding the cover, the reasons for closing out

the same characterization, in no way mentions consequences. Is this strictly because zero times any number is zero or why --

MR. SULLIVAN: In part, we have also closed out the consequence analysis work. WE feel we have an adequate basis there now to perform the consequence analyses needed for TSPAV.

MEMBER HINZE: Are there any documents which present the results of your consequence studies?

MR. SULLIVAN: Not in terms of dose calculations, but there is consequence information in the synthesis report that you don't have.

MEMBER HINZE: We will be getting that?

MR. SULLIVAN: Right, the close out of Valentine's work is reported there.

MEMBER HINZE: I see. Is -- could you summarize his work for us since we are not going to see that for six months?

MR. SULLIVAN: I can't do that here, but I could do that this afternoon.

MEMBER HINZE: Okay, great. Let's -- further questions?

Okay, Kevin? That's Coppersmith with a C and not a K. It's spelled phonetically on there.

MR. COPPERSMITH: Thank you. I appreciate the

opportunity to be here. I do not intend, I always bring a large stack of vu-graph. I don't intend to go through all of those. In fact, this is very difficult to do. As Tim said, I'm going to give a very brief overview of a large multi-expert elicitation, involved, occurred over a period of a year, involved multiple workshops, field trips, a lot of interaction and discussion. The report gives an indication of the level of detail of the study. All I can do at this point is to give a basic overview of the process and some of the conclusions and sensitivities. And that's pretty much it. I'd be happy to answer questions.

Detailed technical questions, I feel like I should have a panel of 10 experts up here answering the questions in terms of the details of their particular models. We have, in response to the questions in the past, the technical exchange, for example, try to highlight the interpretations that remain, for example, the issues of structural control and whether or not faults, fault density was considered and we will be happy to try to answer those types of questions today as well.

Maybe I should move to the other side. In terms of objectives of the study, again, defining some terms here to assess the probability of disruption by volcanic event of the proposed repository. Disruption is

defined as the physical intersection of magma with the potential repository volume. That can be viewed as the very tip end of an ascending dike as well as a hit, direct hit by the repository. Any type of intersection of a dike with the volume represented by the proposed repository.

A second key objective is the quantification of uncertainty. The basic concept of having a multi-expert study is to characterize and quantify uncertainties. So that's the focus not only in the individual evaluations made by the experts, but in the fact that we are using multiple experts to define the full probability distribution and as I'll talk about later on, that's the basis for a belief that the results are essentially robust to most types of new findings that could become available.

Looking at both eruptive and intrusive features, everything is couched in terms of the probability here is an annual frequency, annual frequency of intersection. This is as those who are involved in risk who are dealing with the probability of intersection which is a hazard probability, that will be multiplied times the consequences given an intersection of various types and that the product of those two will represent the risk associated with igneous activity.

At risk in the parlance of this Yucca Mountain

is the total system performance assessment. Some of the aspects of the PVHA study, I just want to emphasize that the quantification of uncertainty is a central focus. We're trying to capture all the important elements, including the expert to expert diversity of interpretation. We ask the experts themselves to note represent particular proponents of views, that the evaluators of alternative views and to apply weights to alternative models. They're exposed to all the pertinent data, to the research themselves and the methods for conducting hazard analysis that were available at that time. This is an opportunity, obviously to use all the Yucca Mountain data that have been gathered by the various groups as well as the their own experience bases and having done work in other volcanic fields throughout the world.

Ultimately though, the reliance that an extra place is on a particular data set, say a geophysical data set is a function of their own experience and their own prerogative to use the types of data that they feel are important.

They're encouraged to consider a full range of methods and data. Many technical experts are used to being proponents of particular views, particular models. I have been in that position myself and I'll give a talk

at American Geophysical Union about a particular model. I'll be a proponent of that model. I'll have published on that model. This is not the role that we ask these experts to take. In some cases, we ask them to discuss their published views. We're asking them to consider the uncertainty in their model versus other models and to apply weights to alternative models. So we're not looking for preferred estimates or bounding estimates or conservative estimates. We're asking them when they're in doubt, between two alternative models or parameter values, to not choose one or the other, but to weigh the alternatives.

Throughout a formal process of expert elicitation was followed. Only for those that are in this particular, interested in this particular area, I'll only show these as examples of the technical guidance for expert elicitation that were followed throughout the course of the study.

NRC has published guidance of branch technical position. The Department of Energy as well as combined efforts for guidance and recommendations on how expert elicitation studies should be carried out, the process followed the PVHAs is consistent with that guidance.

Members of the expert panel are shown here and I won't go into the experience and capabilities of these.

They're well acknowledged experts. I should point out that they include individuals who have a detailed knowledge of the Yucca Mountain data sets like Bruce Crowe. A majority of the Panel have not had, did not come to the Panel with an experience of Yucca Mountain. They gathered the information. It was disseminated. They spent a lot of time in workshops and field trips to come up to speed on the particular data sets. The advantage of having the mix of both is obviously the site specific experts are familiar with the local data, but the regional experts can come in with their experience data bases having worked in other parts of the world.

CHAIRMAN POMEROY: Were these selected from a larger pool, Kevin?

MR. COPPERSMITH: Yes.

CHAIRMAN POMEROY: And was the reason for their selection based on essentially the FAC principles or other principles that are laid down in some of that other guidance?

MR. COPPERSMITH: yes. It's not only, we have individual selection criteria, but you're also looking for a balance and disciplines of capabilities and institutions throughout the Panel. So those come into play.

I wanted to point out though that this is a very large study. I've been involved in many multi-expert

hazard studies. This is one of the larger studies I've been involved in and it involves not only a large panel, but also many individuals who participated as presenters in workshops or are actively involved in the field trip. This is an example of some of those who are involved in work shop presentations, can just go down through the list, people like Allin Cornell, experts in probability and uncertainty characterization; people from the Center, talking about the studies that they had done locally in the area or Paul Delaney, talking about dike geometries and distributions and so on. So a large number of people, at one point counted up on the order of about 75 individuals involved in one aspect or another of the study.

This type of expert elicitation process derives much of its power from interactions of experts. There are many opportunities for the experts to come together in workshops and make presentations as well as a couple of field trips, and an opportunity to see field relationships. To proceed through the process that's outlined in the branch technical position as well as other guidance on expert elicitation processes from discussions of the data sets, observations in the field of those data, dealing with alternative hazard models, volcanic hazards itself or probabilistic volcanic hazard analysis is not,

does not embrace or involve all the aspects of basaltic volcanism that are possible and certainly the range of expertise in this Panel goes well beyond what's needed to carry out a PVHA.

So it's an opportunity to talk about things like nonhomogeneous spatial models, nonhomogeneous temporal models and the data that are required to characterize those.

Alternative interpretations are obviously what we're after. We're looking for alternatives in interpretations of models that are consistent to various degrees with the available data. Individual elicitation interviews were held. The feedback workshop to look at the preliminary interpretations and the calculated results of those interpretations. We're trying to foster debate among the experts. We're trying to get them to examine the technical basis for their interpretations and there is a certain amount of technical challenge that we try to encourage throughout that process.

I'll just show a couple of generalized logic trees. The process that we're looking at for uncertainty characterization is essentially shown here. Alternative models or alternative parameter values for a particular aspect of the hazard model is shown diagrammatically with alternative branches in logic tree.

Now I'll just step through some of the assessments that the experts went through to give a feel for the types of evaluations that were made.

The first aspect to deal with the concept of a volcanic event, its length and azimuth. In this case, as Chuck mentioned, the experts themselves were allowed to identify or to define a volcanic event in any way that they wanted to. That ranged basically from an individual dike as represented by an individual cinder cone to a collection or a dike set that would have more substantial length to it. Usually event definitions took into account the age of the particular volcanic center as well as its geometry. Proximity, for example, of two or three cones that appear to be the same age that are say three kilometers apart. There will be some probability of that being a single event, as opposed to three distinctive events.

So the length distribution, I can show some examples of that, largely comes from dike geometry lengths as well as dike set geometries. Azimuth is driven to a large extent by the orientation of the maximal horizontal stresses in this area, almost all the experts agreeing that the predominant azimuth in the northeast quadrant.

The types of temporal models that are considered, the homogeneous types of models or

nonhomogeneous models, most of the experts preferred the homogenous type model over different type frames. In other words, events would be counted and used for a rate calculation over a particular time period, say the lat million years, the last 5 million years and so on. It would be assumed that that rate was homogeneous over these different time splices. But of course, that's start time, that time period of interest is uncertain and there would be various probabilities assigned to a particular time period.

There were, in the case of Rick Carlson, used a volume predictable nonhomogeneous time dependent model, taking into account the waning volcanism over the last -- over the post-10 million year time period. The time period of interest, I mentioned uncertain and given a particular weight across for each individual expert, the region of interest was that this is the overall area that would be considered to be the area of interest for the PVHA analysis. In general, the Amargosa Valley as a topic province, that Gene Yogodzinski will talk about in a minute was used as that region of interest. In other words, it's an area within which the volcanic hazard begins. It's this area that may be the overall geometry of the source at depth within the lithosphere, it may represent an area within which just simply a spatial

background rate of volcanism is applied. The question came up earlier, what is the rate density of volcanism for this area. In general, they would start with the region of interest and then segment areas within that that might have a lower or higher rates.

Spatial models. In general, there were several spatial models that were assumed. One was a zonation model where areas are identified and those areas identified have a rate density of a number of events per year that comes largely from the counts of the observed volcanic centers over various time periods.

The zonation boundaries are uncertain and that uncertainty was characterized either by alternative zone geometries or by a boundary that had essentially a fuzzy boundary that allowed for uncertainty in the location of the boundary.

Another approach, the spatial aspect, kernal smoothing, which is basically running the smoothing operator of th observed pattern of volcanic centers. These type of approaches that were advocated and discussed by Chuck Connor were used to a large degree by these experts with some changes. For example, they would have changes in the actual kernal itself whether or not they used Epanechnikov of some sort of Gaussian smoother. The smoothing distance over which the smoothing occurred was

uncertain. It could be variable, and other approaches which allowed for smoothing within particular source zones.

Another zonation or another approach which was used to get at the spatial aspect was advocated by Mike Sheridan, used by a few of the experts. He's gone around the world and looked at the pattern, the geometry of volcanic fields within basaltic volcanic fields and sees that in general the geometry of the field shape is parametric. It tends to follow by Gaussian distribution and that type of field shape was used by some of the experts to define the greater flat region and to define its characteristics.

So these characteristics of the various spatial models are important. Volcanic hazard, like seismic hazard and other hazard is a function of two things. Where things occur, the spatial aspect and the frequency with which they occur, the temporal aspect.

So we are looking for as much uncertainty characterization in those two as we can. We want alternative spatial models and their uncertainties, alternative temporal models and their uncertainties. Much of this deals with the spatial part of the problem, in addition to the particular temporal model that is used. When it comes to characterizing the temporal models, much

of that comes from counts, event counts within a particular region. And it's just shown diagrammatically here and move around across all of the various centers, Lathrop Wells, Northwest Crater Flats and so on and develop an uncertainty distribution in the number of counts, event counts and using their event definition at that particular location.

Going back a little bit, some of -- these are all source specific. We're dealing with a particular source zone. They become specific for spatial smoothing. They're also used to define for a vivariate Gaussian parametric shape, the number, the pattern of these centers on the map can help define the location axes of that distribution.

The issue of abrupt and gradual boundaries, it became clear after the first round of the elicitations were over, we did calculations and we went back to the feedback workshop and identified key issues. One of the key issues was the nature of the boundary between Crater Flat and Yucca Mountain. That's an important characteristic . Uncertainty needs to be quantified in that assumption and so this is an area. We gave them various options for dealing with the nature of the boundary. I would say in all cases uncertainties were characterized in the nature of that boundary.

We can talk more about it, but there's many ways this is done. For example, the source zones that are identified by a particular expert represent the pattern or the rate density difference between that zone and areas outside of it.

Within that zone, particular events occur that have a geometry, have a particular length and orientation and those lengths, as I'll show in this slide range up to 10 kilometers or more in length and these, for example, here's the maximum dike length shown here and the orientations. This is again, an integration across all of the experts.

These events are allowed to extend out of those zones into adjacent regions. All the experts allowed that to happen. So in fact, much of the hazard that occurs here in terms of the number of the frequency of intersection of dikes with the repository comes from dikes that occur, whose centers are in greater flat of the extent of the dike, but allow an intersection within the mountain block itself.

MR. MARSH: Now these are just estimated dike positions? These are not observed? these are not based on observed orientations of dikes in the area today?

MR. COPPERSMITH: that's right. There's no dikes --

MR. MARSH: There's very little seen except for some of the old --

MR. COPPERSMITH: Basically, it would be like voting on what you would think the distribution would be.

You can see the dike orientation, for example, it's based almost entirely on in situ stress measurements coming from the focal mechanisms of earthquakes, in situ hydrofrac measurements. There are a number of stress indications in the local Yucca Mountain area, has been the focus of study by the program.

MR. CONNOR: Could I make a quick comment on that, Kevin?

MR. COPPERSMITH: Yes.

MR. CONNOR: This is Chuck Connor. First you see the dike in Solitario Canyon which is 7.5 million years old. that's in the Solitario Canyon fault and also in pliocene Crater Flat which is very near surface, but you can map dike segments in the near surface in pliocene Crater Flat. That includes numerous dike segments in a zone that's 750 meters wide, that kind of thing and extending for something like 3 or 4 kilometers through pliocene Crater Flats.

So those are really the two dike sets closest to the repository that are best exposed. they both have north trends.

MR. COPPERSMITH: Most of the younger centers, the dikes are not observable.

MR. MARSH: But that data isn't in here, right? That's just used to make the opinion, right of the experts?

MR. COPPERSMITH: Sure. These are their judgments based on available data.

MEMBER HINZE: Kevin, if I can interrupt you for just a moment. Going to the NRC's concern about the PVHA and specifically with the geological basis of the zones, and you've processed models which have abrupt and gradual and also there are some models that include Yucca Mountain. My recollection of all the experts at least --

MR. COPPERSMITH: All the experts allow for finite probability of volcanism or the intersection probability to be is finite. It is above zero. So in all cases the site lies within a region that has a finite probability of being hit by a volcanic dike.

The issue is rate density.

MEMBER HINZE: Did any of the experts include Yucca Mountain in their -- at the same rate density that they did for Crater Flat?

MR. COPPERSMITH: Yes.

MEMBER HINZE: And would those results of those processing, give us a clue as to the degree of

concern that we have to have with the basis of the zones?

MR. COPPERSMITH: Well, to me when I go back and I look at and I think ultimately all the experts should be involved in this discussion, perhaps in licensing in a few years, if you look at the basis for their interpretations of their interpretations of the spatial distribution of future volcanism, however they did it, parametric field shape, spatial smoothing, source zones. It was based on a combination of things. The combination of their knowledge and experience in association of various types of structural elements, either large deep-seated structures or shallow faults and the observed distribution of say post-500 million year centers.

For example, the issue of association with volcanic events and faults is obviously known by and it was a basis, there was a lot of discussion about that topic. Some of the individuals, George Thompson, Alexa McBirney have published on this particular issue. It's something that obviously they're aware of.

There was a lot of discussion about the deep seated structure, the structural trends, Walker-Lane trend, other more northwesterly trends versus shallow seeded structure, what happens during the assent of magmas in the upper few kilometers of the crust, the co-existence

of extentional faulting with basaltic magneticism, obviously it's an issue for everyone who has worked in the basin and range.

I think, in general, though the driver, I think everyone is aware of the fact that there are normal faults in Yucca Mountain and there are inferred faults beneath Crater Flat. I think the driver though in terms of the rate difference between what's going on or is predicted for it to go on in Crater Flat versus Yucca Mountain is what's happened over the last 20 million years.

MEMBER HINZE: Is the results of this PVHA consistent with what we heard from Chuck Connor that the affected structure would change the probability by a factor of 2, roughly 2?

Is that --

MR. COPPERSMITH: I don't understand the notion because I know that almost all of these models are based on structure. The issue is what scale? Are we dealing with individual fault will be the location of the dike or are we dealing with regional scale structure? I think all -- I think everyone involved in this issue of small volume basaltic magmas is aware that regional structure plays a role, but it's an issue, I guess, of how it plays into the probability models themselves, to be

able to say it's a factor of 2 or 3.

My feeling at this point is that those issues, I think you can boil this all down to, we're dealing with the rate, and I think Chuck was trying to say the same thing, the location of events and the rate. In order for the results, the probabilities annual frequencies of intersection to be significantly different, we need to find an order of magnitude more events or we need to have a much higher rate density in the site area than has been - than has been assumed before.

MEMBER HINZE: In the range of the experts' view about probability, what was the -- was there a primary factor in all of them that made the spread? And if so, what was that or what was the view, either a number of different things, dike orientations, etcetera.

What gave the tails?

MR. COPPERSMITH: the tails? I can tell you what drives the mean.

MEMBER HINZE: What gave the range?

MR. MARSH: Sensitivity analysis here, the parameterization would really be useful.

MR. COPPERSMITH: Yes, we have in the -- we have about 40 or 50 pages of that with plots in the report. Basically, the issue deals -- it's fairly simple.

You've got the spatial models which deals with the location of events. Where will future events occur. Right now, most of the experts believe that where you see them is where you're going to get them with uncertainty. Okay, so the fact that you've got distributions over the last three and a half million years or so or maybe the last 5 million years including Amargosa Valley in a particular area, that's where they're expected, that's where the rate density is highest. That's where they're most likely to occur.

That is uncertain and the uncertainty in that is either couched in terms of variations in that geometry or variations in uncertainties in the length of dikes themselves as they might extend in Yucca Mountain. That's one issue. the second is the rate issue, the frequency issue. How often, what's the rate per year of volcanic events in this area? That's where event counts are used and there's uncertainty in those event counts. One of those uncertainties is maybe we don't have all the events identified. And this was called the hidden event factor, essentially, how many more might we find and in general you can see people will add ten to 50 percent more. In other words, take that event count, multiply it by 1.1 or 1.5. That's to account for the fact that number 1 in Amargosa Valley, we have quaternary fill that might be

hidden beneath that. I have the aeromag information they had at that time. Or lost underneath the actual lava sheets themselves or otherwise not identified these individual events.

So that is the rate part. And I think when we look at what drives the hazard right now, it's that where you allow the future distribution to be, and what that rate will be, so you need a significant difference, significant change in those two.

Someone asked the question about the rate density in this area per square kilometer for a typical volcanic field. This has very low event counts. It's been pointed out for a long time the average recurrence interval here is about 200,000 to 500,000 years between events. So this has a very low rate density and that's the problem, right? If we had a field with 1,000 events on it, we'd have much higher confidence in the spatial distribution and the frequency. We have very low events, so that's always been the paradox of this particular field.

So we have, I would say that the types of things that need to change radically would be much higher rate density in Yucca Mountain which means that the spatial distribution would need to change significantly from what's been judged by this Panel and the rates need

to go up significantly also.

So we must -- and I say significantly, for these types of models, factors of ten, that we haven't seen. So I think Chuck is right when he says this change is by a factor of 2. That's right, and that 2 is on the 10 to the minus 8. So when we deal with -- I deal with a lot of work in the seismic area, we're dealing with usually annual probabilities of exceeding some design ground motion of 10 to the minus 3 or 10 to the minus 4. these are on the order of 10 to the minus 7, 10 to the minus 10 is the range we have for this.

MR. RYAN: Kevin? A perspective on this figure 362 with respect to the dike orientation plot and the maximum dike length plot, both of those complete spectrums are often observed in the same single volcano.

MR. COPPERSMITH: Yes.

MR. RYAN: So it's in one sense --

MR. COPPERSMITH: Yes.

MR. RYAN: Can be interpreted as variation within vis-a-vis variation between --

MR. COPPERSMITH: Exactly. I hate to use the words, but I'm going to half to Paul. These are the differences between aleatory and epistemic uncertainties.

CHAIRMAN POMEROY: I knew that would get in there.

MR. COPPERSMITH: Yes. They were told that. The first would be the randomness, the aleatory component that is allowed and you'd expect it for a given set of events. The second is their uncertainty in what that is. We separated the two, the epistemic part goes in the logic tree, the aleatory, the random component is embedded in the hazard directly.

I had to say it. These issues of aleatory and epistemic are ones that the seismic experts are learning about daily.

MR. COPPERSMITH: Let me just show some of the results. Like I said, the PVHA report has much more sensitivity than I am able to show here. This just gives a feel for -- these are all, again, the annual frequency of intersection of a volcanic dike with the repository for each of the experts. And in general, you can see that their range -- the reason these are discrete values is the logic tree is a discrete process. There are particular paths in that tree that also is associated with probability. So if we say we deal with a dike that is 6 kilometers long, is oriented north, 40 degrees, east and it's within this particular source zone, it has this rate density of occurrence and that leads to this probability of intersection. That's one particular path associated with a probability which is the product of all those

branches leading to that point.

When you put those all together, these logic trees have tens of thousands of branches or more that take into account their uncertainty in all of the spatial and temporal components. So for a particular expert, the amount of uncertainty that's captured is on the order of about two orders of magnitude on the annual frequency of intersection.

This gives a feel for that within expert uncertainty.

MEMBER GARRICK: How independent were these arrived at in terms of the individuals?

MR. COPPERSMITH: They are not independent.

MEMBER GARRICK: They are not independent, but obviously they weren't independent in the sense that the panel discussed the results, but were they independent in the actual construction of the distributions in terms of the actual construction? Did they go off by themselves and --

MR. COPPERSMITH: There were individual interviews with each of the experts, but then we had a feedback workshop where they shared their preliminary assessments and then they were finalized after that.

MEMBER GARRICK: Now what happened between those last two steps? Was there much convergence?

MR. COPPERSMITH: No, there wasn't much change at all. Actually, that's been fairly common where we've carried out a feedback process, it's usually relatively small amount of actual change that occurs. There's much more that goes on in terms of the thought process and ultimately much better documentation of the technical basis for the assessments.

They're challenged. For example, when someone stands up there and George Walker will have an event, a dike length distribution that goes up to 55 kilometers and George will say, are you kidding, for these low volume, what are you talking about? George will say well, I saw it in Iceland this is what I saw there and then he'll think about his weight, whether or not he wants to give that lower weight, higher weight, whatever. But there's a technical challenge that goes on in that feedback process.

They then go back and finalize on their own, their assessments. The old issue of they're either completely independent or as interdependent as you can get them, in between is where you don't want to be. This is a very interactive process.

MEMBER GARRICK: What information do you have in the report that indicates the supporting evidence for the distribution by individual?

MR. COPPERSMITH: There's an elicitation

summary that's given by individual and signed by each individual. It's their documentation.

MEMBER GARRICK: Yes, is it clear in the process what the individual's distribution is based on?

MR. COPPERSMITH: Yes, they do not calculate the distributions. They provided all the input to those. They gave us the model to do the calculations. They see the results. It's a -- all of the input components in terms of the alternative models that were used and so on are explained in the report.

MR. MARSH: Is there a simple explanation that you've looked through sensitivity analysis of why they differ like they do in the mean?

MR. COPPERSMITH: Yes.

MR. MARSH: Can you give a few significant factors?

MR. COPPERSMITH: Yes.

MR. MARSH: What are they?

MR. COPPERSMITH: In general, they have to do with, I'll be going into each individual, but they have to do with the spatial distribution of what they think is going on in Crater Flat versus Yucca Mountain area and the rate density, the number of events per year that they assume will occur. And thirdly, the event geometry, what is the length and orientation of events.

MR. MARSH: So it's basically a difference in philosophical approach to the weights you give to the three things?

MR. COPPERSMITH: That's right. For example, George Walker, in event geometry, had a bimodal distribution. He felt that you have 50 percent probability that it would be in the northwest and the northeast sectors and it was aleatory that was the actual range that he expects to see. A bimodal distribution given 1000 dikes. You go around and count, half of them have this distribution and half have the other.

There's a different probability of intersection given those two cases, given the geometry of the particular center. So those could all be dissected and what we did in the study was to show for a given dike geometry or dike azimuth or hidden event factor or event counts at some particular, Crater Flat, and so on, the variation due to different time periods that are assumed or different spatial models that are used, we show how the results change for those different models.

MR. RYAN: It's entirely possible that George's response to that was influenced by his mapping of the Kulao volcano which, in fact, shows that kind of bimodality.

MR. COPPERSMITH: He gave elicitation summary,

he gave three or four examples. I've never -- in his elicitation over the course of a couple of days we must have visited 30 or 40 sites around the world in vivid detail. The issue of rafting, he can give you an example of when he got on a rafted piece of lava and observed velocities and everything else. It's an amazing group of people who looked at -- represented over 300 years of cumulative professional experience. It's quite a group.

MR. FOLAND: One point of clarification to follow up on Bruce's question, the opposite of that effect, the models that turn out to have high probability, are there some factors which are common among the various experts which might lead one to think that there's something systematic in the high estimations?

MR. COPPERSMITH: I would say their event counts are very similar to each other. They range from say -- total accounts in this area may be 8 to 20 events and I think that's a relatively small, maybe a factor of 2 to 3 variation in event counts leads to essentially the frequency part of this is very comparable.

Also, in general, have a fairly high reliance on the spatial location of observed centers over the last 5 million years. Most of them use the last 5 million years of record. They do not want to go into the miocene aspect when you had calderas and silicate volcanism stage.

The tectonic history tells me to stay out of that and to begin at 5 million years.

So they're basically using the same time period, the same number of events and in general, the same overall distribution of events that again comes from the observed centers. I think that's what drives the mean. So to get the mean to change those are the things that would have to change significantly.

MEMBER HINZE: Did you have a major change from abrupt to gradual boundaries?

MR. COPPERSMITH: We had, for those who went to the feedback workshop, we spent a day on this issue of what is that boundary abrupt or not between Crater Flat and Yucca Mountain.

The difference is in belief. What you're dealing with in abrupt to gradual is you're saying how does the rate density of events change. It's similar to a seismic source zone in that sense. Within this area you've got an A value that is a certain amount and in this area, there's an A value that has a certain amount. What they've done here though is to allow for events to extend out, number one. Their event geometries, their average, their mean estimates of event geometries are 2 to 5 kilometers. You saw the maximums go up into 10 to 20. So they all allow, number one for it to extend out. That's

like the uncertainty in the epicenter. And secondly, they explicitly, some gave uncertainty in that boundary either through like Bruce Crowe, here's a bunch of alternative source zones or like George Thompson, here's my uncertainty, my fall off. I've got a 5 kilometer wide zone where that boundary could actually be. So if they explicitly dealt with that issue, now but they do believe that, in fact, because when you go up to Yucca Mountain, the last 10 million years there have been no observed volcanic centers. In a bedrock environment you should be able to observe them. In fact, we do see one up at Solitario Canyon, at 10.5 million years, versus Crater Flat where the action has been.

Now when it comes to details then, well, there's faults at Yucca Mountain, couldn't that localize volcanism? Yes. But it hasn't. It hasn't very often, so they allow for finite probability event to occur. All the site lies within a zone for everyone. There is a finite probability, but it's low and the reason it's low is because it hasn't in the last 10 million years.

So I think it basically boils down to their prerogative to the weight they'd like to put on that particular model.

CHAIRMAN POMEROY: Kevin, before you take that away, can I just ask a question kind of somewhat for the

record?

I've heard the statement made informally that the range of probabilities resulted from one or two people's particular opinions or derived opinions. My interpretation when I looked at this slide a while ago was that that wasn't true. Is that a fair --

MR. COPPERSMITH: Number one, you have to think of the personalities. If you've been through these before. These people easily are swayed by one or two dominant personalities. I've never seen a panel of more dominant personalities. They are all very sure of themselves. In fact, we were warned beforehand that hey watch it, this is a contentious bunch. And in fact, they are very confident. I see no evidence that they were swayed by a particular view in an undue way. I think they are obviously all of us if we're sold by the technical arguments for a particular idea, we will agree with it. For example, at some of the methodology issues, the spatial smoothing, for example, many of the technologies for doing that didn't exist before Chuck did his work. And when it was presented, they said this is a good way because they do derive a lot of comfort from the pattern of post-5 million year centers. Well, here's a way to use that. And so that spatial smoothing process was used. Then they put some changes on it and said maybe I'll draw

a source zone and say that 90 percent of the probability density lies within it, but will allow 10 percent to trail out. And within that I'll do special smoothing to get the local probability distribution.

I think that things like that, you could say well they all jumped on the smoothing band wagon or they all jumped on the Amargosa Valley isotopic provence, not without consideration. I think they thought about the issue and there are certain ideas that they've felt comfort in.

I think the dike orientation in the northeast quadrant, they're swayed very much by the pattern of and it was discussed of the maximum horizontal compressive stresses that are a whole bunch of stress measurements that have been made in the area.

The next vu-graph just summarizes that probability distribution. This is the PVHA probability distribution function, the PDF that ranges from 10 to the minus 7 up to 10 to the minus 10 per year. You can see that that full range of about three orders of magnitude is somewhat more than any particular expert which is on the order of 1.5 to maybe 2 orders of magnitude. That's obviously the expert to expert uncertainty.

In general, we were looking at about a third of the uncertainty coming from expert to expert

differences, two-thirds from within, what's called from within expert differences and that also is not uncommon for these types of studies. Particularly if we're dealing with say more than five experts or so.

But that probability distribution then is something that has a mean value and has a number of parameters that describe it. But I think when we deal with changes then, we should be looking at what would change this distribution and particularly since we know the risk is often driven by the mean value, what would change the mean value of that probability distribution.

In the discussions that Chuck was having, the 10 to the minus 7, 10 to the minus 8 area, falls within this distribution. But the mean value is about 10 to the minus 8.

MEMBER GARRICK: You say the risk is driven by the mean values, but that's if you're using mean values as your propagation parameter?

MR. COPPERSMITH: Yes, exactly. Again, I've bene involved in PRAs for seismic and I do hazard analysis. We provide the full hazard distribution that can be broken down in fractiles. That would be the intent here and I guess Abe will talk a little bit about the sampling, but right now we start with PDF and then there's interaction that goes on with the risk analyst in terms of

what components they feel of that PDF will be most important to risk. I'll try to stay out of that. I know a little bit about that problem.

Just in general, and conclusions, really a heavy emphasis on uncertainty characterization. That's really what we're after in this type of study. We tried to get them to consider as many alternative models, spatial and temporal as possible. This summarizes the components of uncertainty. These are the significant contributors to uncertainty. One of the other I didn't talk about, but one of the other sensitivity analyses that are done, one is just to turn knobs and looks at the variation and the mean result or in the full probability distribution. The other is to look at that total PDF and look at what contributes most to the uncertainty, what's the contribution to variance in the PDF and we've done that and looked at basically the rate parameter and the choice and spatial model, some of these issues are the ones that drive the uncertainty characterization. So this is the description of that PDF.

Now what we've done recently is to evaluate some of the new data that Chuck talked about and I have to begin with a premise that the PVHA was focused on the knowledge and uncertainty and the frequency of dike intersection. It's believed to be robust and quantitative

and then we'll now look at it as new data or gathered, we'll look at their effect, the significance of those data to the PDF is the important part of the problem.

One of the reasons that multi-expert studies are done is so that they will have some robustness to them. Rather than have an individual expert give you a single value which is subject to change with whim or with the addition of new data, these individual experts provided a probability distribution themselves. Across a panel we have still broader probability distribution.

So we're hoping for an inherent robustness that's come from this and again multi-expert studies are usually motivated by the need for something that will be somewhat long lived, some of the seismic hazard studies done for the Eastern U.S., for example, were done in order to calm things down and to characterize uncertainties that would be robust for some period of time.

They were given all the -- devoted a lot of effort looking at the existing data, testing alternative hypotheses and so on, so now we come to new data that have been gathered and we need to look at their influence on the PDF. I would say that in general we should be looking at changes in the mean of that distribution, but we can look at recalculation of the full distribution, if needed.

New data as they come up, first we look at

their implications to PVHA. There are geologic data, even volcanic data that can be gathered that, in fact, don't have any influence on the input parameters to PVHA, compare it to the assessments made by the experts and then do a quantitative assessment or calculation and compare it to the PDF.

So the two new data sets that we looked at and were discussed earlier. There's increased volume estimate of Little Cones that was based on the ground mag that Chuck Connor talked about and an additional very volcanic feature in Amargosa Valley, again based on modeling on ground magnetics.

In terms of the volume at Little Cones, it is included as a potentially significant. It does affect the assessment that one of the experts made. He used the volume predictable approach to the temporal aspect of his problem,Rick Carlson, and therefore the volume, the integrated volume within Crater Flat which was used to make an assessment of the time periods or the frequency of occurrence of volcanism within Crater Flat could be affected by the revised volume estimate.

The bottom line when you go through that and make the change is a very small change in the total volume estimate which is the way that he carried the problem through and results in a very, very small change in the

mean annual frequency of intersection.

In the case of the Amargosa Valley anomalies, I need to clarify a little designation of what we're talking about. This aeromagnetic map which may be a very poor copy in your packet, it's the aeromagneitc map that was developed by Jean Langenheim at the U.S. Geological Survey and she presented it at two workshops. It was provided to the experts for their consideration. This is the Yucca Mountain area here. This is the Amargosa Valley and area of deep quaternary fill and these are the anomalies that exist within Amargosa Valley. These two Anomalies F and G, are the two that were identified in Langenheim's map at the time. This small anomaly here now apparently is the one that has been evaluated with ground mag and seemed to also be very similar in character to F and G. So we're dealing with the potential addition of another event here or another particular anomaly and looking at whether or not the experts considered F and G themselves to be potential events. And so we've gone back, given this information, gone back and looked at the characterization that the individuals made, not only in terms of their consideration of what's going on in Amargosa Valley, but also their event definition. For example, we're dealing with a northeasterly trend, a total length that's on the order of about 4 kilometers,

consistent with the event definition for many of the experts. So with the addition of another event here doesn't necessarily mean that we add another event depending on their event definition.

We also, for those that gave a low probability to either F or G being an anomaly, feel that the new data do support more highly the existence of those features being, in fact, buried volcanic centers.

So the event count, the basic bottom line in terms of the effect of this is the event count definition, being increased by 1 to 3 new events.

What that does in terms of their mean or average event count in Amargosa Valley, again, there's uncertainty characterized here so the number of events that an expert might consider for Amargosa Valley would be the possibility, let's say there are three events there with a particular weight, four events with a particular weight, five events with a particular weight. So the average of those is the number of events times those probabilities. And so the average across all the experts at Amargosa Valley before this information was 4.7 events in Amargosa Valley. We went back and looking at the event counts and made a revision based on the new information and have essentially 6.1 events in Amargosa Valley.

And that -- let me just -- there are many,

many of these source maps that we could look at. This is just an example from Bill Hackett who considers one particular interpretation of -- that includes Crater Flat. This is one source zone within which you will have a certain rate density of volcanic events. Another zone that looks like this that would include some of the older events, his zonation, locations were based on the age of particular centers. Presumably B is the only anomaly out there that has been drilled and dated on the order of about 3.8 million years old. Presumably, these based on the depth of the quaternary cover and polarity and so on can make some arguments that they also were in that age, this is in the post 5 million year category, would be included in this particular source zone.

So I'm going through and looking at those definitions, revising the event counts and we've just --I guess 1996 isn't particularly old, but just to make the point that was the assessment that was made previously and this is the updated -- my 9-year-old daughter thinks I'm very old, so this is --

MEMBER HINZE: Is that in our pile of materials?

MR. COPPERSMITH: Should be. If not, I'll take the blame.

MS. DEERING: Yes.

MR. COPPERSMITH: It should be the second to last.

MEMBER HINZE: Fine, thank you.

MR. COPPERSMITH: And this is the calculated difference. I mean it's obviously, this change, you can see how it works. You have your added particular account out of the increase, say a 20 percent increase in the number of events in Amargosa Valley. That plays into a particular spatial distribution. If it lies within a source zone like Bill Hackett's did that includes the site that would lead to increased hazard and there is a slight increase in the hazard related to this. If you're using a spatial smoothing approach that smooths those with a short smoothing distance, the operator has a short smoothing distance, keeps the events in Amargosa Valley, they will not extend and intersect the repositories so there's no difference for those particular scenarios.

So when you take it, put this revised input and turn the crank on the whole PVHA, the difference in the cumulative distribution is shown here. That's the dashed line. So essentially it's about a 3 percent increase in the hazard related to this additional event.

Actually, in some cases it's more than one event for some of the experts it ends up because F & G are better defined, they end up adding up to three additional

events.

So the bottom line in terms of this new information is it doesn't have a significant impact on hazard and I think that in general the types of things that would lead -- we need to see for those again who are involved in risk, I think ultimately the test of significance should not be one at the hazard level at all. It should be at the risk level. What would lead to a significant difference in the risk measure and since so far at these probability levels the PVHA or the hazards result from volcanism has no effect on the risk at all in any way. It's difficult to see what types of changes in it would lead to significant differences in risk.

Now changes in the mean hazard, the mean volcanic hazard would come again from I think very different rate densities, counts that would be increased by an order of magnitude essentially, coupled with very different spatial models than were assumed here, essentially allowing for rates that are comparable to Crater Flat, if not higher, occurring in the Yucca Mountain block. I think this panel of experts because of the absence over the last 10 million years of volcanic events within the block would be very reluctant to allow for the types of numbers we see in Crater Flat on the order of 5 to 10 events with Crater Flat, occurring up in

the Yucca Mountain block.

MEMBER HINZE: Questions?

MR. MARSH: Yes, I have a couple of questions, Kevin. I'm struck by the fact that the probabilities, individual ones that are estimated by people is very reflective of the experience of these people. The three people really who deal with active volcanism, Sheridan, Fisher and Walker have very tight, nice bell-shaped patterns and the other people, it all scatters all the way to Duffield's, really scattered.

What I'm curious about is have you ever run this process with nonexperts? In other words, people who are geologists, let's say, but who don't deal in volcanology? What I'm curious about is how much the process, the model drives the results rather than the experts in the sensitivity of that regard. In other words, you get a group of people together, you give them this information, you let them talk and everything else and the way you do the calculations comes out, do you get basically a similar thing? Have you ever tried that?

MR. COPPERSMITH: No. There's arguments about the whole issue of expert -- what are the components -- how much is diversity -- expert to expert diversity needed, for example. Do we need -- these studies are not cheap. They involve a lot of people and a lot of time.

MR. MARSH: I can imagine you wouldn't want to take people to the field and everything, but I would --

MR. COPPERSMITH: The reason that is brought up isn't because of the resource part of the probable, but couldn't it better be done by an individual or by a small group capturing the uncertainty in the best way that they can.

I think what happens here is you do need obviously informed experts. They have to be able to understand not only come to it with particular disciplines and experience that are pertinent to the problem, but they need to spend a lot of time on the site-specific information that is available.

MR. MARSH: The feeling I get is that just because of the number of occurrences of volcanism in this era, time period, in the region is that that sets the mean by a margin and we're just talking about expertise tweaking things back and forth a little bit. I'm just wondering whether that baseline calculation sets the results and then you add in this other voting back and forth and it basically gives you an uncertainty sprint and --

MR. COPPERSMITH: To a large extent, but the -- in general, yes. It sets the basic orders of magnitude. We're not dealing with 10 to the minus 3. If we had an

active late quaternary or holocene volcanic field, you' d be dealing with orders of magnitude, more likely occurrences.

It does set the basic frequency of the basic rate.

MR. MARSH: So this sets the basic difference between what we saw with Chuck in this model. It's basically the model, the baseline for the models is actually different.

MR. COPPERSMITH. No, actually the baselines are very similar.

MR. MARSH: Have you calibrated, trying to do and see if you actually take the same information, use the same kind of -- do you get the same numbers, do you know?

MR. COPPERSMITH: Yes, they're very similar? The event counts is a common, like I mentioned before, it varies by maybe a factor of 2, maybe as much as 3 across all these experts.

MR. MARSH: You actually treat it and incorporate it exactly the same way in both models?

MR. COPPERSMITH: And particularly, your latest has allowed for dikes to have a particular dimension. But I think the differences lie in the spatial distribution. A model that says there's equal likelihood of let's say if you're going to use it, assume that the

fault density within Crater Flat is the same as the fault density within Yucca Mountain and you're going to assume that they have the same rate density across that area, then there will be a real difference from what these experts have said because they want a higher rate density where you've seen plio-pleistocene, volcanic centers.

MR. MARSH: That actually brings up a philosophical difference in the field itself, you know in seismology or seismicity, is that there's a group that says, for example, in the San Andreas Fault or the Aleutian Trench, where there are no earthquakes now, there will be a big one in the future because it's just stuck or the other people believe where there are no earthquakes now, there are just not going to be an earthquakes.

MR. COPPERSMITH: Exactly.

MR. MARSH: So it depends on how you almost have a group, you may get a group of people who have all of one belief or all of another belief.

MR. COPPERSMITH: WE tried, that issue of spatial stationarity, Allin Cornell is one of our, was on the methodology development team. Peter Morris from Applied Decision Analysis. There are others who have a real background in seismic part and dealing with the issues of spatial stationarity. Because it is a driver, particularly in regions like this. The eastern United

States the historical record is very short.

Here's a case where we're dealing with the observed pattern, very few of volcanic events within this region. to what degree we believe that spatial distribution will hold to the future and that's where variations, for example, in the smoothing constant, how long the smoothing distance do you allow? Is it few kilometers, saying where it's occurred is where it's going to happen almost exactly? 20 kilometers, 30 kilometers, those differences are basically your degree of belief in the stationarity in the future.

I think the driver here though, as opposed to say the historical record for seismicity is that record is a geologic record. It's a post-5 million year record. These numbers are looking forward say over the next 100,000 years. that has much more significance than say just the short historical period.

MR. COPPERSMITH: In fact, there might be an anisotropy, more or less, in this process because of the fact that when you have very few occurrences, they actually may occur around each other. When you have widespread occurrences, they may occur more homogeneously. And so there's a kernal awaiting function in this also.

MR. MARSH: Mike Sheridan who has developed this parametric field shape is based on typical fields

that are 100 to 1,000 events. And when he deals here with let's say 10 to 20 events, he has that same problem. These tend to cluster and as the center is shown, as you look around, and look in detail, you find that there is actually much more clustering going on. His parametric field shape says that it's relatively uniform over that Gaussian distribution. He also gave other weight then to spatial smoothing that had much more tighter clustering to allow for -- maybe this is because a few counts is much more clustered than his field shape approach would say.

MR. RYAN: It might be interesting to think of a hypothetical experiment in which one looked at, for example, Long Valley which last erupted some several 100,000 years ago with the Bishop Tuff and think of a comparable experiment that would be conducted with a data base that would go up to I suppose 1975, and then one with another data base that would pick up at 1975 and go up to the present and think of the differences in outcome. Now that we know there is seismic activity and a great deal of deformation activity at Long Valley, you would come away with two exceedingly different kinds of conclusions.

MR. COPPERSMITH: One of the questions, I can't again answer that directly, other than at the last workshop we asked the question of the experts, what would cause, do you remember when we asked them do you feel that

your assessments are systematically different from the larger technical community if they had gone through the same process and their answer was no, they are not systematically different. They weren't sure they wanted to put in any of their colleagues through the same process.

We also asked them what would cause you to change? What new data would cause a change? All of those assessments were basically premonitory or precursory types of information. And ascending a zone a size -- a fountain of fire in Crater Flat and so on, but then when pressed, we're not dealing with a process here of prediction. We're dealing with long-term forecasting.

So when we asked the question, okay, it's out in Crater Flat, you added one event out there to your event count and it's in a place where you already had a higher rate density, how would it change your hazard assessment.

Certainly it changes your precursory short term prediction. In most cases, they felt, no, it wouldn't change my hazard assessment, it would just cause us to lean to a short term warning or prediction. So in fact it deals with, the answer to those questions are a factor of 10 increase in the number of counts, things like that or the geologist finding multiple dikes within Yucca

Mountain, those are the types of things that lead to real changes.

MEMBER HINZE: Further questions? If not, it's my privilege to thank you, Kevin, for an excellent presentation and particularly the sensitivity studies on these new hits that are extremely revealing. I would beg the Chairman's indulgence her to let us have a short time off before we move on to Gene Yogodzinksi. Can we have some lunch?

CHAIRMAN POMEROY: Certainly. When are we coming back then? Can we make it back by 1:15? Let's do it.

(Whereupon, at 12:34 p.m., the meeting was recessed, to reconvene at 1:15 p.m., Tuesday, April 22, 1997.)

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

(1:25 p.m.)

CHAIRMAN POMEROY: The meeting will come to order.

MEMBER HINZE: We are now at 11:00, and Gene Yogodzinski is front and center. Gene is here representing the UNLV program and the State of Nevada, which has been supporting that program. Gene has been involved in this, and he is currently a faculty member at Dickinson College.

Gene, are there two handouts from you? There are two sets. There are two parts of the talk.

MR. YOGODZINSKI: Let me start.

I'll talk about two things today. The area of interest for PVHA at Yucca Mountain, and this is something I was very much involved in in my two years at UNLV as a Research Associate. Gene Smith and I worked this up and presented it to the experts, and it very much considered in their models. Something that happened since I -- I left UNLV at about the time of the last -- the third Geomatrix workshop.

The second part of the talk will be something that I was completely uninvolved with. That is something Gene Smith and his student, Lori Dickson, who is working on her master's degree at UNLV, something that they have

worked up since the time I left there. And so I'll very much be presenting this for them. I think primarily I'll be talking about the first topic, though.

My interest, as it evolved, was in trying to establish an objective criteria for drawing a boundary around the area of interest for volcanic hazard assessment in Yucca Mountain. That is, the objective being to define the natural boundary that outlines the mafic magmatic system for the Yucca Mountain area.

And the assumption that I started with in doing this was that the distribution of pliocene and younger mafic volcanism is in some way tied to the distribution of melting anomalies in the mantle. This assumption places importance on the source chemistry for the basalts. And by far the largest database that goes directly to this is neodymium and strontium isotopes, and that is what this idea is built around.

These are, very quickly, those pliocene and younger mafic centers that are in the immediate vicinity of Yucca Mountain that are not covered by alluvium that have been the focus of most of the study for volcanic hazard assessment. There are, as anyone who has been out there knows, or anyone who has looked at a map like the Luedke-Smith map knows, it seems like you can't go 10 kilometers without tripping over a cinder cone.

There is a lot of this stuff out there, and there is a lot of data -- strontium and neodymium isotopic data -- for these four rocks in the Western Great Basin, which is what people call this area.

Yucca Mountain being right there, the California-Nevada border, I just took all of the data at the time that I had gleaned out of the literature, plus some that we developed up here, and also in the immediate Yucca Mountain area, and what these are are not really -- some of them are volcanic fields. They are really convenient geographical groupings based on the places where we had access to data.

And to go right to -- well, here is what they all look like together. These are isotopic measurements. Many of you know exactly what these mean and how they're determined. Some of you might not. You can think of these strictly as chemical fingerprints. When the mantle melts, it imparts its chemical fingerprint -- neodymium isotopic and strontium isotopic -- on the magma that that melting produces.

And assuming that very little has happened to that composition on the way from the mantle to the surface, what you get in the basalt flow or in the cinder cone or in the dike is what is -- it reflects what was in the mantle source.

This is 277 data points. It has been pointed out to me that there are more than this actually there in the literature. Nothing has been systematically excluded. No one has suggested that conclusions from 320 data points is any different from the conclusion that I'll reach based on this 277.

This work really came to an end when I left UNLV, and so I'm really reporting to you now just a very brief update on what I was reporting a couple of years ago at the Geomatrix workshop. So that's all the data.

What is very interesting, and I believe important about the Yucca Mountain, samples from the immediate area around Yucca Mountain, is that they are not exactly all over the map. They really form a very tight cluster all the way at the low epsilon neodymium and the high strontium-87/86 end of the data array.

What this implies is that these rocks, these basalts, were produced by melting of the lithospheric mantle, the very cold part of the mantle. It requires about a billion or a billion and a half years to create this isotopic signature in the mantle. It has to be isolated from the convective part of the mantle for a long time.

So this is a very distinctive end member, and this end member up here would represent the

aesthenispheric part of the mantle. That is, the part that is convecting and mixing over time. And I think most geochemists and petrologists agree that, broadly speaking, this array is a mixture of convective and non-convective mantle sources.

One outlier at Thirsty Mesa, but other than that, in the range of minus 8 to minus 11 epsilon units is very distinctive.

As we go around, to get just the one other point I'd make about this, is that these -- it probably does -- it's a very good question as to exactly how you actually melt lithospheric mantle, which should be cold. It's probably because it has water in it, and that is something that came up earlier. It probably has to be hydrous mantle to be melted and some decompression of that hydrous mantle probably allows it to melt.

Certainly, there is every reason geochemically in the trace elements of these rocks that leads us to believe that they are related somehow to some ancient subduction process, and that, too, is consistent with the idea that this is hydrous mantle.

Okay. Set that aside. So I'll try and get to the punch line as quickly as I can. I won't go through all of these areas. I'll skip Lake Tahoe, Mono Lake, and Lake Mead. These areas are relatively far away and

chemically have very little to do with Yucca Mountain.

I'll go to the Pancake and Reveille Ranges up here far to the north of the Mojave Range, and then I'll look at the combination of these two areas here. And what we'll conclude is, really, it's only this what I call Death Valley southeast that appears to be related chemically to Yucca Mountain.

Here is all of the data for the Mojave. Principally -- well, a lot of -- it's spread out all over the place, but a lot of the data up here is in the Semavolcanic field. The Reveille Range and Lunar Crater field were up to the north of the Yucca Mountain area, and you can see what part of the diagram this anchors.

This anchors the aesthenispheric part, the warm, convective mantle source part of the geochemical variation in the Western Great Basin. These obviously have little to do with what's going on down here, and that difference is even more dramatic when you look actually at the trace elements in these rocks.

The area to the west of Yucca Mountain actually has a lot more in common with Yucca Mountain itself, and I'll combine the Saline Range, Big Pine field, and what I call Death Valley northwest -- these areas -- on the next diagram.

And what you can see from this is that at

least some of the samples from Big Pine and Saline Range or Death Valley northwest extend down into the area, that really strong lithospheric end member, the area at the low epsilon neodymium. They extend into that area, but there is a very large or compositional range in the data for this area.

The distinctive thing about the Yucca Mountain area is that it is really centered in a small bull's-eye, relatively little chemical variation. Nonetheless, not entirely unique.

As I said at the outset, the only place in the Western Great Basin that has a unique geochemical signature that matches that of Yucca Mountain is the area that is immediately to its south.

MEMBER HINZE: Are there any temporal variations that you can give us that would provide any evidence that this is moving up and down?

MR. YOGODZINSKI: It is much -- well, I'll get to the question -- that's a very good question, and it's much easier to do a strontium or a neodymium isotopic analysis than it is to get a good age. And so most of these samples are not dated, and that's a problem.

So I look at this, and again, one of the things I'll try and point out is that actually in the southeast Death Valley area, at least that's what I call

-- the Funeral Formation is the formation name that appears on the maps -- there is relatively little data there. But all of that data that is available, all of those points that are available, match very closely in a very -- again, at this very distinctive isotopic end member what we see in the Yucca Mountain area.

So I would summarize, before I go to the map. Basalts in the Yucca Mountain area define a distinctive regional isotopic end member. There are similar basalts in the Saline Range of Death Valley northwest, but those geographic locations show a much greater variety of compositions. Pliocene and younger basalts in southeast Death Valley are isotopically identical to those in the Yucca Mountain area, and basalts of the Yucca Mountain area and southeast Death Valley form what I consider an isotopic province centered on the Amargosa Valley.

And so if you're looking for a natural boundary to draw around the magmatic system that is controlling or contributing to the formation of pliocene and younger cinder cones, in this part, in the Yucca Mountain area, I think this is one objective way to do that. Crater Flat is located right here; the proposed repository is here.

Northern boundaries -- off to the north is the Reveille Range and the Lunar Crater volcanic field.

Chemically, entirely different. Down to the south and the Mojave, chemically, it's another world. There is a lot of similarities out in this part of the world, in the Saline Range and Death Valley northwest.

But the boundary here encompasses just those analyses, those rocks, that fall within that very small compositional range. The area is controlled by Buckboard Mesa on the northeast, by the Sleeping Butte cones on the northwest, by this sample, D85-48 published by Rogers, et al., in 1995. That anchors the province down here. Cinder Hill volcano, published by Farmer, et al., 1989, controls the boundary here.

There is no control in this part of the world. I have simply drawn it around the distribution of basaltic centers in this part of the world; basically, in the Black and Greenwater Mountains, the Funeral Formation. This comes off the Luedke-Smith map. And really, no control out here.

The distances are not that great. Here is the proposed repository. The distance here is very similar -- that is, from the Sleeping Butte cones to the proposed repository -- is very similar to the distance from the proposed repository down to the aeromagnetic anomalies, which bridge the geographic gap down to the Greenwater and the Black Mountains.

Let me go to the next -- if we look at the most recent volcanic activity within the province, it actually jumps around quite a bit. The Crater Flat alignment at about a million years, right here, the Cinder Hill volcano, the best estimate on the age of that is about 700,000 years. That is way down here at this end.

At 300,000 years, the Sleeping Butte cones erupted, and at about 100,000 years, Lathrop Wells. So based on the youngest centers that we have out here, there is no clear pattern that emerges.

I think, though, what Britt said earlier today is really true. When you look at volcanic fields, they very often have some very clear spatial/temporal trend. In the Reveille Range to the north that we have looked at quite closely, you can see a large initial outbreak that -- about six million years ago that shrinks down to a very small volcanic field.

That kind of spatial/temporal pattern is very common in these kinds of volcanic fields, yet that kind of spatial/temporal pattern has not emerged in any way in the study of volcanism around Yucca Mountain.

And I believe, in part, it may be because so much attention has been focused on this area and not on a broader -- some broader area. There is very little known about the age distribution or the distribution of events

down in this part of the world.

Based on the amount of age information that is present in the Luedke-Smith map, it appears that these -- that in the southern half of this province ages are slightly older -- five to eight million years -- than in this part of the province. If that could be more accurately characterized, there might be -- a spatial/temporal pattern might emerge.

So my conclusions for this part of the talk are simply that the boundary around the Amargosa Valley isotopic province encompasses the magmatic system that produced mafic volcanism around Yucca Mountain for the past six million years or so. And I regard this as a natural boundary, and the system it encompasses should be considered in the PVHA process.

And, in fact, it was considered by most of the experts. This is Richard Carlson's area of interest. The AVIP and half the AVIP -- I don't know what kind of impact this could have, since there is so little information on mafic volcanism in this part of the world. This stuff has been studied, really, to death, and very little has been done here. There is very few ages and relatively few analyses done here.

MR. FOLAND: Gene?

MR. YOGODZINSKI: Yes?

MR. FOLAND: Just while you're on that, in the analysis that was done here, were the events in the southern part of the region included in the event count?

MR. YOGODZINSKI: I don't know the answer to that.

Kevin, I think that -- is it true that none of the events in this part of the world were included in this?

MR. COPPERSMITH: This is Kevin Coppersmith. What is shown on there is Rick Carlson's. He did not use them because of the thought at that time, like you said, that these are older than five million year --

MR. YOGODZINSKI: Right.

MR. COPPERSMITH: -- centers. There is uncertainty in the counts, even considering them to be older. And we had a presentation by Bruce Crowe, who had done some mapping down there. He had made, you know, estimate event counts around the order of 20 events or so over an older time period. A couple of experts used that but gave it relatively low weight because of the uncertainty in that southern part of the province.

MR. MARSH: The other thing that is obvious here, may be obvious, is that, you know, this may be a compositional province in the mantle, or in the source region. But that doesn't mean it's a boundary in the

crust.

In other words, if you tap a region below and it brings things up -- because it so happens over a five million year period this is what it will look like down there. It's like looking at cloud cover from a satellite above the earth.

MR. YOGODZINSKI: Right.

MR. MARSH: It doesn't really tell you anything about the structure in the crust that may be controlling the magmatic transport or that there is a boundary in the crust or a block in the near surface.

MR. YOGODZINSKI: This will tell you nothing about any kind of transport within the crust at all. I think the idea here is, or at least my idea here is, that this is a batch of mantle that has had a very similar history for a very long period of time, and so is likely to respond to basin and range extension in a similar way.

Now, once the melt is generated and the melt rises into the crust, where it's going to go, this has nothing to say about that. But I presume that predominantly the melts are coming up, and then they are going to be directed in the near surface by some structural control.

This area has had a long history, and it has remained a coherent area over that long history. And,

again, the idea is that it is going to respond in a similar way when basin and range extension and those kinds of forces act on it.

MR. MARSH: You know, you could take and just say, "I'll ignore the isotopes," because of the fact that we're just looking at a reservoir down there, and we'll look at age. We'll look at the ages, and we'll ignore them. That's another way you can draw things.

MR. YOGODZINSKI: Right.

MR. MARSH: But it isn't quite clear to me, and maybe I'm missing something, why this is terribly important in terms of -- I mean, you used the word "boundary." You used the word in the -- I think when you drew this thing around. And you call it a province, and I think "province" is an interesting word to use in the mantle and it would be -- but it isn't quite clear to me what you mean by drawing the circle in the crust.

MR. YOGODZINSKI: The alternative to this would be -- well, here's an alternative. Would be something like this. Chuck maybe is going to correct me on this, but I think most of the models require that you look at some area. The question is: what is the area that you look at?

You can restrict it by age, and I would restrict it in both senses, both -- I really only focused

on pliocene events. The southern end of this area seemed to have some that leak into the seven or eight million year old time span. But that is really very poorly known, very poorly known.

So given the option of looking at something like this, which has some geologic criteria associated with it, and something like this, this is maybe reasonable. This is saying that maybe the AVIP has some significance, and this is saying that we have no idea what may be significant in controlling the spatial patterns.

MR. MARSH: If I remember right from Chuck's explanation, though, is that in the models like he calculates is that the area -- you get to a point where the area itself is not terribly important.

MR. YOGODZINSKI: Right.

MR. MARSH: And that is right. And, I mean, it should be. It's like remember doing point counts. You get to a certain point, and it doesn't matter how many points you do.

But this kind of study -- I don't know if you've seen Lee Silver's four corners area that he shows like over a period of 30 million years or more, an area will show similar isotopic -- all kinds of trace element characteristics that are site specific for that area.

MR. YOGODZINSKI: Right.

MR. MARSH: I don't know that he's ever published them.

But in and of itself, those are tremendously different episodes, etcetera. So it all relates to, more or less, the chemistry of the earth there. But it does not tell us much about the process, and I think one of the things that is really important here is the process. In other words, is the eruptive cycles, for example, producing magma coming out?

MR. YOGODZINSKI: Right.

MR. MARSH: This may tell you about the flavor of the magmas, but it may not necessarily tell you about the process.

MR. YOGODZINSKI: Right. And if you look at volcanic fields that clearly stand out on their own, if you go to the north to an area that has its own very clear geographic distribution, and the Reveille Range, and the Lunar Crater volcanic field, that is very clearly its own isotopic province, and it very clearly is behaving independently of -- at least at some level independently of anything that is happening down here.

And if in this part of the world we can look at something that is chemically distinctive, and it's behaving in a temporal-geographic way that's distinctive, I think it's reasonable to expect that that kind of

control might be happening down here.

MEMBER HINZE: I guess the bottom line is that isotopic province is not necessarily -- can be, but it is not necessarily a volcanic source zone province. If that is true, then let me ask the question, what more could be done with the rocks, outside of the age, to see whether these are part of the same process, as you put it, Bruce?

MR. MARSH: Well, the key thing -- I think the key thing in process is that, especially for this evaluation, I think is that you need to look at things that are of a very similar age. And so it would be interesting to know whether everything that's in this general region -- the ages of them, by and large -- you know, volumes and ages.

MR. YOGODZINSKI: Right. That's what I would conclude, is that the Funeral Formation should be mapped and dated, and it's basic geologic work. It's not rocket science. It hasn't been done.

MR. HILL: And if I could interject for just a minute -- this is Britt Hill.

Fred Thompson and a few of his co-workers have had some people out in that area. There have been some more dating studies done in the Funeral that have only appeared in theses, and the age ranges from about 4.9 to 4.2 ma for at least 25 different events. And we're

looking at -- I believe it's about 11 cubic kilometers of basalt, based on what is presently there.

MR. YOGODZINSKI: So that was how many ages in that small age range?

MR. HILL: I think it's about five or six.

MR. YOGODZINSKI: Okay.

MR. HILL: But they pretty well covered the stratigraphic section.

MR. YOGODZINSKI: Right. That places it, I think -- it's obviously not quaternary, but it places it in the pliocei : and that's I think what most of us are interested in for this process.

MR. FOLAND: But what about the mantle? What is seen in the mantle? Now you've got this surface expression of perhaps a mantle signature. Partly getting at this, what is the driving force in the mantle for producing these melts? And what does one see seismically that might correlate with your province?

MR. YOGODZINSKI: They're so small in volume I'm not sure you could see it seismically. A geophysicist could answer that. I think to melt this mantle, which has been cold and isolated for a long period of time, it has to be hydrous in the first place. And there is every reason, based on the trace elements, to believe it is hydrous. There are amphiboles at Red Cone. Britt talked

about those earlier.

Amphibole is not a common mineral phase in these rocks. But then again, an amphibole breaks down very quickly as you bring it toward the surface. So I think there is very good reason to believe a priori, because of its isotopic signature in an indirect way, that it is hydrous, and some sort of lithospheric thinning in the Western Basin Range. That is, some decompression with a hydrous mantle.

I think that that is mostly what most petrologists believe has allowed you to melt this cold mantle at relatively small percentages. It is not a lot of magma, not at all. So the extension, in combination with a hydrous mantle. That's my view.

MR. FOLAND: Well, I mean, someone mentioned the tomography. But if I remember, Evans & Smith years ago, they actually found the slower velocities actually at the end of the entire trend. So going all the way down to sema was the slow velocity, hotter mantle, more aesthenispheric mantle. And then if you go up to Lunar Crater, that's the other analog.

And Yucca Mountain sits right in between these two --

MR. YOGODZINSKI: Right.

MR. FOLAND: -- not hot spots. Maybe hot lines.

MR. YOGODZINSKI: Right. But there is -- it appears, based on the chemistry, that none of that aesthenispheric mantle has mixed with -- has been really involved and can be seen chemically in the magmas at Yucca Mountain.

MR. FOLAND: At the surface.

MR. YOGODZINSKI: Yes. Yes.

Okay. Any more questions about --

MEMBER HINZE: Just one quick question. Do you have any further comments about the source zones that were used in the PVHA? You've shown us Richard Carlson's source zones. Do you feel that the AVIP was understood and properly --

MR. YOGODZINSKI: I believe it was understood, and I believe it was largely accepted. But I believe it had relatively little impact, because the event count was largely unknown, things like that, the ages are largely unknown.

And soon after I gave this talk last time, I left UNLV, and this has really been stagnant ever since, to be honest.

Okay. Now let me go on to something that Gene Smith asked me to present. And I think it actually connects nicely with a lot of the discussion this morning. Citadel Mountain is an analog for Yucca Mountain, an

analog with respect to the locations of the cinder cones on the mountain.

Citadel Mountain is a tilted fault block cored by Oligocene ash flow tuffs. It's located up in the Pancake Range north of the Yucca Mountain area. I'll show you a map, a rather poor map, in a second. It's an uplift of the fault block. It's mostly on the northwest and southeast with a dip slope off to the northeast. So in this sense, it's a little different than Yucca Mountain. Most of the uplift of Yucca Mountain is on the west, the dip slope off to the east.

A very simple, but I think relevant point that I'd like to make is that their cinder cones erupted along the entire range crests, as well as in the adjacent basins in this part of the world.

Now let me try and go to the map. That is actually not as bad as I thought. Actually, very quickly, if I put this up, I can get you located. Yucca Mountain is in this part of the world. This is the Reveille Range and the Lunar Crater volcanic field, and Citadel Mountain is here in the Lunar Crater volcanic field.

Now I'll try and talk about this. This is a xerox of the 1972 published map by Sullivan & Ekrund and others. And what I've outlined here in blue are the lava flows associated with cinder cones produced in the Lunar

Crater volcanic field. I have gone over -- I needed to make this figure because I wanted to highlight the structural features.

The mountain -- there is a dip slope on the mountain off to the northwest, relatively steep topographic gradients in the southeast -- excuse me, the northeast, relatively steep topographic gradients in the west and northwest. The mountain is cored by ash flow tuffs outflow, very much like those at Yucca Mountain.

And from this point, dotting all the way down the dip slope of the mountain, there are cinder cones. Now, if you've driven around out there, you've seen cinder cones perched up on the ridge crests. This is much less true in the immediate vicinity around Yucca Mountain, but you don't have to go very far before you see this.

Lunar Crater -- the reference is right there. So if you keep in mind where Lunar Crater is here, and this point down here, you can keep oriented on the following two figures.

This shows the distribution of lava flows in green and cinder cones in red. Lunar Crater is right there.

Again, steep topographic gradients here and here, a dip slope there, the Citadel Mountain, and a series of cinder cones over this area.

Now, Lori Dickson has worked on mapping this area and dating -- mapping out especially the relative relationships, age relationships among the lava flows, and they are in the process of obtaining new 4039 ages on the lava flows. And I'll show you her preliminary map, and I'll --

MR. RYAN: I'm sorry. What's the distinction between the brown and the green fields there?

MR. YOGODZINSKI: The green is the lava flows, and the red are the cinder cones. Now, some of the cinder cones produced big lava flows, and some of them are nothing but cinder. And so I'll now show you a map that connects the cinder cones with the lava flows that they produce.

MR. RYAN: And the lightest green is?

MR. YOGODZINSKI: This here is -- I think those are lake sediments in these tuff cones. This is Lunar Crater. It's an explosive crater, and there is apparently lake sediment at the bottom.

MR. RYAN: But they're compositionally equivalent?

MR. YOGODZINSKI: Yes. Everything here is basalt. Actually, the composition of these basalts are very different from basalts in the Yucca Mountain area.

This is the stratigraphy as it has been worked

out by mapping. The red cone here is associated with this cone. All of the cones are shown in red, and they are sitting on top of the lava flows that they produced.

The oldest one is P1. There is actually no cone associated with that. There are a series of dikes and scoria down here. This is thought to be the location where the P1 lava flow originated. It is the oldest one. It has been dated at 3.84. I'm not even writing the numbers down here. These are done at New Mexico Tech. It's preliminary data done on ground mass concentrates.

Slightly younger than that, you can see it geologically, that it's slightly younger, the relationships, are the H-cone, which originated at the crest of Citadel Mountain. And we know that the topography was well in place at this point, about 3.8 million years ago, because in this part of the world the lava actually cascades down across the existing topography. So the topography was there when the cones erupted.

Qc-cone is the large one up here, which erupted on the lower flanks of mountain. C-cone and R-cone there are the youngest, but the relative ages between them is not known. R-cone, again, produced the lava flow that cascaded down off existing topography, about 1.24 million years ago in the case of this volcano.

So the very simple point that --

MR. FOLAND: What's the gray?

MR. YOGODZINSKI: The gray is other cones that did not produce significant lava flows. In some cases the gray cones are older, and in some cases the gray cones are younger.

Lunar Crater is about 1.26 or 1.24. These cones, Gene tells me, are actually older than this lava flow. But some of these cones, like these two, are clearly younger than this lava flow. But there is virtually nothing there to date.

The other point that Gene asked me to make was to be sure that I said that on the published geologic map of this area, there are some cinder cones that are easy enough to map, yet they do escape the scrutiny of the mapper. This cone does not appear on the published geologic map of this area, and these two small cones here do not appear on the published geologic map of this area.

Ken?

MR. FOLAND: Yes. Well, you're presenting somebody else's work, so I won't hold you accountable.

MR. YOGODZINSKI: Okay.

MR. FOLAND: But the ring around Lunar Crater, those are flows, those are large flows, and those are --

MR. YOGODZINSKI: Well, wait a minute now. I

have been here. I haven't worked here. I have been here. Now, there are --

MR. FOLAND: Correct me if I'm wrong, there are some flows exposed in the walls of the crater. Those are older flows.

MR. YOGODZINSKI: About 3.8.

MR. FOLAND: 3.8.

MR. YOGODZINSKI: Right.

MR. FOLAND: So there is a whole series of flows.

MR. YOGODZINSKI: And then there is -- but the rim is made of tephra that is younger than that, about 1.24. Is that --

MR. FOLAND: No, I think that perhaps it's 100,000 years or something. That is pretty young. There is a 1.24 flow that comes from the north.

MR. YOGODZINSKI: Okay. Okay.

So, yes, in this case that cone is younger, but I think that Gene explicitly pointed out that these cones were older than at least the C-cone.

MR. FOLAND: Okay.

MR. YOGODZINSKI: So in the PVHA process, there were magma boundaries, structural boundaries to the flow of magma from the Crater Flat Basin up into the mountain. And this analog, in a very simple-minded way,

the analog study of Citadel Mountain suggests that these should be looked at very carefully.

It gets very easy to go out there and find something that is geometrically, geologically similar to what we have in Yucca Mountain. And a cross section through Citadel Mountain, and a cross section through Yucca Mountain, a comparison there might shed light on this magma barrier concept. I think I'll just leave it at that.

MEMBER HINZE: This is a topographic barrier?

MR. YOGODZINSKI: Structural barrier, magma barrier. Someone else is undoubtedly in a better position than I to comment on those ideas as they came into the experts' models.

MEMBER HINZE: Questions?

MR. FOLAND: I actually don't follow that, in terms of the magmatic barrier, because it doesn't seem to me that there is a magmatic barrier. You can just go right across -- what, is it Route 75 or 375, whichever one it is.

MR. YOGODZINSKI: No. I think the point here is that there is no magmatic barrier. In a mountain, Citadel Mountain -- this is the way I understand the reasoning -- in Citadel Mountain, geologically, it's very similar to Yucca Mountain, and there has been no magmatic

barrier.

MR. FOLAND: Okay. So it's the topographic argument, which I think is what Bill was suggesting, is that here is the mountain and the lava certainly came up in the elevation.

MR. YOGODZINSKI: Certainly came up at high elevations.

MEMBER HINZE: At one time, DOE was multiplying their results by .15 because of the height of Yucca Mountain. I'm wondering, Kevin, what role did topography play in the PVHA elicitation?

MR. COPPERSMITH: This is Kevin Coppersmith.

Let me make a point on the barrier. Number 1, none of the experts had a barrier, in the sense that a dike that was assumed to have its origin within Crater Flat could not extend into Yucca Mountain. There was not a hard barrier at any of the source boundaries.

So the way these models work is that, first, you look at the spatial distribution, you represent events as points, so that you're looking at spatial clustering of individual points. Then, you associate each point with a dike geometry, a length, an azimuth, and whether or not that dike is centered on the point, or, in fact, there's a probability distribution on where it will be relative to that point.

So for points that originate within Crater Flat, all of the experts allowed them to extend into Yucca Mountain. Okay? So there is no barrier from that point of view.

On the issue of topographic control, it was discussed quite a bit, because everyone can bring to bear an analog that shows dikes in upland areas, as well as within basins. I think George Thompson made the point that he expects, because of the configuration of the Basin Range of opposing fault blocks and opposing faults, dipping towards each other along the Basin margins, he would expect more volcanism within the basins.

But it was allowed for, and it was never -- it never came through as a strong determinant. In other words, I'm going to outline topography or the amplitude of topography to tell me about the likelihood of volcanism.

MR. YOGODZINSKI: I think that part of Gene Smith's reason for asking me to present this was in part because this is something that had been looked at subsequent to the PVHA process, and he would probably be in a better position to defend it than I am.

MEMBER HINZE: Thank you, Kevin, for those remarks. And certainly, you are quite appropriate to put that caveat on the presentation.

And we will thank Gene, and you pass our

thanks on to Gene as well, and thank you for your presentation on the AVIP.

We now move to the next part of the equation, and that is the consequence analysis. And Britt Hill, 15 minutes? No.

MR. HILL: 15?

CHAIRMAN POMEROY: Just put up the number, Britt.

MR. HILL: Oh, okay.

(Laughter.)

Well, most of what I would like to talk about this afternoon, since we are rather far behind, I'll try to just touch on the salient points and hopefully get some feedback from people on areas that aren't clear from previous publications.

MEMBER HINZE: Don't short sheet us. We want the story.

MR. HILL: I'll try to be succinct.

There is really two things that we're trying to look at that I'll try to address in this presentation that are very relevant to developing consequence models. How much waste material can be entrained by an erupting basaltic volcano, and how far across the countryside can this stuff be scattered? And those are the two areas that I'm going to be focusing on during the next couple of

minutes.

I've given this presentation to many different audiences and found that it is usually useful to try to give people an understanding of what is magma. And I realize these are not going to be the gnat's eyelash of reality, but it's a good starting point. This is the physical conditions of this material that is going to be impacting on the repository system.

You have temperatures of roughly about 1100 degrees C, densities of about 2,600 kilograms per cubic meter -- so you can put that on your desk as a really wonderful paperweight -- viscosities on the order of 10 to 100 plus pascal seconds. For comparison, I looked it up in the CRC handbook, sucrose is about 1000 pascal seconds at 100 degrees Centigrade.

So these are not the big, sticky magmas like you're going to see at day sites like at Mount St. Helen's, but really fairly fluid material. If we ever get to the process stage of how this impacts on a canister, we'll need to be accounting for viscosity effects.

But the magma, when it comes up into the engineered system, is going to have an assent velocity on the order of about a meter per second to start with. And then, as the eruption slowly builds up during the course of days, we can be having material flowing through the

system at about 100 meters per second during the eruption.

And all of that can be translated -- how much vesiculation you want into the actual forces being applied to the engineered system.

We are also evolving some fairly acidic gases at these temperatures, and we have every reason to believe that the degassing from the volcano will adversely affect canister performance in the repository setting, rather than enhancing performance.

A lot of these are secondary issues that I'm not going to be talking about today. We have lumped them under the term of "indirect effects." What we're primarily concerned with, and what the calculations I'll be presenting later are all about, is how much of the waste is actually getting entrained by the eruption and scattered out into the accessible environment. That's the key point.

The secondary effects of -- well, we have degassing, we have lava flows in the tunnels, or whatever scenario you wish, that's a separate analysis.

We really don't know what is going to happen to a waste package when it gets hit by basaltic magma. We have taken a very preliminary look at the thermal effects. Given these conditions that heat capacity and the physical characteristics of a waste package, it is not going to

last too long.

It is going to fail thermally on the order -- anywhere from seconds to about a year under these ambient temperature conditions. So we're starting from a base assumption that the waste package is going to fail under magmatic conditions.

We are certainly open to additional analysis that shows how robust or non-robust a waste package will be under these kinds of eruption conditions. But again, our starting assumption is that the package will fail during an igneous event.

We also don't know or don't have a very good sense of how the waste itself is going to behave during an eruption. We're dealing with essentially a pressed pellet that has been thermally and mechanically stressed, that has a density of around 10 grams per cubic centimeter.

We do know that it is fractured and that these originally about centimeter size fuel pellets will be broken down into finer fractions. There is considerable debate going on to what the grain size distribution of those fractions will be. But right now, we are starting off that the particle size is not going to be at the one centimeter size -- the intact pellet -- but the pellet will be fragmented into various smaller components.

And part of the calculations I'll present will

show different assumptions about if the waste is this diameter, with that 10 gram per cc density, this is how it will affect its transportability in magmatic eruption.

One other source of sort of the big unknowns or the uncertainties that we have right now is that we're very uncertain about the dispersal capabilities of most of the Yucca Mountain volcanoes. We don't have the tephra deposits preserved, except for some very faint remnants at Lathrop Wells. Everything else has been stripped away.

We look at the cinder cones, and we interpret how dispersive they could have been. You can go anywhere from essentially a small event at Northern Cone that probably had no distributed material, except some elutriated ash blown out of the fire fountain, all the way down to Lathrop Wells, which is 120-odd meters of totally non-consolidated broken scoria that had to have been produced from an eruption that had a sustained column on the order of kilometers.

But we can't get a probability distribution function from the limited information we have right now. So we're taking a modeling and analog approach to better understand how, potentially, these Yucca Mountain type basaltic volcanoes could transport material into the accessible environment.

The whole point of this is that in order to

get to these dose numbers, the risk numbers, and the significance that everybody seems to want, we have some data, we have some models, and we have a number of assumptions. And all of these have to be mixed together into some sort of a final answer right now.

We've talked in many different forums about analogs and what is and is not analogous to basaltic volcanism in the Yucca Mountain region. One of the volcanoes that we pay particular attention to is the 1975 basaltic eruption of Tolbachik volcano in Kamchatka that is, we believe, very relevant to understanding eruption processes for Yucca Mountain type volcanoes.

Both of these systems -- Yucca Mountain and the specific setting for Tolbachik -- occur in extensional tectonic settings, with no obvious evidence of shallow crustal reservoirs. They are basaltic, fairly non-evolved in the scheme of getting up into the basaltic range of things, derived from hydrous mantle lithosphere.

And I'm not here to argue petrogenesis, but really to talk about the eruption process. And in terms of process, what happened at Tolbachik is very analogous to what we would speculate has happened at volcanoes like Lathrop Wells. The Tolbachik eruption -- we had a northern breakout and a southern breakout. We're focusing on aspects of that eruption.

The northern breakout has -- the first cone has a volume of cone plus lava plus tephra that is identical to what Bruce Crowe and his co-workers have calculated for Lathrop Wells. In the Yucca Mountain region, we have volcano volumes that range for quaternary from about .06 to about .2 cubic kilometers, when you convert it all back down to magma volumes.

And then you go into the pliocene. That gets up above a cubic kilometer very easily. The total eruption at Tolbachik is about .45 cubic kilometers, so we're seeing, at the first pass, very comparable eruption volumes to compare the processes at Tolbachik with the processes we'd expect at future eruptions at Yucca Mountain.

And the whole thing is that we can look at Tolbachik, look at what has happened there, and try to understand the clues that we have at Lathrop Wells and other volcanoes in the Yucca Mountain region, and constrain beyond speculation but try to turn some of this into real data to constrain the process models for dose calculations.

The reason I'm harping about Tolbachik is that for 12 hours at the very end of the cone 1 eruption, that basaltic volcano dispersed roughly three million cubic meters of shallow subsurface rock up to the surface. We

have some pretty good geologic controls about what the subsurface structure is in Kamchatka, and in some specific area of Kamchatka, where we have roughly 800 meters or so of basaltic rock, then a range of uncertainty.

We can't say if the base of the basalts are 800 to about 1,300 meters, and then a dominantly sedimentary section. So we've got a real nice layer cake stratigraphy to look at. We have examined the xenoliths, the wall rock fragments that come out at cone 1, and also looked at another part of the deposit I'll speak of in just a second, to constrain this volume.

And using just simple geometric arguments on the proportion of volcanic to sedimentary rock at Tolbachik, with the total volume of material erupted, you can say that, as a minimum, the conduit in the late stage of this eruption widened to at least 37 meters in diameter, assuming a cylindrical geometry, down to a maximum, given this range of uncertainty, of around 60 meters in diameter.

Trying to get an estimate on the uncertainty gives us an average widening of the conduit to around 49, plus or minus seven. Let's just call it 50 meters in diameter -- is what happened during this eruption for the last 12 hours when the conduit went from about one to two meters in diameter and widened out to 50 meters in

diameter.

The reason this is important will become apparent in a second. Let me just say that we have to facies associated with this event. First, you have a fairly abundant amount of xenoliths. These wall rock fragments are scattered about on the surface of the cone.

But even with this extreme brecciation event occurring, you're still only seeing about one to two percent of the total surface area is covered with these wall rock fragments. So they're not hugely abundant. Twenty percent of the cone is just covered with pieces of the subsurface.

What's more important is that an ash layer was distributed for about 10 kilometers around the volcano that consists of finely pulverized subsurface rock. And this occurs anywhere from about a 25 centimeter thick down to a core vanishingly small thick, white ashy clay section that represents the bulk of this three million cubic meters of pulverized wall rock.

So it's not even so much what you're seeing on the cone itself, but the very late stage of the tephra fall is just blankéted with this white ash deposit. And just estimating, in the past 20 years I'd guess about 20 to 30 percent of that deposit has already been removed by erosion. It's just the same as kicking up, if it's dry,

it's fairly dusty, it's v^ery easy to remove this deposit.

There is one other key bit of geologic evidence associated with this brecciation event, and that is with these xenoliths, you can find a very unusual type of volcanic bomb. That is, a mixt''.e of different compositions of wall rock, each of which has a range of thermal history to it.

So you can find pieces of older basalt that are very intact -- you can't see any thermal effects to them whatsoever -- all the way down to ones that have a very nice quenched margin to disruption fracturing and a little bit of invasion to them.

And also, the sedimentary -- more importantly, the sedimentary xenoliths that you see within these bombs range from completely unaffected thermally -- something that was just thrown into this melt and quenched -- all the way to ones that you'd mistake for pumice, they are so melted and inflated during this brecciation event.

So what we believe these xenoliths are showing is a sampling of this brecciation, where we're entraining different depths or different distances away from the conduit during this brecciation event, taking pieces of the distal wall rock if you will, the part that isn't adjacent to the conduit, that hasn't been heated up and melted, and mixing those fairly cool pieces of the conduit

in -- or cool pieces of the wall rock in with the rock that is adjacent to the established magmatic conduit.

All of this is in matrix support and a quench basalt matrix. So this isn't an agglutination of layers through time, but rather a single instantaneous event that records this white ash process, this conduit widening process, at Tolbachik volcano.

Now, to tie in to why you're paying attention to this for Yucca Mountain region is that we find the same sorts of remnants at Lathrop Wells, and to a lesser extent at Little Black Peak. First of all, the proximal xenoliths, the xenoliths on the cone at Lathrop Wells, are unusually abundant.

When you go up to Lathrop Wells and the people that we've taken to that have a wide range of experience there in, say, the basin and range, you come up and you look at that and you go, "Wow. There are a lot of wall rock fragments, the xenolith fragments, sitting in Lathrop Wells' cone." Unusual for a basaltic volcano. The same sort of abundances occur at Little Black Peak.

More importantly, we find the same kind of polylithologic/polythermal bombs at Lathrop Wells that you find up here at Tolbachik.

These occur only on the southern flank of Lathrop Wells, at what we are to interpret would be the

last gasp out of the volcano. They are not distributed throughout the volcano. You can only find them in one very specific area, which is exactly what you'd expect if this was solely a late stage event.

So we're dealing with two volcanoes at Tolbachik and Lathrop wells where we have a similar eruptive volume, a similar sort of tectonic setting, coming up through a crustal section that has been disrupted in the upper kilometer or so.

And based on the distribution of lithologies for Lathrop Wells and the structural controls that we have available out there, we'd say that Lathrop Wells disrupted anywhere a crustal section, a dominantly tuffaceous crustal section, of about half a kilometer to two kilometers deep.

So, right now, we are thinking that the subsurface -- the late stage disruption at Lathrop Wells was comparable to what we have observed at Tolbachik, and thus we should be taking into consideration that late stage conduit widening under a future volcanic event may disrupt an area of 50 meters in diameter.

Given the average thermal loading on the repository of roughly 83 metric tons uranium per acre, depending on how you want to put your spacing of waste packages and how much waste is in a waste package, that

would translate to roughly four to 10 waste packages by widening the conduit to that 50 meters in diameter.

So when I talk a little bit later about the dose calculations, and we go from one waste package up to 10 waste packages, this is the geologic basis for that assumption.

MR. FOLAND: Britt, this is Lathrop Wells.

MR. HILL: Yes.

MR. FOLAND: Are you proposing that other volcanoes in the Yucca Mountain region were like Lathrop Wells?

MR. HILL: No, I'm not. As a matter of fact, there is little evidence to support that.

CHAIRMAN POMEROY: Did you say, though, Little Black Cone is --

MR. HILL: Little Black Peak has the same -- there is a few of the polylithologic bombs. It is quite a bit more eroded than Lathrop Wells. But the relative, to say, going out to Black Cone, Red Cone, or a number of other volcanoes that would be even less eroded in the Western Great Basin, there is a remarkable abundance of xenoliths there.

And still, it's not Lathrop Wells, it's not roughly one percent on the surface. But when you go out to most cinder cones in the western U.S., maybe every 10

square meters, 20 square meters or so you find something that is an obvious xenolith from shallow crustal, if you get lucky.

Ken, you've seen a lot of cinder cones out there. Would that be your observation?

MR. FOLAND: Yes, I think based on that.

MR. HILL: Well, when you go and you throw a one square meter square down on Lathrop Wells or Little Black Peak, and you start picking up 10, 15, 20 centimeter size and greater pieces of wall rock, that is unusual. And certainly, you don't find that on little cones or the quaternary Crater Flat cones. But you do see it at Little Black Peak.

I haven't done the statistics to say whether Hidden Cone has it or not, but Hidden Cone is contemporaneous with Little Black Peak. They're both about 350,000 years.

But Lathrop Wells is not isolated. There is elements of that, but the trend is kind of interesting. If you look at eruptive volume from pliocene, even through the quaternary, we're getting distinctly more explosive in every quantifiable measure that you can see through time.

So I think the trend would support that future volcanoes would be capable of Lathrop Wells' style eruptions, rather than Northern Cone or pliocene Crater

Flat.

MEMBER HINZE: Is there anything in the water content of these rocks tc differentiate Lathrop Wells and Little Black Cone?

MR. HILL: For all intents and purposes, from the preliminary work we have done, from the work that Frank Perry and other people in the Los Alamos program have gone through, there is no gross geochemical distinction between Lathrop Wells or anything in the quaternary out here.

There are subtle petrogenetic variations, but nothing to say that while Lathrop Wells is anomalous petrogenetically, relative to the other quaternary, they are extraordinarily similar. In most places, you'd call them identical, until you really started expanding your scale.

So we've got waste. We disrupted part of this repository. Where is it going to go? I just show these two plots to give a sense of scale for given the range of volumes that we see at Yucca Mountain, what would an analog volcano look like if it erupted through the repository.

We'd start off with Tolbachik. Again, about .45 cubic kilometers, a pretty good size quaternary event. We'd be having eruption through here. You can see our one

centimeter isopach would come down about 50 kilometers south. I have rotated the isopachs around as though the wind is blowing from north to south, because these black dots are at critical group locations, at 20, 25, and 30 kilometers south of the repository.

So we had a Tolbachik kind of event. This is one guess of what the isopachs would look like. You'd have roughly five centimeters to about three centimeters covering your critical group location. In contrast, if we had a very small eruption, something like Cerro Negro, that has a magmatic component of .008 cubic kilometers, which is about an order of magnitude smaller than the smallest event we've got preserved out at Yucca Mountain, this is what the isopachs would look like.

A one centimeter isopach would only go down about 12 or 14 kilometers south of the volcano. We wouldn't be getting more than a millimeter at 25 kilometers, and at 30 kilometers it would just be a dusting of ash. So we're factoring this in when we do our consequence models to sample a range of volumes that would give us this kind of a distribution to accommodate the scale of future eruptions.

Now, in order to calculate -- in performance assessment models, in order to calculate the thickness of ash, we have been using a model that was developed by

Suzuki and published back in 1983. I realize that there are a large number -- maybe not a large number -- but certainly a variety of tephra dispersion models that can be used to simulate the transport of material from a volcano.

Many of those models, however, are really developed for large silicic eruptions. That transport is predominantly in the fines component range, the ash component, down into the hundred micrometer or finer range. Also, for eruptions that go up to a level of neutral buoyancy and develop a very large umbrella cloud through them, that stagnate at very high altitude -- 12, 15 kilometers or so.

These are not characteristics of basaltic volcanoes. They tend to form fairly low eruption columns on the order of five kilometers or so, and get blown over fairly rapidly by the wind.

The Suzuki code gives us a better handle. It doesn't account for an umbrella cloud, and also it is a little bit better suited for modeling the course material rather than the distal fines. And we're really worried about this area -- 20 to 30 kilometers south of the volcano.

We are also examining the possibility of modifying this dispersion code using some of the work done

by Andy Woods. I think his model may be a better process model than the processes captured by Suzuki. But right now, we are going -- we are reviewing our preliminary calculations using the Suzuki model.

MEMBER HINZE: Do you have any plans to exercise those codes, bring them up, or any of the other codes otner than the Suzuki?

MR. HILL: We're evaluating Woods' model right now. It is too early to say whether it's going to be a superior product to what we're using with Suzuki or not.

I'm very comfortable with Suzuki. I understand where a lot of the limitations are. We have tested sensitivity of the model through a number of input parameters, and I talked about that in the annual report.

We have been evaluating this model using data collected from the 1995 eruption of Cerro Negro, a very small volume basaltic volcano down in Nicaragua. The reason we used that is that we collected the data and have all of the parameters we need to evaluate the model. If you go into the literature for basaltic eruptions, you'll see that eruption duration is usually well represented. But nobody talks about the time that the eruption actually sustained a column.

Most of these basaltic eruptions have an early phase where there is very little convective activity, and

then a main tephra dispersing phase that's usually a smaller portion of the total eruption, nor are wind speeds and wind directions ever really mentioned. It is very difficult to get a robust data set that you can actually test for a basaltic volcano, but we developed that data set specifically from Cerro Negro in order to evaluate these tephra dispersion models.

So it's one where we have a high confidence in the quality of the data set used for accuracy, but wouldn't presume to say that this validates the Suzuki model. It's a reasonable test, and let's leave it at that.

What we found is that the Suzuki model is very sensitive to wind speed. If we use the wind speeds that we observed during the '95 Cerro Negro eruption, we underestimate by about 50 percent the deposit thickness in the 20 to 30 kilometer range.

Now, this is the difference between performance assessment and geology. When we first came up with these numbers, it was, "Oh, boy, you know, we're underestimating by about 50 percent." And when I talked to the performance assessment people, it's, "Wow, we're within 50 percent. This is a fantastic model."

(Laughter.)

Really.

(Laughter.)

VICE CHAIRMAN GARRICK: If they're within 500 percent, we'll be happy.

(Laughter.)

MR. HILL: We also found, in addition to wind speed, there is a moderate sensitivity to the column height, particle diameter, and particle density. But we feel we can constrain column height reasonably well based on mass flow and total eruption volume. Particle diameter and particle density you can only vary within certain reasonable limits.

Other parameters, such as shape parameters, sorting parameters, really don't have a significant effect on the deposit thickness in the range of distances that we're trying to consider.

So the bottom line is we have evaluated Suzuki. We feel we are within about 50 percent of the thicknesses of 20 to 30 kilometers, and that that gives us a good basis to evaluate, from a more probabilistic sense, the risk associated with dispersion of basaltic volcanoes.

And we have folded all of that into our total performance assessment code, version 3. The module is called ASHPLUME. And that is based on the Suzuki model, and we're using that to sample the range of input parameters more stochastically, such as wind speed,

volcano volume, eruption duration, to get a sense of the dose numbers that I'll present in a little bit.

So are there any questions on the approach we're taking or the reasons we're taking this approach for tephra dispersion or for subsurface entrainment?

MEMBER HINZE: Why don't you go on and let's see what we pick up as --

MR. HILL: Okay. I'd have Tim come on up and talk for a few minutes about the non-disturbed repository performance, to give us a context for the volcanic doses that I'll be talking about afterwards.

MR. McCARTIN: Okay. As Britt said, I will give a slight introduction to actually the next portion of Britt's talk, and that is to talk about some preliminary dose calculations that have been done for the total system to put the doses that Britt will be presenting for just volcanism into perspective in terms of the overall system performance.

And very briefly, I think it is important to understand the doses I will be presenting. I will focus just in fairly short order, really, on TSPA '95 and some NRC staff calculations that we did in support of our evaluation of the NAS recommendations. And it is important to understand why these calculations were done.

Certainly, I'm not trying to denigrate the

calculations. They were done certainly using the best information we had at the time, but the purpose for the calculation I think needs to be kept in mind when you see what the doses represent. And for TSPA '95, they were coming to closure with their TSPA results as the NAS recommendations were coming out.

They included a peak dose calculation at five kilometers, but it was done at the very end of the study, and so you certainly don't want to interpret these results as the best that they would do if they did the calculation today.

Likewise, for the NRC calculations, the NAS recommendations came out. We quickly went to do some calculations to see, in terms of the peak dose and a reference biosphere critical group, looking at a million years, what did that mean in terms of implementation. We certainly used parameters and models that we felt were appropriate.

However, we did not go into any great detail to define the critical group, parameters for that critical group, etcetera, but trying to see the mechanics of actually doing the calculation out to a million years, were there any special kinds of pitfalls that we wanted to discuss with EPA in development of the standard and in terms of consideration for our own regulation.

CHAIRMAN POMEROY: Tim, as you go through, could you just -- I don't want to stop you now, except to say when you're talking about this, can you indicate any point where you've used very non-conservative values versus upper bound values, so we can perhaps get a clearer understanding of where --

MR. McCARTIN: Sure.

CHAIRMAN POMEROY: -- these --

MR. McCARTIN: Yes. I'll try to point out a few areas where -- yes.

CHAIRMAN POMEROY: Thanks, Tim.

MR. McCARTIN: First, the TSPA '95 results, once again, it was undisturbed performance only. Volcanism was not considered quantitatively, although other TSPA efforts did consider volcanism. They looked at a drinking water dose at five kilometers. That was something very quick for them to do.

And, once again, we would suggest that, you know, it was done because they were calculating doses to the old standard, which had a five kilometer compliance boundary. There wasn't any look to see, well, where is the critical group, certainly lifestyle of a critical group, etcetera. And so it was done at the end of their particular exercise, and there really wasn't time to go to any of the details that are important in a dose

calculation.

But in terms of the TSPA '95 results, anyone familiar with the report knows there are many, many different plots in that. I selected one. I'm not trying to suggest this is the most typical, but you can see the doses range anywhere from the microrem out to around 10 millirem. And I guess one thing to bear in mind, that it was a 10,000 year calculation and it was cut off.

They did have calculations in there for longer time periods. However, the down side with the longer time periods was it was a point value calculation. It was basically one -- what they called an expected value, where they used the mean values of all of the parameters. It was just one realization.

And generally, we're more comfortable presenting results where you can see the spread of how the dose varied with the variation in parameters. But that was, once again, the five kilometer drinking water only dose.

CHAIRMAN POMEROY: So here's the 10,000. This is cut off at 10,000 years, though.

MR. McCARTIN: Yes.

CHAIRMAN POMEROY: So is there a -- I guess the question is: is there a significant dose immediately beyond --

MR. McCARTIN: If you look at the other calculation they did to dose, they did show some million year calculations. But, once again, it was a single realization. It would suggest that the doses at the high end might go as high as, let's say, 100 millirem for a perspective.

But it is always dangerous, in my mind, to select a point value calculation. It is hard to get the perspective of where it sits when all of these things are varying, but --

MEMBER HINZE: We're with you 100 percent.

MR. McCARTIN: In evaluation of the NAS recommendations, we did, as you know, some calculations for our deliberations with EPA. We analyzed the nominal base case, which we're assuming the probability was near unity. Peak dose, we went out to a million years. We looked at a couple of different compliance points. One, obviously, for traditional aspects is at five kilometers, and we looked at a drinking water pathway only.

We also looked at the dose at 30 kilometers. That considered all pathways. That is more like the Amargosa Desert area, where you would potentially have a farmer and have the other ingestion pathways.

And we tried to use the average member of the critical group approach. I will say we used sort of a

representative person in the population. I don't think we went into any great detail in terms of looking at what is the lifestyle of this average member of a critical group, but we used a representative person. And as you know, we're doing a lot of thinking, soul searching, as to what that lifestyle characteristic should be.

And so the calculation we have is sort of a population person average, if you will. It is still an annual individual dose, but we sort of averaged all of the characteristics in the region into this one person. Is that appropriate for the average member of the critical group? That is yet to be determined.

We also looked at volcanism and human intrusion, which was analyzed separately. For volcanism, we looked at a 20 to 30 kilometer pathway, once again, using that same average person if you will. And we were looking at the ingestion pathways for a farmer at that 20 to 30 kilometer location.

Once again, what did our doses look like? And you can see for the two -- this is the CCDF of our peak dose out to a million years for both the Amaragosa Desert and the drinking water. And you can see doses vary. They are around -- at the Amargosa Desert, the high end is maximized out to about 100 millirem. It is about a rem at the drinking water location.

Now, once again, I caution everyone to realize that there was not a lot of work done to determine what the appropriate characteristics of the five kilometer location, as well as the 30 kilometer location, in terms of dilution, which is very important, and the characteristics in lifestyle.

Now, the characteristics in lifestyle, certainly for Amargosa Desert, is more important than at five kilometers where we were doing merely a drinking water dose only. But, once again, we made some simple assumptions for dilution, etcetera.

MEMBER HINZE: What did you use for your model for the dispersion of this? What did you use for the dispersal of the ash? What did you use for this?

MR. McCARTIN: Actually, Britt Hill will talk --

MR. HILL: These are for undisturbed only.

MR. McCARTIN: Yes. I'm sorry. This is for the nominal base case. This is not --

MEMBER HINZE: Okay. Fine.

MR. McCARTIN: -- undisturbed, if you will. I'm just trying to give the base case, and then Britt will talk through the volcanism calculation. But this is more or less to give some context for the doses that you'll present.

In terms of how they compare, you can see looking through the different numbers, if we look at the median and mean doses, you can see sort of where they sit numerically. And the range was, as noted before, it varied from one to around at the five kilometer drinking water dose only. Very dependent, however, on certainly assumptions made with respect to dilution and what that group did at five kilometers. Likewise, .2 to 118 at the 30 kilometer location.

You know, I guess a couple of the big differences that might be noteworthy, although we're considering all of the pathways here, not just drinking water as here. Part of the reason I believe the doses are lower are you get a little more dilution in terms of at the Amargosa Desert location, but also you have alluvium, which you can take credit, if you will, for some retardation in the alluvium.

Generally, we have taken no credit for retardation within fractures. At five kilometers, you are still in a fractured rock regime with the plume, and so there is a couple of reasons there for the differences in the doses.

VICE CHAIRMAN GARRICK: Tim, what was the infiltration rate for this?

MR. McCARTIN: Oh. Yes, that's a good point.

For these -- well, we varied them, and we used, I'll say -- it was about a year ago, but generally, our range has been one to five millirem -- or five millimeters per year. These calculations were done prior to some of the later DOE estimates. That might suggest a slightly higher range, you know, as high as 10 millimeters per year. But these we were varying between one and five.

VICE CHAIRMAN GARRICK: And that number seems to be going up a little?

MR. McCARTIN: Right.

VICE CHAIRMAN GARRICK: Yes.

MR. McCARTIN: Right. Yes. Although a lot depends -- you know, there is many parts of the modeling that we're looking at closer, certainly with respect to the phase 3 modeling, in terms of the way water will get into the drift, how much will it impact the waste package. This model was using our phase 2 approach, a little more simplistic.

But with that, I guess the main thing to leave you with is what the dose -- the median doses, mean doses are, and then Britt will -- which is more the purpose -- talk through the volcanism results in more detail.

MEMBER HINZE: Thank you very much, Tim.

And, Britt, we have a handout from you?

MR. HILL: Yes. It's the second part of the

one you have.

CHAIRMAN POMEROY: And, Britt, while you're being wired up, can I just say that it's even more important, so that we don't have to go through the discussion that you and Kevin had at the technical exchange, that when you go through the model itself, can you indicate where there are bounding assumptions, where there are reasonably conservative consumptions, and where there are non-conservative consumptions?

MR. HILL: Sure.

CHAIRMAN POMEROY: Assumptions, rather.

MR. HILL: Thank you for the lead in.

Basic assumptions -- we're assuming the volcanic eruption occurs through the repository anywhere from 200 to 10,000 years after closure. This way we're not getting the first couple of hundred years where we've got a fairly high buildup to worry about.

The base assumption, based on some of the arguments I outlined earlier, is that one canister has failed and that all waste, about 10 metric tons, is available for magmatic transport. When we are doing our total system assessment, we are varying the area so that we are having smaller amounts of waste being considered. But again, as a base assumption, we say one canister has failed.

We know from the site data that wind is blowing to the south 14 percent of the time. That has been factored in for our critical group locations in the doses that we're calculating. We're saying the critical group is located 20 to 30 kilometers south of the repository. We have not calculated any five kilometer doses. Let's just leave it at that.

And we're also using the current dose conversion factors that were used in the dose calculations for the NAS recommendations, which are also the ones that are being used in the current version of our TPA code. These calculations were run last year, not using the complete TPA code, but were using the modules that are incorporated into TPA. So these numbers would be reproduced if you ran TPA right now using these input parameters and assumptions.

Some of the areas where we --

MEMBER HORNBERGER: Excuse me, Britt. Is the dose primarily inhalation?

MR. HILL: The dose is driven by ingestion through plant uptake and meat uptake. There is an inhalation dose. One of the areas that we're doing some thinking about is the resuspension factor for volcanic ash.

Right now, we're using a resuspension factor.

I think there are five available. The closest --

MEMBER HINZE: What is a resuspension factor?

MR. HILL: It's on the ground and it gets kicked up. We're using one for sand, and that is not very accurate. If you've ever walked across a sand dune versus volcanic ash, you know that you get a lot more volcanic ash in the nose than you would on sand with medium grain size particles.

We're thinking about that and doing some sensitivity -- will be doing some sensitivity studies to see whether the inhalation dose is going to be critical.

For five kilometers in, we'd, of course, be looking at inhalation and ground shine as the pathways, in addition to ground water from the other release.

Paul, you were asking about critical processes and assumptions. Well, here they really are. First, the behavior of the ascending of the magma in the disturbed geologic setting. We would like to do some better process modeling to really understand what happens when this ascending dike hits the drifts at 300 meters below the subsurface.

We have taken primarily an analog approach to start with, and we have been asking around. Nobody has been able to come up with a good 300 meter tunnel down there that a basaltic volcano of continental type has come

up through. So our base assumption on this is it is behaving just like the undisturbed geologic setting in terms of wall rock entrainment. These are the basic assumptions that the DOE has used as well.

We have a big unknown on canister response to ascending magma. That is one of the areas where we are doing more thought. I try to evaluate the available information. But, quite frankly, igneous activity was not a design criteria, and it hasn't really been addressed for what happens under these mass thermal and chemical loads that an igneous event would place on a canister relative to the normal response from the ambient environment.

So, right now, we feel we have a good justification for making the assumption the canister will fail as a reasonable and conservative measure to do these calculations.

The waste particle size distribution -- since we're dealing with transport of dense materials subaerially, how big that material is is going to critically impact the dose that you get from that material. We're starting off with waste pellets that are roughly centimeter size that will broken down into smaller size fractions.

The calculations I'm presenting will show three different size ranges, a median diameter of 10

millimeters, one millimeter, and 10 microns. And we'll evaluate dose based on those ranges. That range is based on literature information and evaluation of the best information we have on the fractured nature of these particles.

Right now, based on some work that we've uncovered from the NRC's NUREGs coming out from the reactor program people, we would skew our bias down -- skew our bias? That's wonderful.

(Laughter.)

We'd say that the finer grain fraction is more probable than the one centimeter fraction, would be more supported by the available information.

Again, we're kind of in the dark about --

MEMBER HINZE: What is coarse here? The Suzuki model is better for coarse textured materials? Did I understand that?

MR. HILL: The Suzuki model accommodates all range from I think it's --

MEMBER HINZE: Okay.

MR. HILL: -- was it 15 microns or so?

MR. CONNOR: About, yes.

MR. HILL: 15 microns in diameter on up to -- we're cutting it off at 10 centimeters in diameter from grain size distribution. It is really -- you're looking

at gravitational settling.

MEMBER HINZE: Okay. The validity of it is not greater in one size range than in another?

MR. HILL: No. No.

MEMBER HINZE: Okay.

MR. HILL: It's not that.

We're looking at how is the waste actually affected by the ascending magma, whether it's incorporated into the particles or operates as discrete particles. How it's actually being entrained, again, is a big unknown. We don't have volcanic rocks coming up through dense friable material. But we're making the assumption that the waste is adhering to the tephra particles, and that the particles have to be a certain diameter greater than the waste.

So when we say it's a 10X incorporation ratio, that means the particle, the tephra particle has to be 10 times the size of the waste particle in order to transport that waste particle into the accessible environment.

This gives us a transport cutoff. If we have a very fine grain eruption, it just haven't the transportability to move this material down range. We're hoping to get a better process handle on that, if it turns out that this is critical. But right now, we feel this is a good starting assumption, that the waste is being

transported.

And finally, the dispersal capability of the volcanoes is like I had outlined previously from analog volcanoes and is effectively modeled using the Suzuki code. We have the uncertainty of about 50 percent. But given the range of numbers that I'll show, and the range of uncertainty in the entire total system assessment, that is deemed accessible by everybody I talk to, really, except those pesky geologists.

Okay. For each one of these dose calculations, we did 300 simulations with one canister, 10 metric tons available for transport. We're getting a mean annual peak dose, and the second number is the standard deviation about those realizations. The reason things are a little odd is you end up with log normal distributions.

We're sampling stochastically the eruption power and duration. Simply put, we're accommodating for column height, eruption rate, and total mass of the system. So we're varying those parameters between what we're seeing for Yucca Mountain volcanoes.

The eruption rate, of course, we don't know, but we can make reasonable assumptions from analog volcanoes about, given this volume, how long the eruption could reasonably go on, and control column heights in that

roughly five kilometer column height range.

We're also varying a little bit the shape of the column, whether it's sort of getting blown over or is mushrooming out. It turns out that is not very sensitive, but we're sampling it stochastically anyway. Time of the eruption, which is going to be mass controlled, wind speed, and tephra diameter.

The real bullet is that the grain size of the waste, as you might have guessed, is a critical assumption for determining dose. If we have an average diameter of the waste at 10 microns, with a range of waste particles plus or minus one log unit, so we're sampling that from one micron up to 100 microns, and the incorporation factor is the tephra must be twice the size of the waste in order to transport, at 20 kilometers we're having a dose of 50 millirems per year. If you move to 30 kilometers, that drops down to seven millirems per year.

And you can see that the incorporation ratio for fine waste grain sizes really doesn't matter too much. You're getting a very small variation.

So if we say the waste is fine, we don't really have to worry too much about the transport mechanism relative to the tephra. It's when we get down -- you jump all the way down to the bottom, about saying that it is one centimeter in diameter, then, of course,

we're getting a cutoff in the amount of material that can be transported quite dramatically.

You can see at 20 kilometers we're about 10^{-3} millirems per year at two times incorporation, but you notice how much that decreases when you go to a 10 times incorporation ratio, just because there isn't that much large material in a basaltic eruption. So we're getting, you know, down to -- I've taken some flack for that, but it's essentially an incalculable dose, rather than saying zero for there.

All of these, again, are for one waste package, for the event within that specific timeframe.

MR. RYAN: Britt, the waste density, again, in grams per cc was?

MR. HILL: Roughly, 10 grams per cc.

MR. RYAN: That's what I thought I had heard earlier.

MEMBER HINZE: How did the sand dunes form out there?

MR. HILL: They are Eolian, blown by the wind.

MEMBER HINZE: Do you think that might happen to this tephra?

MR. HILL: Very easy to imagine that tephra is going to be redistributed through sheet-wash processes as well as Eolian processes out there.

MEMBER HINZE: Might be concentrated in --

MR. HILL: Certain areas, it is definitely going to be eroded, even though we have all come to agree that there is not much of an erosion rate out in the Yucca Mountain region.

(Laughter.)

We also have come to understand that Lathrop Wells, if the tephra sheet is 100,000 years old, it is completely stripped. So we wouldn't expect to have unconsolidated tephra centimeters to 10 decimeters thick remaining around on the countryside.

We are evaluating that. We are doing some initial calculations. If we had small volume eruptions that dispersed materials, say, 10 kilometers or so to the south, how much of that mass would be washed down into the 40 mile wash drainage, and potentially transported into the accessible or the critical group locations 30 kilometers or so down range, and trying to get a handle on whether we need a module and a detailed conceptual model for remobilization of ash or whether, really, the significant process is the direct dispersal rather than remobilization factors.

MEMBER HINZE: Our friend Tim just talked about the frightening experiences you can have with spot calculations in performance assessment. I was wondering

if you've calculated this at 21 kilometers. Do you have any idea about gradients? Do you --

MR. HILL: Well, that's the reason we did. Even though we don't have an explicit standard, we're pretty much getting towards the 30 kilometer standard. But we did the 20 kilometer critical group location to give a sense of spatial variation given the variance in these parameters. It's not simply linear, but it falls somewhere -- at 25 kilometers, somewhere roughly between these five and seven, for example.

CHAIRMAN POMEROY: But it's not linear, did you say? Is that correct? It's not linear?

MR. HILL: Yes. You wouldn't just make a two-point line and go back to the repository. First of all, because the critical group assumptions on the ingestion pathway, we'd have to go back from the non-farm input, and that depends, well, where north of highway 95 you say there's going to be no farming. Those are for people other than I.

MR. CONNOR: Well, the ash blankets also have basically an exponential decay with distance from the vent, so just the ash itself is non-linear. And, of course, any incorporation is going to create non-linear on top of that, so it's very non-linear.

MR. HILL: Yes. I'd caution people not to

extrapolate forward or backwards from these dose points.

MEMBER HINZE: Is there any indication that these models -- the Suzuki, or Andy Woods, or whoever -- has a constant percentage of error as you go away from the source?

MR. HILL: Well, the Suzuki model has been reasonably tested a couple of times in the literature. Lori Glaze and Steve Self, for example, modeled the -- was it the '81 Lascar eruption in Chile using Suzuki. It has also been applied to St. Helens and found to be a pretty good indicator of these larger volume silicic eruptions.

I'm not sure -- this is something we've discussed internally quite a bit, why are we getting the underestimation in Suzuki? Is it something inherently different about basaltic eruptions, or just conceptually at a very fine scale, because again, we're using very low column heights.

Our mis-match is at the distal end. The proximal end works really well with Suzuki. So it's almost like material is falling out too soon, or something is not quite working right that for a large eruption, you don't catch the error for a large fine grain eruption, you don't catch that, but just down to these sensitivities, the limitations of the model become apparent.

But again, there has been nothing in the

literature to indica this is a real problem. I don't think it's a real problem for the calculations we're presenting right now.

MR. RYAN: Were your calculations using a five kilometer column height in this case?

MR. HILL: No. The calculations are -- the column height is sampled based on our eruption power and duration. Volume and duration based on historical basaltic eruptions. So that feeds back into some of the empirical relations that Walker and Wilson have developed.

MR. RYAN: Have you used, observed basaltic eruption column heights as guides?

MR. HILL: Yes. I have gone back and dug through the literature on I believe it was 11 historical basaltic eruptions that we feel are analogous in that they have sustained convective columns that have observed column heights. I think it was 10 out of 11 matched extraordinarily well. One of them I believe is the 1968 Cerro Negro eruption. The duration of that eruption was very long compared to the ash blanket produced. I think it's just a reporting error that the actual time of temper dispersion was very short, about a week, relative to the months of eruption that are reported in the literature.

MR. RYAN: Your isopach maps that you showed for Cerro Negro and Tolbachik showed a piling up around

the vent.

MR. HILL: Sure.

MR. RYAN: Do you always find that? Let me back up just a little bit and mention that as we all remember, one of the remarkable things about the 1890, 1980 Mount St. Helens activity was that in fact the bulls-eye was not near the volcano itself, but was at Ritzville several tons of kilometers downstream. So that the plume mass distribution from St. Helens was very much like taking the garden hose and sticking it vertically in a very strong ambient wind. You had lots of mass that was reaching out far away from the volcano.

MR. HILL: Yes. This gets again, into my understanding of why we're getting that secondary fallout so far down range for St. Helens was an agglutination process and an adhesion process of these very fine statically charged ash particles in a wet environment, which is something we would not be worrying about for the basaltic tephra, which is predominantly not ash-sized and coarser.

We can and have varied this. In the Suzuki model, there's a beta factor that affects column height, or excuse me, column shape, which can give you that secondary fallout.

MR. RYAN: Spreading around the trajectory.

MR. HILL: A little bit more.

MR. RYAN: On either side of it.

MR. HILL: Yes. We vary that within all reasonable ranges. That doesn't accommodate the variant with the deviation from observed deposit thicknesses for Cerro Negro.

The first part of your question is not even so much St. Helens, but most basaltic volcanos show a two-slope line in thickness versus distance. Within the first five kilometers or so, you have got a fairly steep fall off. Then you get around five clicks, and things level out. It's not truly exponential, but it's alike a two-part exponential trend.

Tolbachik doesn't show that except the proximal data within a couple of kilometers we don't know how thick it is because it's tens of meters thick, and nobody has done ballant. But I suspect it's a similar sort of process.

MR. RYAN: You mentioned that the wind was blowing, was it southerly 14 percent of the time?

MR. HILL: Yes.

MR. RYAN: In your assumptions. What's the other 86 percent of the time? What's the azimuthal variability?

MR. HILL: It's the southern quadrant of I

think it's 15 degrees sector that encompasses from the repository if north is up. That's where the wind is blowing in that quadrant 14 percent of time. I think it's not a uniform distribution. It's skewed more towards the north, but other points of the compass are fairly well represented. We have the data for that.

But again, it is important to remember we are calculating dose at a specific point, not over an area. That point is strategically located at that specific place. So we have to when we go through the dose calculations to get the risk, we have got to account for the probability of occurrence of different processes.

MR. TRAPP: Britt?

MR. HILL: Yes.

MR. TRAPP: Just one thing. It kind of goes a little bit to Bill's question. Going through the Suzuki model and comparing it to the distribution of known ash blankets, we appear to have a fairly good correlation. There's a little bit like you said, 50 percent under estimated.

What we are doing right now is assuming the waste basically is following proportional according to these type of factors, the same thing as the ash. Now because of the difference in the particle size, the particle weight, et cetera, all this other kind of thing,

it's something we really haven't looked at in total detail. It's something we need to take a look at. It will be carried forward in further work.

MEMBER HINZE: John, that really gets to a question that I had of Britt and perhaps you. What are the weak points here, and how are you going to go about solving them? It seems to me that one of them is the diameter of the particles, but the other is the incorporation. How do you solve those problems?

MR. TRAPP: Let me try one thing that we I know are going to try. This may end up getting there. If you assume, in this case I'm talking really assume some of the worst case estimates, assuming the extreme small particle size and not this case, the 10 millimeter, but talk about the one micron size.

Assume that this stuff is uniformly distributed through the ash blanket. Take a look at some of these analyses, find out what the answers are. If the answers by the time we get done with them and give them to the performance assessment people and then the people who decide whether they like performance assessment or not, if these don't bother them then why worry about it any more.

If the numbers do get to be high enough that we do have to start considering it, then we need to analyze those factors much more critically. So yes, we

will be doing more sensitivity studies and this type of stuff to try to figure out where we have got to sharpen our pencils.

A similar question is this is for one canister, so we're talking about four to 10 times, if you have another Tolbachik. Right?

MR. HILL: Right. And that's where we get to the final slide.

MR. FOLAND: Britt, one final question. Did you actually modify the particle densities?

MR. HILL: Yes.

MR. FOLAND: In this code for your --

MR. HILL: To accommodate for the mass of the waste, yes.

MR. FOLAND: What you said confused me, that it wasn't incorporated.

MR. HILL: Okay. It is in there.

MR. RYAN: Just curious. Sorry to interrupt. What is the diameter of these particles, the waste particles?

MR. HILL: Of the waste particles?

MR. RYAN: Yes.

MR. HILL: Well, that is what we were varying. It could be anywhere from a starting assumption of one centimeter in diameter all the way down to 10 microns in

diameter for the median diameter.

CHAIRMAN POMEROY: One other just comment. After the technical exchange, a few of us were very interested of course in that rate of change of those as a function of distance, if you will. If there are any possibility of getting more information from these models, even though you do have to change the parameters that go into them as a function of distance away from the repository. We're not absolutely sure where EPA is going to end up with this. It would really be nice to know something about that sensitivity, given these assumptions and calculations.

MR. HILL: I think we need clear guidance on that, rather than something that we would go doing off on ourselves because there is a great sensitivity to calculating any number outside of 30 kilometers.

CHAIRMAN POMEROY: I am aware of that.

MR. HILL: I am saying we have the ability to do it. It's a policy decision on whether or not we do that. But we can implement it if we would like to.

CHAIRMAN POMEROY: Very good. Thank you.

MR. HILL: So everybody likes CCDFs. I was presenting mean values before. Of course you wonder is the mean conservative or would you even put that term to a mean. For these dose calculations, I think the mean is by

any objective measure, reasonably conservative. Let's forget the exceedance probability for just a moment and just look at the base case of volcanism, where it does occur.

Probability is one here, and we're dropping down on our CCDF. The mean, as represented, leave off the zero for just a second. From our previous example of 50 millirems per year, the mean represents the 94th percentile of that data. So it's hard to see how the 94th percentile as your mean is not a conservative measure of the dose that we're expecting here. So that's the point of the first.

VICE CHAIRMAN GARRICK: So this means that your PDF's are very skewed. If this were made up of a series of scenarios, if you had represented this as a family of curves and a cut curve across the CCDF would indicate a highly skewed PDF.

MR. HILL: Yes. You have got a source of anxiety down at this region that's driving your mean. You do have large -- sometimes you are going to get a large dose out there. I'm not saying risk, I'm saying the dose can sometimes be troubling. That's just what the distribution is saying. That there are certain scenarios where you are not going to of course get much of a dose, but there are going to be ones that are going to give you

a large effective dose out there.

I would like to emphasize that these calculations are not what I would characterize as worst case. These are reasonable bounds on our understanding of the processes at this time. We have not gone out to try to find worst case scenarios to evaluate at this stage. It's calculations presenting a range of understanding, if you will. Not a worst case scenario. I would urge people to not present these as the upper bound of the scenario. It's the upper bound of the current parameters.

CHAIRMAN POMEROY: I was hoping maybe we couldn't go into that, but since you brought it up, you are talking about 10 canisters.

MR. HILL: Yes.

CHAIRMAN POMEROY: Do you think that might be 100 canisters?

MR. HILL: No. I don't.

CHAIRMAN POMEROY: Thank you. What are you talking about in terms of a volcanic volcanism probability number?

MR. HILL: Ten to the minus three on 10,000 years or 10 to the minus seven on an annual basis.

CHAIRMAN POMEROY: It's not the upper bound of your --

MR. HILL: Of our current models and current

understanding, I am comfortable with that range as the upper limit. I do need to acknowledge that there are other models in the reviewed literature that would get an order of magnitude greater than that, or that there are some other processes that we need to be considering about spatial shifts through time on a regular fashion that may impact that number.

But based on the recurrence rate that we have out there in the quaternary, it's hard to see that getting up into that 10 to the minus six range.

CHAIRMAN POMEROY: Of the other pieces that are built into this, as listed over there on the lefthand side on the bottom, are there ones that you can clearly identify as not being upper bound?

MR. HILL: Waste grain size?

CHAIRMAN POMEROY: Yes.

MR. HILL: Dose point, the closer in, and depending on your scenarios, that's going to affect the dose quite a bit, as we're all aware.

Depending on are we talking about a single dose point or a range of dose points, it gets back into the wind and the distribution on that. These are more of standards that we don't control. But I think you are going to see a large difference if I said 50 waste packages versus 10 waste packages. That's not 100.

That's not an order of magnitude. It's only 50 percent, depending on which side of the equation you are operating on. But I think that would start giving you significant increase in dose.

MR. TRAPP: Britt, on your previous slide, there was a very important point brought up. I think it kind of addresses some of this, or maybe it wasn't one of the previous ones.

Anyway, the point is that we are making a basic assumption in all these models that we are dealing with what we call an undisturbed case or a case where the repository isn't there. We're not totally sure what the effect of the stress reduction, et cetera, this type of thing will have to the possibility of any magma going through there. You can hypothesize both ways. One, that it will basically fizzle out in the repository horizon. The other, that you end up with kind of a coke bottle effect. You shake the thing up and blow the top off the whole thing.

We don't know right now. It's an area that we would like to take a little bit more look at and find out if it really does effect any of these numbers.

MR. RYAN: One of the things that happens, depending on whether or not -- depending on the level of the heat capacity of an object that's potentially going to

be encased by basaltic lava, depending on its heat capacity and its volume, and the amount of gas say is within that, is that if the heat capacity is sufficiently high and if the volume is sufficiently large, and if the included gas phase is sufficiently small in volume, that the enclosing basalt will in fact chill a rind around that object, very much like the streamlining on a volcanic bomb, or in fact the so-called tree molds that are often observed in the field which are passive markers of points of prior chilling of basalt.

Once chilled, of course, the rock is a very poor conductor, and can be rather insulating. So I think what I am saying is it's by no means a foregone conclusion in my own mind that contact of a waste canister by basalt mandates a rupture of that canister and mandates a release of the radioactive waste contained within it into some ambient flowing basalt stream. This may well happen, but I'm not completely convinced it's a guaranteed fait accompli.

MR. HILL: I share your sentiment exactly, that I think we have a very poor mechanistic understanding. We do not have I should say a mechanistic understanding of what happens with magma heating the waste package. Certainly to have it in contact with magma, versus putting it into the eruption column or into the

widening the vent and in training it during one of these brecciation events though, may be a very different scenario to worry about.

I would say that just given the mass and the load, the physical load that you are going to put on it during that process, it's hard to see it not failing. So I would agree with you that the dynamics of contact of the magma and the small conduit during initial ambient or initial ambient flow may well not be thoroughly fracturing the canister. But when we get to the later stages of the eruption, they may well be weakened, certainly by that stress, if not breached by it.

Then we put this additional mechanical stress on a thermally stressed system, that's one of the basis, why we are saying our starting assumption. I want to emphasize that assumption, is that the canister has failed. That's to us reasonably conservative. If we can come up with a mechanistic basis that shows like I said, the canisters are robust against certain or all eruption conditions, then we have got a solid basis for evaluating performance models. We don't have that, it's not coming out of the canister engineering groups. So we have to go forward with an assumption that isn't going to under estimate risk.

MEMBER HINZE: What's the quality between one

and ten of the communication between your modeling and what DOE is doing in terms of modeling?

MR. HILL: After the technical exchange, it sounded like it was pretty good. DOE wanted to use the ash plume modeling to better simulate the dispersal of material. Before with the older standards, we weren't concerned about a critical group or really dispersing it. It was waste on the ground constitutes the basis for doing the dose. We didn't have to worry about dispersion.

It sounds like there has been an acknowledgement that there are different ways to approach incorporation ratio, what the sub-surface area disruption might be. Greg Valentine has one approach. I have a different approach based on the way I am looking at basaltic volcanos.

MEMBER HINZE: You have got a 50 meter radius?

MR. HILL: Fifty meter diameter.

MEMBER HINZE: Diameter, 50 meter diameter. What does Greg have?

MR. HILL: He is looking at a percentage of entrainment based on work he has done at the Lucero field. There's a Journal of Geology paper by Valentine and Groves last year. When I last talked with Greg at the IAVCEI meeting, that's pretty much what's in the synthesis report, is the bulk of the work that was done out in the

Lucero field.

So he's looking at a stratigraphic section out there for xenoliths in a spatter cone, by his own admission in a spatter cone, and using that to say well, given that we have a 10 meter thick repository interval versus a crustal section of X kilometers being sampled. Therefore, that's the percentage of material that's available for transport.

Then he uses the average thermal load per acre, I believe, to fold that into the amount of material for dispersion.

MEMBER HINZE: So there's no problem in data communication between you and DOE in terms of canister or canister strengths or alterations?

MR. HILL: I wouldn't be presumptuous on that.

MR. TRAPP: I would say there's no problem, because really in that area it's an area that there hasn't been communication.

MR. HILL: Quite frankly, previous TSPAs have always assumed that if magma touches a canister, it's failed. So there hasn't been a disagreement on that point.

MEMBER HINZE: One of the problems that we have had that apparently has been present in the past is this communication problem. I am trying to see whether

that -- you know, we have really been pleased with the improvements in that. I am just curious as to how it is continuing.

MR. HILL: In a way, it's hard to evaluate because there's such a difference in performance models now with the dose standard versus the release standard. Tim presented what was done in TSPA 95, but the volcanism scenario is really based on TSPA 91 and TSPA 93, which was designed around a whole different really series of problems. So it's not really fair to compare whether they are going to or have going to or did going to incorporate this when they haven't done the analysis yet.

Certainly everything we heard at the technical exchange is a willingness to consider it.

MEMBER HINZE: Great.

CHAIRMAN POMEROY: And just for the record, Britt, would you kind of read for me what that second to the last bullet says?

MR. HILL: That's where we're getting up for the final stage here. I have gotten to the CC, the mean is conservative. There is from the previous slide, the mean is less than 50 millirems per year. We talked about the geological basis for saying it may disrupt 10 canisters. So again assuming that we're just doing something simple on this, that we're dealing with 500

millirems per year. The probability models range up to 10 to the minus three for 10,000 years. So there's risk. 10 to the minus 3, 10 to the minus 4, 500 milirems per year. That's where the multiplication comes through for less than or equal to 0.5 millirems per year.

Now I would like to put one particular spin on that because we're always talking about significance. We have significance relative to an undefined standard or release standard. I don't think anybody is saying that the release standard is going to be a millirem per year. A volcanism of itself is not going to be causing the repository to fail from a release standard of around that range.

The significance in terms of the program is even harder to evaluate. Tim showed a range of numbers here. We don't have a 30 kilometer dose from the DOE that we can directly compare to for this. From the NRC's numbers, comparing five kilometer drinking water with 30 kilometer Amargosa Valley, it's about an order of magnitude decrease. For the mean dose from the NAS recommendations is 14 millirems. Now how significant is another milirem to all of that? In one sense, it's about a 10 percent contributor. In another sense, it doesn't push us over the limit.

But this is not the real evaluation of

repository performance for undisturbed settings. There are many scenarios being developed that show that the expected dose from the undisturbed repository is significantly less than 10 millirems per year. In that case, this might be the only significant release process for the entire repository system.

So there are, I would emphasize, different ways to look at significance, absolute versus relative.

VICE CHAIRMAN GARRICK: Just looking at this one for a moment longer, and assuming you are addressing the 10 to the minus three volcano.

MR. HILL: Yes.

VICE CHAIRMAN GARRICK: If you were asked to draw a whisker diagram of the 90 percent confidence interval, where would that appear, to your best judgement? In other words, if you drew a five percentile, where would you put the five percentile and 95 percentile at the 10 to the minus three probability? I am just trying to get an idea of your estimate of the uncertainty about the mean. Do you know what I mean by a whisker diagram?

MR. HILL: I know what you mean. I am just very reluctant to do that.

VICE CHAIRMAN GARRICK: Yes. Well, you must know where it is if the mean is 94 percent, because that's got to come from that kind of information. So it's

somewhere.

MR. HILL: I think that's something I would rather follow up accurately rather than giving a back of the envelope --

VICE CHAIRMAN GARRICK: Yes. Well that would certainly answer the question of why the mean is that, if you had the whisker diagram about the 10 to the minus three ordinate.

MR. HILL: The other thing is that these calculations have all been for a direct release only. We haven't been evaluating the indirect release. There is considerable debate within the program about what the significance of that could be. If we have essentially the igneous dike, the intrusion penetrates the repository farther than the area of direct disruption, it will cause X number of additional canisters to fail. But those additional failed canisters will not be released by volcanism, so what's the performance impact of failing additional canisters on the order of maybe 100 more canisters at a specific time.

Again, that comes back down to certain models are saying that by 1,000 years you've got 50 percent failed, in which case another 100 doesn't make too much difference, or that through coupling processes and others, that the canisters stay in tact through the lifetime of

the repository. It's hard to find a straight decision point to say this is or is not significant.

MEMBER HINZE: Britt, we thank you very much. We apologize for keeping you up there, but it's obvious that there was a great deal of interest in what you had to say. We appreciate it very much.

I would like to suggest that we take a 12 minute break and come back. I apologize to our colleagues that are coming up next, Abe Van Luik and Tim Sullivan and John Trapp, but we'll have those next.

(Whereupon, the foregoing matter went off the record at 3:28 p.m. and went back on the record at 3:46 p.m.)

CHAIRMAN POMEROY: Let's reconvene right on schedule.

MEMBER HINZE: Our next -- what schedule are you on? Our next speaker will be Abe Van Luik, who will be talking about the incorporation of volcanism into TSPA-VA.

MR. VAN LUIK: Don't pay any attention to April 24 on the first viewgraph. This is no longer my title. I am actually -- but it's still me.

MEMBER HINZE: It's still the Department of Energy?

MR. VAN LUIK: Yes. What they have done is

made me the Technical Manager of Performance Assessment, so I don't have to worry about the other types of details that come with management.

What I want to do is summarize the volcanic scenarios we have used in previous TSPA analyses, going over some of the same territory that was just covered, quickly however. I want to do a summary of the volcanic scenarios that we want to consider in TSPA-VA. We want to incorporate alternative interpretations of the probability of occurrence, and incorporate alternative models of direct and indirect effects and consequences.

Let me move right along here. This is what we have done previously. We looked at basaltic volcanism. We looked at the left side of this tree. The intrusion acts directly on the repository. A dike forms. There may be transport of waste. There could be indirect effects also from no waste magma contact. TSPA 91 looked at this. TSPA 93 looked at this. We have before looked or Link, et al, has looked at basaltic cone forms. You notice here that there is a point probability and the PVHA that you have heard about already gave us some new information there. But this is where we were up until we started working on TSPA-VA.

If you remember TSPA-91, we looked at direct entrainment of waste to the surface. The amount of the

entrained waste was a function of the volume of the dike, extent of wall rock erosion, area of the repository intersected by the dike. We had random dike orientations, lengths and amount of wall-rock erosion.

One thing about TSPA-91, we had two separate groups doing TSPA-91. We had Sandia and the Pacific Northwest lab. If you recall the presentations we made at that time, Sandia used all the information gathered by the project. PNL went off in its own direction, and went to the literature and came up with a more conservative calculation, but the bottom line was comparable, which gave us some amount of feeling that we had a pretty good case.

TSPA-93, we looked at indirect effects, looking at intrusive events, accelerating waste package and waste form degradation, the acidic gases that you were talking about a minute ago.

What were the results? Well, in TSPA-91, we felt the models were conservative. The probability that we picked was 2 X 10 to the minus four over 10,000 years. We looked at the old EPA sum. If you recall 40 CFR 91. You can see that this point right here is defined by the probability of occurrence. This is a very similar plot to what you were just shown by the center staff.

Then our point right here, if you look at the

compliance points for the old EPA standard, we were a couple of order magnitudes away from that in any direction that you want to go in.

So basically, we figured that in TSPA-91, we had covered the issue of direct entrainment and movement of waste to the surface.

TSPA-93, again looking at the same standard, but this time we did some calculations way out to a million years. You can see that given that your probabilities were fixed but increased your time period, that this line just keeps on marching up as you extend your time. But even at a million years, we could still meet as it shows here, a 10,000 year standard.

Basically what this shows right here for the 10,000 year case is that the indirect effects had no real effect. The accelerated waste package corrosion a little bit, and that's it. The rest of it was pretty much a normative case.

Now we're going to improve on these because now we have the PVHA which gives us credible estimates of probability of occurrence and uncertainties for intersection of a dike with the repository. We have the Los Alamos national laboratory's volcanism synthesis report. We have also the modeling that you have been hearing about from the Center.

At this point, I would like to give a few seconds to Tim Sullivan. He can have a couple of minutes if he would like, to give us a little overview of the volcanism synthesis report.

MR. SULLIVAN: I hate to do that. Abe, you are on a roll, and you haven't been interrupted yet.

In response to your question, Bill, I had a little bit more information. Britt is probably as familiar with this as I am.

There's some information provided in the volcanism status report, the 1995 report, that presumably you all have. In the synthesis report itself there will be significant updates to the consequence analysis chapter, that's chapter 5.

There are four main topics that Abe has covered there. The first is the final detailed results of studies of eruptive effects and sub-surface effects, including lithic abundance studies that can be used then to constrain the estimates of waste entrainment for various eruption styles. That is based of course on analog studies that Greg conducted in 1995.

Second, interpretations of the alteration of silicic host rocks, reduced by basaltic dikes and sills for both vitric and zeolitic tuffs. This will support the indirect effects, model updates that Abe is going to refer

to shortly. Again, these are analog studies.

Thirdly, also reported in there are studies of factors that influence shallow intrusion geometries.

Then finally, preliminary modeling and theoretical studies of the sub-surface effects of intrusions will be reported in the synthesis report.

In terms of that communication question that you asked --

MEMBER HINZE: That was four.

MR. SULLIVAN: Four, correct. I would just make this comment. The contractor's technical staff for the volcanism program now consists of Frank Perry part-time. So there's little opportunity really for communication except at professional meetings presumably as Britt referred to before.

MEMBER HINZE: Tim, one of my concerns about the communication is that the Center and the NRC are moving with dispatch with the Suzuki model, and trying to put some constraints on it. What I was trying to get at is there any information that's coming out of your consequence studies that might be useful to them? Or is this information in their hands or just not in the public arena?

MR. SULLIVAN: For the dispersion models, there is no new information, with the exception of the

lithic abundance studies that would contribute to waste entrainment estimates.

MEMBER HINZE: Journal of Geology article.

MR. SULLIVAN: Well, updated from that, yes. We'll get you that information as soon as we can.

MR. VAN LUIK: Thank you. I think a point on communication also is that we are counting on being able to cooperate with the Center, with the NRC's permission of course, in obtaining their models and evaluating them and vice versa. If there's anything that we have in the performance assessment world that you want to look at, I think we can make that available to you.

So what are we proposing to do in terms of scenario analysis for TSPA-VA? The same diagram as before. We now have the PVHA instead of a point frequency. We are going to do in tact waste entrainment and distribution at the surface. We are also going to reevaluate what we did in TSPA-93, given new information, looking at no waste magma contact directly, but the waste is -- the source term is modified.

We are going to look at the basaltic cone, waste entrainment and distribution at the surface. Then we have already done the work by Link, et al, in 1982. We are going to seriously consider the NRC's Tephra dispersion analysis that you have just been exposed to.

Then of course as Tim explained, the Los Alamos volcanism synthesis report will support this effort and also look at the intrusion acting indirectly on the repository through altering sub-surface flow, either through lateral diversion in the Calico Hills or creating fast path flow along a dike alignment.

So these are the things that we are proposing to handle in the TSPA-VA which is due next year.

MEMBER HINZE: Before you remove that. You are not going to get by that easy. Why the light line, the thin line around magmatic alteration of waste? It was that way in the other one too.

MR. VAN LUIK: I believe that you need to go to the document that describes these particular ideas. I thought that this one was basically through discussion in the document, said to be really an alternate view of this one.

So this should be the magma modifies -- the nearby magma modifies the source term. This is really the same thing as the alteration of the waste. So I think this is something that could be cleaned up.

So what are we looking at for alternative models in terms of probability and disruptive events. We want to use the elicited values from PVHA. We think they have a good pedigree.

We want to employ importance sampling to assure that events are sampled and weighted appropriately. As someone pointed out this morning, with the 10 to the minus eight probability, doing 1,000 runs doesn't assure that you are going to hit an event.

We want to use fixed best estimate, direct effects consequence models. Direct effect is considered to have about a 10,000 times greater consequence than indirect effects to do this important sampling.

We want to do consequence analyses using peak dose to average members of the critical group located at five and 30 kilometers up to 10,000 years and then up to 100,000 years.

You don't see a million years here like we did in TSPA-91 and 93 because since TSPA-95, we have accelerated the movement of water in a mountain considerably. We believe that actually our peak doses will show up before 100,000 years.

We want to look at sensitivities to alternative probability distribution functions, still honoring the elicited PVHA frequency of intersections. We will create alternative CCDFs at peak dose so that you can have a -- what did you call them, a horse tail.

What are the alternative PDFs that we're going to be studying in the sensitivity analysis? Looking at

fixed 50th and 95th percentile values, for example. Sampling from the raw probability distribution function, where we are transforming it to a log normal that honors the 50th and 95th percentiles. Sample from a uniform distribution of elicited median values from a log-uniform distribution encompassing the 10th and the 99.9th percentiles.

Discarding the one outlier. You saw the probability distribution function. There is one outlier at the low end of the distribution, and recompiling the PDF. We will do these as sensitivity analyses. Then we will document these results in the TSPA-VA.

This is of course addressing the question of how sensitive is all this to the shape and the properties of your probability distribution function.

Looking at direct effects and consequences, we will review the appropriateness of the conceptual assumptions in the Center's model and compare it to models used in TSPA-91 and recent information from the volcanism synthesis report.

Assuming that the Center model is representative, we would like to use it and define for it reasonable ranges for key input parameters. What we would like to do is use DOE experts and a subset of the probabilistic volcanic hazard assessment external experts.

This is not a formal expert elicitation, however.

We would like to make sure that the bounding parameters selected by the Center for the analysis they just showed you are represented within the range of parameter values that we select. Examples are the magnitudes of ejected material, depth and percent of wall rock entrainment, dike length and width, the eruptive material characteristics, and there's many more factors that go into that model.

We would like to conduct conditional simulations of consequences, meaning that we set the probability of occurrence equal to one, and sample over the whole range of the parameters that we will fix using this small subset of experts and document these results in TSPA-VA.

MR. TRAPP: I do have to make just one comment, Abe. That word bounding in there, we would object to strenuously.

MR. VAN LUIK: Yes. That's why we put it in quotation marks. It's our view that you did some bounding, and it's your view that you did some very sensible estimates. You know, that's --

AUDIENCE COMMENT: (Inaudible.)

MR. VAN LUIK: We would love to have one of your people as our expert, but I think it's illegal for

you to work for us. I think it would really mess up the licensing process too. So we need to respect each other and our independence I think.

Alternative models for indirect effects and consequences, we want to review the previous work that we did. The indirect effects on TSPA-93, we think was pretty good, but it did not go far enough.

We would like to revise those models or develop new models, including models to bound the effects of the enhanced degradation that we looked at in TSPA-93, enhanced degradation of cladding and waste form. Revise the solubility of radionuclides, given the new chemistry that will be imposed on the system. Modify transport characteristics along likely flow paths. Look at a revised saturated flow field if necessary.

Conduct conditional consequence modeling. Again, this is conditional probability one, of indirect effects using bounding models of indirect effects. This time the bounding is not in quotes because this is our bounding, not your bounding.

Reconfirm that the consequences of indirect effects are significantly less than those due to direct effects. We can't reconfirm that of course. That will be a different story. But this is our expectation. Again, document this in TSPA-VA.

MEMBER HINZE: And you will have hydrologists, et cetera, working with you in putting this all together then?

MR. VAN LUIK: Of course. PA no longer works in a vacuum on our project.

MEMBER HINZE: Great.

MR. VAN LUIK: TSPA-VA will utilize recent results from the PVHA, the synthesis report, and the Center work. The sensitivity analysis, the probability of occurrence, direct and indirect effects and consequences will be conducted and documented. We will document everything that we do in TSPA-VA.

If either consequences or risks are significant, then volcanic scenarios will be included in TSPA reference case. In other words, if our result also comes out as the Center's result did with a 500 millirem per year result, we will include it, even though the risk at that point is still .5 millirem per year.

But if both consequences and risks are insignificant, then we will document it in TSPA-VA, but we will not include it in the reference case, and probably will not go forward with that case in licensing. We will say this issue is closed at that point once the NRC of course agrees to that.

So this is our plan for the TSPA-VA. I have

just donated about 20 minutes of time because you asked no questions. I think the bottom line is look forward to a good product and a comprehensive treatment of the subject.

VICE CHAIRMAN GARRICK: Before you leave, Abe.

MR. VAN LUIK: Yes, sir.

VICE CHAIRMAN GARRICK: You went through this awfully fast.

MR. VAN LUIK: By the chairman's request.

VICE CHAIRMAN GARRICK: We'll have other opportunities to talk to you. But there are some issues that as a risk analyst that concern me a little bit. One of those is that you would allow yourself to get into a trap of talking about either consequences or risks.

If you talked about consequences or likelihoods that may be a rational process. But to talk about consequences or risk or to put yourself in the position where you might be measured on the basis of consequences is really asking for trouble.

MR. VAN LUIK: I would agree with you to some extent. In fact, my recommendation was to go strictly for risk. If the consequence was high, but the probability made the risk low, to just leave it out.

However, at the technical exchange that we just had with the NRC, one of the agreements was that we would look at either consequence, in other words, set the

probability equal to one and see what the consequence is, or risk. If those are high, we will continue forward with the analysis. So this is part of our agreement with the NRC staff I believe.

Is that correct, Tim? You are going to talk next with John on the agreements, but that was my impression. If not, strike those words and we'll just go for risk.

VICE CHAIRMAN GARRICK: Yes. I think that is very dangerous. The time that we have gotten in real trouble in the reactor risk field is when we made any gestures at all towards decoupling of risk into components that in this case don't even make sense. It doesn't make sense to talk about consequences or risk given that consequences are an inherent part of risk.

MR. VAN LUIK: That's true, yes.

VICE CHAIRMAN GARRICK: You have just muddied up the language of the business a lot. So it is just a suggestion that you may want to reconsider what that's all about.

MR. VAN LUIK: I will definitely reconsider.

VICE CHAIRMAN GARRICK: The other thing I wanted to comment about a little bit is every once in a while some of us sort of reflect on what we would really like to see come out of the performance assessment

business. You have an enormous risk communication problem with the performance assessment.

MR. VAN LUIK: Yes.

VICE CHAIRMAN GARRICK: So given that this is entering into a public arena, I would think that you would want to do everything you possibly can to make the results and the presentations as understandable and comprehensive as possible.

I don't know for sure how to do that, but one thought that occurs to me is that out of all of this analysis, it would be very nice if in the end, you came up with what you would consider to be a certain number of scenarios that have been so defined that they encompass all of the issues and questions that have been suggested. Let us assume that that might be 20 different scenarios.

You could somehow present those scenarios in such a manner that you would be able to address each one on the basis of its own merit and to some extent take a first step towards making the kind of commitment that the public will be looking for from the scientists. Namely, which of these scenarios do you think in fact we should worry about the most.

In the end, what you would really like to do of course is to appropriately weight the scenarios such that you really do answer the question of what is the

risk. If you can't do that, at least structure it in such a way that you can talk to each one of them and communicate as much as you can about your confidence of which scenarios are the most important in terms of risk.

Are you planning to do something like that? Because right now, you are suffering from the severe problem of conditional risk assessment rather than an unconditional risk assessment. You present information on the basis if we have this infiltration rate or this set of initial conditions, this is the set of CCDFs we get. I don't think we're doing the job when we do that.

I think we need to go that next step and say well, this group is what we think are if not a direct result of combining all of them on the basis of the evidence that's available, at least on the basis of our best shot, our best judgement, these are the one or two or three scenarios that we're convinced from a technical standpoint, maybe even not even from a compliance standpoint, but from a safety standpoint, are the most important.

That would enhance, it seems to me, the whole communication issue of what I have come to call PPA is all about, probabilistic performance assessment. Because up to now, it has not really been that.

MR. VAN LUIK: I fully concur with what you

are saying. In fact, so does my management. Because one of the things that we have been loathed to do up to this point is exactly what you have said, which is to actually make a pick out of all these choices and say this is the most likely path in our judgement. It would be nice to have everything in a PDF so that you can do that probabilistically.

What our management did last week was an unconscionable thing. Lake Barrett had to brief Tara O'Toole yesterday on the latest results from PA and on a couple of strategic things that are important to her. Lake finally said, you guys, give me an expected value case now because I am tired of hearing it could be this, it could be that. Give me what your gut instinct says, even if you can't do it.

You know, we made the argument that we couldn't at this point do a fully probabilistic 50th percentile or expected value case. He said well give me your best shot. So we actually created an expected value case with plus or minus a standard deviation because he said it is impossible for him to brief someone who is not steeped in this whole folklore of CCDFs and statistics and have them understand what the actual expected performance is. So he is pushing us in that direction. I believe that from now on, we will show those types of results.

For sure, in the executive summary of TSPA-95, and that will go to Congress and other people.

We will show what we as an integrated program expect the mountain to perform like. We will try to do it in ways that the TSPA-VA executive summary that actually goes up on the Hill and everywhere else, can be read by a politician and they will understand these are basically the risks that we are facing.

So those are the marching orders that we have, is to make it understandable and what's that other word, traceable and transparent.

VICE CHAIRMAN GARRICK: Well, the only reason I made the speech is because as you went through this, it seems like you were trying to respond to so many groups and so many different requirements that it wasn't clear to me that you the analyst really believed what you are doing any more. You were kind of a victim of the process. The process is being determined by expert elicitation activity, outside organizations, and what have you. That has to be considered.

But I think in the end, what we want to know is what the real experts that are pulling all this together really believe and can defend with fervor and conviction.

MR. VAN LUIK: Well, that's another reason why

if you look on the viewgraph, they are a little bit sneaky because they keep saying these will be sensitivity studies and documented in TSPA-VA. But the bottom line is only those things that have high consequence and reasonable sized risks will be reported in the actual nominative case of the TSPA-VA and be carried forward into the executive summary.

So the sensitivity studies will satisfy every client that we have ever had or thought we had, but the actual executive summary will sum up the case that we want to make to the public.

VICE CHAIRMAN GARRICK: Okay. I still wish I had never seen these last two bullets.

MR. VAN LUIK: There were a couple of discussions on that. I thought that we were bound by the agreement that we made, but we'll find out about that next, I suppose.

MEMBER HINZE: Further questions?

MR. VAN LUIK: I appreciate the suggestion then. I'll go home and take them off if nobody objects.

MEMBER HINZE: Let me ask you a quick question, Abe.

MR. VAN LUIK: Yes, sir.

MEMBER HINZE: What about the issue resolution reports that are being prepared in the process by the NRC.

What role do they play in this, if any?

MR. VAN LUIK: They play a role. We are expecting those I believe, at the end of this calendar year. Is that correct? All of them are scheduled to come out in time for us to address them in the TSPA-VA. For each one of those reports, we will do our analysis and then address the content of the resolution report.

If they come in much later than the end of this calendar year, it's going to have to wait until the gap between VA and licensing to address them individually. But we're hoping in each case everyone that applies, somehow the TSPA, that we will be able to address them in the upcoming TSPA-VA report.

MEMBER HINZE: I guess that's been made loud and clear to the NRC.

MR. VAN LUIK: I believe so. I believe that we have commented on their schedule and applauded them for getting them done this calendar year.

MEMBER HINZE: Well thank you. Once again, a great job. We really appreciate it.

We'll move directly then to John and Tim Sullivan, who will be talking about the agreements from the technical exchange. I think perhaps we can lead right in then to the future activities, if that's all right with the two of you. How are you handling this?

MR. SULLIVAN: We don't have this choreographed. This is one of the things we didn't sit down and --

MR. VAN LUIK: Can we take both ties and clip one thing to both ties?

MR. SULLIVAN: Is it all right if I -- I'll chime right in here.

MEMBER HINZE: Take Bruce's chair, if you don't mind.

MR. TRAPP: Well, I could say that along with everything else, this obviously is a first of a kind because how many times have you seen the DOE and the NRC on the same viewgraph?

What I really was kind of thinking about doing and I'll stand out this way, is basically read the agreement and maybe give a comment or two. Let Tim give a comment or two, and then go onto the next agreement.

The first agreement basically is that NRC and DOE agree that the rate of volcanism is relatively constant for the last 5 million years and basically we can use this kind of constant rate to carry on to the period of performance.

What this really addresses is a question of waxing, waning volcanism, et cetera, this type of thing. It has been several models which have been proposed that

go one way or the other. What we're saying, and this is based in large part on the studies dealing with the Amargosa Valley isotopic province, looking at what happens with probabilities and different events when you start taking a look at this. It appears to be a constant rate which we feel should be used in performance assessment.

MEMBER HINZE: John, if I may. That certainly has been an assumption and was used in the PVHA for 10,000 year time period. Question, what happens when we go to 100,000 years?

MR. TRAPP: I really don't see any difference myself. I would be glad to let Britt comment or anytning, but I really see no difference.

MEMBER HINZE: Does anyone have any comment on that, our consultants, anyone?

Has DOE looked at this, Tim?

MR. SULLIVAN: Well, you know, given the long period of record and the long return period for volcanic events, that seems a reasonable assumption to me.

Kevin, do you want to comment any further?

MR. COPPERSMITH: Yes. Kevin Coppersmith. We asked the experts that question in the last workshop of the future forward time period. They saw no difference between 10,000 and 100,000 years. If we went out 1,000,000 years, they begin to say hey, we may need to

think about a time history that might be different from the constant. But out 100,000, you are looking back 5 million. They felt fairly comfortable in that forward time window. There would be no significant change.

MEMBER HINZE: Thanks so much.

MR. TRAPP: Tim, you got anything more on this one?

MR. SULLIVAN: No, this has not been an issue of contention.

MR. TRAPP: Well, this next one was probably the easiest issue to agree on. The reason it's brought up is really there was this one piece of information, and we weren't sure which was talking about a potential six million year old silicic volcanism. Through a whole bunch of codes into some of the different models, assumptions, what happens, the center went, took a look at this stuff, got some material and age data that it's roughly about 9.5 million years.

MR. HILL: Yes, it's 9.3 -- or 9.1 plus or minus .3.

MR. TRAPP: So basically, what it amounts to is we don't have to worry about this all of a sudden silicic episode sitting in the middle of these basaltic episodes, and it makes our projections, our understanding of the system a little bit easier to handle.

Tim?

MR. SULLIVAN: I have no comment on this.

MR. TRAPP: Number three, DOE agrees to consider evaluating new data such as the size and volume of Little Cone, the number of events through Anomaly A through hazard sensitivity studies.

I guess the point I would bring out here is the -- such as it's really a question whenever you saw some of the analysis as new information comes into play, we would expect to see additional analysis, etc. as it goes through. And again, we will be taking a look at -- it's not a question of hey, we found one new rock, please analyze, let's make sure that this is something that might have some effect.

MR. SULLIVAN: As you saw in Kevin's presentation, we've gone ahead and evaluated the impact of this new information on the hazard estimate, and the impact is insignificant. And this suggests to us that new data of the type that's being collected by the center is not going to be significant to hazard assessment.

That is, that data that refines the volume estimates of the buried centers or that information which contributes to refinement of the event counts. And DOE has done this reevaluation as an example, but we don't intend to be continually updating the results of this

study.

We will document what Kevin presented in the letter that's mentioned here at the end of the agreement.

MR. TRAPP: The only other thing I would say along this line is where we're really concerned more than anything else is -- I guess Norm would describe it as paradigm shifts; are we really changing the conceptual model that we're dealing with versus just tweaking numbers.

CHAIRMAN POMEROY: Tim, excuse me; but I assume you mean though that you will continue to look at new data as it comes forward, and that you'll do something to look at its relative importance. I mean, I suppose I could see some situation where new information came forward that was so startling that you would want to evaluate it.

MR. SULLIVAN: Yes, Kevin identified at least those types of new information that the experts thought might lead to them reevaluating, and they were rather dramatic occurrences. We will evaluate developments in the field of volcanism and additional information collected in the Yucca Mountain area between now and the license application and update the PVHA results as required.

That's our plan.

CHAIRMAN POMEROY: Thank you.

MR. TRAPP: This agreement was probably the longest one to try to write and the longest one as far as time to put together in simplest form.

We presented what we consider our -- and I'm not going to call them the range; I'm going to call them our best estimate of values from 10^{-7} to 10^{-8}. DOE is basically going ahead and saying that they want to use the PVHA results.

My basic concerns here or comments dealing with this are related to the fact that again, if you take a look at all the stuff that's in the literature -- for instance, the material that has been put out by Gene Smith on this -- you are talking values which really are ranging from 10^{-10} all the way up to about 10^{-5}.

If you start putting this into a value and try to calculate these whole things, weighed everything, what you end up with is median values which are slightly greater than 1×10^{-8} up to about 5×10^{-8}. You end up with mean values that are just about the same.

I will caution you though, and I'm not sure that this totally gets across, is that again, the numbers that we're using and that the state are using are for the volcanic disruption. The numbers that you're getting from PVHA are volcanic disruption and indirect effects from the

dike.

So while the numbers appear to be converging, there's a wider disparity than appears just looking at the first glance.

Tim?

MR. SULLIVAN: Well, as has been discussed several times today, the information that was used that formed the basis for the state's estimate and the information that has formed the basis for the center's estimate was available to the PVHA panel. And they accorded it, you know, the weight they thought appropriate in developing their source zone models.

And it's on that basis that we intend to proceed then with the hazard estimates from the PVHA.

MEMBER HINZE: You're saying there's a difference between the NRC, the center, and the PVHA because of the lack of consideration of diking sills in the center's and the NRC's work?

MR. TRAPP: Yes, our number basically is to represent the volcanic vent itself, the possibility of a vent forming and dispersing material. The numbers that you get out of PVHA are the vent or -- and this is where the whole structure and everything else starts getting important.

Because when you take a look at these various

structural models that are used in PVHA, they have the vent occurring within the zone or within the zone and the boundary. And what actually comes out and hits the repository is not the vent, but it's a dike.

So you're getting the combination of those two probabilities combined.

VICE CHAIRMAN GARRICK: John, are you suggesting that maybe if you both defined the probability the same that there might not be this difference? It sounds like --

MR. TRAPP: I'm saying if we both define the vents the same and took a look at the PVHA, there would be a slightly larger difference in numbers than you --

VICE CHAIRMAN GARRICK: Well, it sounds like one's a joint probability and the other one is not. One's a combination.

CHAIRMAN POMEROY: John, --

MEMBER HINZE: Go ahead, Tim.

MR. SULLIVAN: I would certainly agree that's a contributor, John, but I don't think it's the major contributor to the difference between the full range of these estimates. As Kevin said earlier, it has to do mostly with the rate density that you assume.

And as long as you include Yucca Mountain in a high rate density area, then you're going to get a

slightly higher annual probability.

Perhaps, Kevin, you need to review that again.

MR. TRAPP: But if you take a look also, and Kevin can go through it, Yucca Mountain was not included in the high rate density. It was included in the low rate density in all cases.

MR. COPPERSMITH: Let me go back to the event definition problem.

MR. TRAPP: That's where it stems from.

MR. COPPERSMITH: We basically have -- we're defining an event or defining an intersection as a dike intersecting the repository. So it's not indirect effects of, say, a nearby dike, one that's up gradient, down gradient, close enough to have a thermal impact or degassing, there are corrosive effects and so on.

It is physical intersection of a dike with the repository. Okay, so the indirect effects would be a different issue. Now, whether or not the tip end of a dike intersecting the repository has sufficient energy to lead to release is not considered here.

Now one way to look at that, we also have the probability of the actual center point of that dike intersecting the repository, and that is about a factor of three less. We also have a model that right now we allow for them to define a probability distribution -- if this

is the ascending dike, here's the center point of that.

They define the probability distribution that it is offset from that center. If we use -- we assume that it is actually the center point, we have that probability of distribution. That's about a factor of one and a half, maybe two less than the integrated probability.

So we can use that information to look at the effect of this. If, in fact, it needs to be at the center point of the dike to have sufficient energy or release, then basically you can either look at those centers, factor of three, or look at the event centered probability distribution. And those -- all three of those PDF's are given in the report.

I think -- going back to what Tim said, I think that is not -- it does contribute to a difference. But I think it really is the issue of rate density that was assumed in the Yucca Mountain area versus the Crater Flat area. The rate densities are lower generally within the Yucca Mountain area because the observed centers are zero in the post five million year time frame.

Therefore, rate densities are assumed that come from like the background zone. Just like in seismicity, the Amargosa Valley isotopic provence, whatever the background zone was, that rate density, for

many of the models, is the driving rate density for intersections in the repository.

MR. TRAPP: All right then, the point that I was trying to make is the rate density that was assigned to Yucca Mountain basically because of the way the lines were drawn was low rate density.

MEMBER HINZE: A couple of questions.

Is there any chance of deconvoluting your analysis to limit it to cones?

MR. TRAPP: That basically is about 6×10^{-9} is what their number came out to be.

MR. COPPERSMITH: Let me clarify.

For some of the experts, cones -- the alignment of cones would be combined into a single event definition -- if they're on a proper alignment and they're close spatial proximity, same age, etc. So these are the event -- center for the event, not necessarily for cones.

MEMBER HINZE: But they had to have a cone to have an event.

MR. COPPERSMITH: Except for the case of hidden events. Okay, there's no location.

MEMBER HINZE: So they could have hidden events?

MR. COPPERSMITH: Yes, all of them had hidden events ranging from about ten to fifty percent more than

observed.

MEMBER HINZE: Let me ask the question, how much would the -- how have you considered the effect of this upon these numbers? You know, you are concerned about this event definition. Have you made some quantitative analysis of how much it impacts the results?

MR. TRAPP: Basically, if you take a look at the fact that the probability is derived totally separate from the consequence -- therefore, if you're sitting with a -- anyway, they are not linked like you do a seismic hazard analysis. You've got a probability for a magnitude seven versus a probability for a magnitude eight versus probability -- and you draw these lines.

This is a probability which is kind of an event definition, and it's really disruptive. It is simply a multiplication factor. So if I say that the probability is 10^{-7} versus 10^{-8}, I'm dealing with an order of magnitude difference in total consequence.

MR. COPPERSMITH: Again, for those who are familiar with the PRA, reactor PRA, this is exactly analogous. We're dealing with -- in this case, this probability of frequency of intersection is comparable to the seismic hazard curve that expresses the frequency or probability of exceeding various levels of ground motion that has uncertainty associated with it.

The consequence analysis is exactly analogous to the fragility analysis, fragility curves. They give the probability of failure as a function of ground motion. So you're given the ground motion; this is the probability of failure of a particular system or component.

It's likewise here. Given an intersection, what does it do in terms of consequence? What's the probability of release? So when we talk about consequence analysis, we're talking about conditional probabilities. And I guess the point that John Garrick was making that it -- when we talk about consequence space, it's just like fragility space.

If we don't put a probability in front of it, it becomes given that that occurs as a probability of one. The most dramatic example in reactor PRA's that I remember is meteorite impact. If you think of it in terms of simply consequence, it's a very dramatic impact. You need to multiply that times the probability of actual meteorite impact at your particular location.

MR. TRAPP: And I would say that basically the way that we've been handling the fragility curve in this would be kind of the thing that is sampled such as the wind speed, the waste package life, the grain size, this type of thing. So there is a probability that way, but it is not -- once you set a point curve, etc., like I

said, it's a straight multiplication.

CHAIRMAN POMEROY: John, just to clarify for me, the center model, your model, does not consider the intersection of any dikes associated with this event?

MR. TRAPP: It's basically a number which is the probability of volcanic disruption, to be another way of saying it -- better way of saying it.

MR. HILL: Paul, this is Britt Hill.

There is an area term associated with that. That's what Chuck was talking about earlier to account for the fact that you can have not just a central conduit, but a distributed number of events. That's that 500 meters to two kilometer thing that he is sampling.

That would be to accommodate the area -- the subsurface area term that is directly impacted by the volcano. And I think there's been a little bit of ambiguity about direct and indirect effects that's used very differently. By direct, I think the way that we're usually referring to that is that material is released directly to the surface from a volcanic eruption by the event.

Whereas, indirect, you still have direct penetration of the dike into the repository, but no material is being transported to the surface by the igneous event. It's a subsequent transport through

normal, hydrologic flow and transport that is giving you the release.

So just a small point of clarification.

CHAIRMAN POMEROY: Thank you, Britt; that's very helpful.

MEMBER HINZE: Are there any plans for the center to include longer dikes beyond the two kilometer --

MR. HILL: We've done some work on that where we're looking again at classifying what is an event that you can look at in Crater Flat, the four plus volcanos there at one million years can constitute a single event. And the end result is, you get fewer number of events, but the events impact a greater area.

And so there really isn't much of an effect. It sort of plays off one against the other. The same thing with increasing the number of dikes associated with it. It's fairly speculative. It doesn't change it too much until you start getting up into a very large number of events or -- excuse me, dikes associated with a single volcanic event.

But we're still faced with that problem of how do you get material from a dike two kilometers away to flow laterally and come up into the accessible environment. And there's just not a good mechanistic basis to make that assumption. It's really the area

immediately beneath the volcano at the surface.

By immediately, I'd say tens of meters as a good first guess that's going to be directly transporting material to the surface. You're not having kilometers of lateral flow from a dike where the volcano is two clicks away from your repository boundary. It's just not realistic to think you're going to get lateral flow in a dike for two kilometers and bring material up to the accessible environment during that event.

MEMBER HINZE: When there are these -- the fire forms that come associated with a dike opening up, for example, in Hawaii, which you're the expert on, how much lateral flow is there?

MR. RYAN: My ears perked up, Britt, at your last comment. I think I would strongly disagree with it.

Let me simply say that in basaltic riff zone environments worldwide, the pattern we see is migration from the upper mantle starting out at depths that vary between 100 and 60 kilometers winding up at shallow depths that could be anywhere from ten to two kilometers depth.

And in that ten to two kilometer depth range, a long period of stagnation and storage. Now, magna will then move out of that ten to two kilometer depth range when it's over pressured by subsequent magma batch additions to the bottom of the system.

And when it moves out, the dominant transport mode is lateral, not vertical. This is simply a least work argument that if the crust is sufficiently relaxed by extension, the system can reduce its potential energy in a very efficient way and do very little work in the process by a lateral migration.

We see this in spades documented seismically in Hawaii. We see it in spades in Iceland. And we think that there's very good geomorphic evidence for the same kind of thing going on in the earth's mid ocean ridge system some 60,000 kilometers of riff zone in aggregate.

The eruption and cone building is really the exception in one perspective, in one sense, to the rule. Some of the older Hawaiian volcano observatory staff members refer to dike intrusions as "uneruptions." So these uneruptions are dominantly a lateral, blade forming, intrusion mechanism.

MR. HILL: All right, but once you've established a conduit at the surface, and this is what we were talking about at lunch -- if you have established a cinder cone at the surface two kilometers away from the repository, even if you've got a dike in the subsurface extending into the repository zone, do you think it's realistic that you would have two kilometers of lateral flow from 300 -- or at 300 meters depth to bring waste

from that repository two kilometers away up the conduit?

And that's really the process that we're worried about, not the emplacement mechanism itself. Certainly there's abundant evidence of lateral flow during ascent. But once the vertical flow is established to the surficial conduit, do we really think we continue at 300 meters depth in the subsurface to transport material for kilometers away up the conduit?

MR. RYAN: I guess the answer is yes and no.

In the very short time frame, yes; I think many of the things you're suggesting would happen. Once you develop a lubricated, high temperature conduit to the surface, that would, for some several days to a few weeks, represent the least work --

MR. HILL: Yes.

MR. RYAN: -- pathway to accommodate any subsequent increase in magma in the system.

However, one thing that we see in spades is that these are pulsed activities that play themselves out over a few tens of years in the migration of a large magma batch to the surface and its eventual intrusion interruption.

So you will get, yes, perhaps the kinds of things you're suggesting in a single pulse. But then, in a few tens of hours or in a few weeks when that dike drops

below its solidus, when it becomes mechanically strong with high, elastic moduli and high sheer strength relative to its porous country rock, subsequent events will then run parallel dikes past that.

So we call these sheeted dike complexes and ophiolite suites and so forth.

You can get both kinds of things going on, both kinds of scenarios are going on. The scenario you're talking about is a subset really of a larger decadeal pulse of activity that plays itself out over several eruptions and several intrusions.

MR. HILL: Right, the event can have multiple phases to it, of course; and that's what we're getting at.

MR. RYAN: Sure, absolutely.

As an example, at Krafla, a volcano in northeast Iceland, it started intruding and erupting in December of '75. It stopped, you know, September of '84. There were 12 intrusions and nine eruptions. The aggregate eruption zone was something like 13 kilometers in length.

Similarly, in Hawaii, an eruption that's now ongoing, started in January of '83 -- it's had well over 50 pulses of activity, each one concluding with a die back of fountaining activity and then cranking back up again.

MR. HILL: Right. And conversely, at Cerro

Negro in Nicaragua for the past 150 years there have been I think 23 significant eruptions, each of which have occurred within half a kilometer of the central conduit and almost exclusively within the central conduit and no evidence of any real significant bocca development beyond the base of the cone.

So there's plenty of examples.

MEMBER HINZE: This is a very good discussion.

MR. TRAPP: I wanted to ask one question myself, and it's basically the bottom line that I would suggest that's coming out of your statements would be the suggestion that a larger amount of waste could become entrained and dispersed than we've been assuming.

MR. RYAN: It's hard to respond to that in a succinct yes or no. Because these are pulsed events and because each pulse can be several million cubic meters in volume, and because all of the pulses would add up to a ten or 20 year intrusion eruption scenario, you could get all kinds of possibilities in terms of how magma would interdict a repository depth.

For example, the first pulse would invade the free volume space of the repository. It may very well fill it up floor to ceiling. That would then begin to cool. And some period of a few tens of years, depending on how high the ceiling was from the floor, that would be

largely subsolidus. It would be intact.

But nevertheless, a decade later, another pulse could come through and rip open this prior consolidation episode. So there are a lot of different scenarios that one could envision with fairly garden variety igneous process.

MR. TRAPP: I guess one way to describe it in PVHA would be we're talking about a larger range of uncertainty.

MR. SULLIVAN: Before you move on, please note here that the last sentence of this agreement is a commitment by DOE to explain how the PDF will be used. And Abe outlined that for you this afternoon, and that will be included in the upcoming letter as well.

MEMBER HINZE: That was my question.

The last phrase of that is recognizing NRC's comments, and presumably that will be taken into account then?

MR. SULLIVAN: Yes.

MEMBER HORNBERGER: Could I just -- just to make sure I'm totally clear on this, am I to infer that the NRC staff does not accept the PVHA?

MR. TRAPP: We've got concerns with the PVHA would be a better way to describe it.

MEMBER HORNBERGER: Concerns? Concerns about

the process, about the expert elicitation itself, about the --

MR. TRAPP: The only ones I am --

MEMBER HORNBERGER: -- qualifications of the experts?

MR. TRAPP: The only ones I am willing to talk about -- because if I talked about any more than that, my management would be on top of me -- are the two I've already raised.

CHAIRMAN POMEROY: Your management takes the chance, of course, that we will make an interpretation of what that means.

MR. TRAPP: Well, this is why I said at the very beginning there will be a meeting between DOE and NRC where they're talking about the whole expert elicitation process, and this is basically the end of this month.

Wes? Roughly the end of the month.

Also, next month, you guys will be discussing this. I am not going to get up and start going into that.

MEMBER HINZE: That was a prior agreement with John, and understandably.

VICE CHAIRMAN GARRICK: This is not an atomic safety licensing board.

MR. TRAPP: Now if you want to get into that, I'll run upstairs, get Norm and a whole bunch of others,

and they could start talking about it; but I'm not going to do it.

MR. HILL: The agreement from the technical exchange, I think, is the fairest way of putting it. We're both -- if we look at the 10^{-7} as an upper bound, we're pretty much in agreement at the current state. There are differing views, however, on the lower bound.

I think that's the simplest way of putting it.

MR. TRAPP: And I'd point out again that our upper bound number basically is approximately the same as the best estimate used by the state.

MR. SULLIVAN: Let me respond to that.

DOE did not sponsor the PVHA expert elicitation in order to determine an upper bound. The purpose of that elicitation was to define the mean value and the uncertainty. And our intent is to use that full distribution. We will be doing some sensitivity studies as Abe described.

MR. TRAPP: Are we ready to move on?

MEMBER HINZE: Let's move on.

MR. TRAPP: Abe Van Luik was talking about the agreements. This would basically be the agreement that he was talking about, number five. It's slightly different than he had stated.

We basically agree that volcanism is of

regulatory interest, and its probability and consequence will be considered. If determined to be significant to repository performance, and now we've got to know exactly what the standard is that we're dealing with, the effects of volcanism will be included in the total system performance assessment.

Right now, the only comment I'd make is it appears to be one that we're going to have to carry through.

MR. SULLIVAN: Well, my recollection is a little faulty.

Abe, I don't see any commitment here on the part of DOE to carry these calculations forward solely on the basis of the results of consequence analysis and isolation, do you?

MR. VAN LUIK: This is Abe.

I believe you're correct. It could be since (a) mentions probability and consequences, and (b) if determined to be significant, what's the subject of if, it should be probability and consequences. A legalistic rendering, which is what we gave ourselves, would be that we agree to both.

I would be happy to substitute risk because probability and consequence is risk. So let us change it to risk and correct that in the letter that we're going to

send to the NRC.

MR. TRAPP: Basically, as it says, the effects of volcanism, so you're talking about what you're modifying. So if the effects of volcanism are determined to be significant with respect to repository performance, you'll include it.

MR. VAN LUIK: Exactly.

MR. TRAPP: So like I said, which standard -- what's the exact standard statement? And the only comment I am making past that is the fact that it appears that it's going to have to be considered. And I think that would help you out a little bit.

VICE CHAIRMAN GARRICK: Yes.

MR. SULLIVAN: I think John agreed, Abe.

MEMBER HINZE: Volcanism pulled him out.

MR. TRAPP: The next one, the treatment of consequences outlined by DOE including extrusive magmatic events (cone and dike formation) and intrusive magmatic events (sill and dike formation) with both direct and indirect effects is generally appropriate at the level that we were discussing.

Two comments. If you take a look at what has been done in TSPA-91, 93, etc., this type of thing, one of the places we had a tremendous amount of problems as far as the consequence analysis was the incorporation ratio.

In other words, the volume of the waste that could get out.

And we basically feel that these numbers were several orders of magnitude too low.

In the discussion that was conducted today, there was basically the statement that how they planned on handling some of this extrusive material was to obtain the center's model and run that one.

I personally get a little bit leery about that comment just on face value because what we end up with is the NRC licensing itself and the DOE working as a regulator, and I think that one needs to be looked at in a whole bunch of different perspectives.

MR. VAN LUIK: If I may address that one.

This is Abe.

What we would like is to critically review your model, put in our own inputs so that all we have is basically your computer framework, which is like borrowing your WordPerfect to put in our Word, and I don't think there's any collusion at all that can be implied from that.

However, if -- you know, if, after you see what we would like to do with it, you still feel that way, we'll create our own model. It's not a problem.

MR. TRAPP: Mike Bell.

MR. BELL: This is Michael Bell, part of that elusive NRC management.

I'd just point out that I'm sure people are aware that NRC has any number of reg. guides that direct applicants to use certain models, certain assumptions, and this set of parameters; and we would find that an acceptable way to meet an NRC requirement.

So I don't really see, you know, any fatal flaw in DOE adopting the center's model. Of course, you know, we'll look very closely at how they use it and make sure they use it the way we intend it to be used.

I guess while I'm up, just let me go back to the one point that came up five minutes ago about what -- is NRC rejecting the PVHA. I'm not sure if that's exactly the way the question was posed. But I think John addressed that very -- in his very first presentation this morning on his four slide issues related to PVHA.

And basically, it was DOE is free to use PVHA or whatever information they desire to make their licensing case. And you know, we'll consider that and other information, I'd say, the way the NRC staff always does when it reviews an applicant's submittal.

MEMBER HINZE: Thanks, Mike.

Is there anything more on this?

MR. TRAPP: I don't have anything to add to

that.

The next one, DOE and NRC agree there's uncertainty and consequence analysis for the magmatic waste package/waste form interactions and will be evaluated.

If you take a look at some of the stuff that Britt's already shown, one of the things that does come out is the effect of the assumption of exactly what size we're talking about of the waste particles, exactly how it's incorporated. Does the waste package last? It really drives a lot of the results.

If we sit there and assume that everything stays in the pellet form, let's face it, it's going to be lucky if it gets a kilometer away. You break it down to the average size of ten microns, etc. You start getting these values on out to these critical groups that we're talking about.

Based on some preliminary runs and some of the things they've done, I suspect if we talk about the one to two micron range which a lot of our waste package people feel is the main number or best number to use, you'll end up with higher doses. We need to take a look at it, and it needs to be evaluated to see really what its effect is.

MR. SULLIVAN: Without reopening the discussion of whether the consequence analysis presented

by the center was a bounding value or reasonably conservative or something else, it's not clear to us that uncertainties in those parameters have been considered in the analysis itself.

And that's what DOE intends to do as part of the consequence analysis that Abe provided.

MR. TRAPP: Any other comment on that one?

Basically number eight, they're to provide a letter which -- or DOE is to provide a letter describing how they're going to handle the subissue resolution, as specified in three or four above, for consideration. In simplest form, last Friday, we did have a teleconference with DOE in which we went over some of the preliminary thoughts they had on this.

There is supposed to be a discussion tomorrow morning where we'll have another series of things going through this. And all I can say is we're working forward on this issue.

MEMBER HINZE: Questions about that?

Let me ask a question, if I may, regarding --

MR. TRAPP: If I said you couldn't, what would you say?

(Laughter.)

MEMBER HINZE: I would say you were being normal. Glad to hear it.

The NRC had a number of comments in the site characterization analysis of the SEP. These were answered by the DOE as part of a letter from Steve Brocum which accompanied the PVHA, as I recall.

Where do the comments -- where do these comments and concerns come into play at this time? Are they being -- by the board, are they not being considered any longer, or are they -- where are we?

MR. TRAPP: It's a very hard question to answer for a couple of reasons.

If you take a look at the comments that were raised in the SEP and the comments which have been raised on the various study plants, there are a whole series of them which are based on an assumption that certain things would be done by DOE.

There's a whole series that basically we're not sure exactly how to handle because the program has changed. There's concerns that are raised with things like -- well, paraphrasing -- DOE, we really think you ought to be going out and doing some more geophysics specifically looking at these items.

We don't feel that that's been done. It's really one of the reasons that we asked the center to take a look and do some of their geophysics. It's obvious they're not going to be doing any more work. I'm not sure

myself exactly how to handle them. It's a question that has been raised with our board, at least to a certain extent.

I understand there's going to be some type of discussion going back. DOE has got also a statement or -- I'm not sure how to phrase this correctly, but they are going to be talking about which study plans are -- let's call them OBE -- so how to handle these things, I don't know; except that we are supposed to address them in the issue resolution report.

And until I know how to handle them, they'll be covered there, but I'm waiting for direction.

MEMBER HINZE: Well, I was at the technical exchange. This obviously was not discussed at the technical exchange.

Is there any ongoing discussion with the DOE on any of these points --

MR. TRAPP: Most of these --

MEMBER HINZE: -- worthy of that at this point?

MR. TRAPP: There really isn't.

MEMBER HINZE: Is there -- the question is whether there's a reluctance to close them when --

MR. TRAPP: I would love to close them.

Mike's standing up. Maybe he's got --

MEMBER HINZE: I was waiting for him to come say something.

MR. BELL: Dr. Hinze, what you're really asking about is a generic question. It's not specific to volcanism.

But basically, there are a large number of open issues from the time of the review of DOE's site characterization plan that, as John said, anticipated certain work was going to be done, certain programs were going to be carried out that now, because of the cut in DOE's program, much of this -- some of this work, at least, won't be done.

The way we see addressing this is -- I guess in our refocus program, as we go through developing issue resolution reports and looking at the significance of missing information, if it turns out that some critical piece of information is needed to close some open issue to resolve one of our KTI's, then we would go back to DOE essentially and say, you know, this particular open issue is going to remain open.

There will be some of these open issues that presumably you'll now conclude is no longer significant to performance and you know, that work didn't really need to be done. It was one of those things that when DOE had a very large program with lots of national laboratories and

a large budget, you know, they planned to do the -- gain scientific understanding, but maybe it, when put in the context of total system performance, wouldn't have been needed.

MEMBER HINZE: So it would be fair to say that they are going to be selectively considered, but you're not going to make an issue of keeping them open?

MR. BELL: Not if they're not important to performance.

MR. HILL: I think a quick example would be a lot of the number of comments that were related to the Lathrop Wells, and I believe many people are familiar with the technical disagreements about the origin of that volcano.

Several years ago, that seemed to be very important towards probability, and now it seems to be a background issue. So we're -- there's no reason to keep on with open items regarding how many events were at Lathrop Wells. It's really not an issue that's significant right now.

MR. TRAPP: And I would suspect there would be a large number which would be handled to the effect that because this thing hasn't been addressed, there is a fairly large range of uncertainty. If you can live with it, that's fine.

MEMBER HINZE: It just seems to me to be a -- the proper thing to do to identify those issues which are no longer important, that are no longer of consideration. And I don't know if the term is close them, but at least tell DOE that they're not -- no longer being considered as comments.

MR. TRAPP: Well, like I said, the spot -- the exact spot that that will be covered is in the mission resolution status report.

MEMBER HINZE: Okay.

Okay, any other comments about the technical exchange and the agreements thereof?

VICE CHAIRMAN GARRICK: I just want a point of clarification that's kind of a nit, but something that John said earlier made me dwell on it a while.

John, if you have an applicant that you required a point estimate calculation from, for example, a conservative one or whatever, you wouldn't object if that applicant supplied you with a full distribution from which you could pull any number you wanted?

MR. TRAPP: No.

VICE CHAIRMAN GARRICK: No? Okay.

I just --

MR. TRAPP: No, basically --

VICE CHAIRMAN GARRICK: Especially in --

MR. TRAPP: My personal point on this whole thing is I think they may be doing more work than they really need to.

VICE CHAIRMAN GARRICK: Yes. But especially at a time when your Chairlady is pleading for movement towards risk-informed performance-based analyses. And it seems to me that's a very good example of doing just that.

MR. TRAPP: They can -- we have no objections to them doing it.

VICE CHAIRMAN GARRICK: Yes.

MR. SULLIVAN: Where are we doing too much work?

(Laughter.)

MR. TRAPP: Basically the spot that we're talking about.

MEMBER HINZE: Excuse me, could we turn off the recorder?

(Laughter.)

MR. TRAPP: Our basic comment there is from what we see and taking a look at volcanism the way it's handled, we think it can be handled by a point estimator rather than taking a look at the whole range of uncertainty and all these other kind of things.

Now, how -- if you want to go through the whole thing, nobody's going to object a bit.

CHAIRMAN POMEROY: John, that's a nice lead in to my question because I -- at the technical exchange, it was my understanding that there was a statement something like if DOE were willing to utilize the point estimate 10^{-7} something, what was that something -- what was the rest of that sentence?

MR. TRAPP: No, it's basically --

CHAIRMAN POMEROY: I'm just asking what the minutes or, you know, the nonexistent minutes would say.

MR. TRAPP: It's basically that the 10^{-7}, we feel, is a -- this is a case where we do have what we consider a good bounding reasonable estimate. Yes, it's --

CHAIRMAN POMEROY: This is important.

MR. TRAPP: It's a number which we feel comfortable with. It's one -- and this is really kind of important. You talk about new information and the impact of new information and all this other kind of stuff, and you have one more event, two more events; you have a little bit of difference in structure, and it makes this number maybe go up a little bit.

If you take a look at some of the things that we've done back of the envelope both on the computer, you know, where you're doing mass of stuff and just sit and scratching your head, we feel comfortable with this range

going from 10^{-8}, 10^{-7}.

And we feel that if you use the 10^{-7} as a number, as a point estimator number, then we would have something that would be able to be carried through with a tremendous amount of confidence through the licensing.

We can see no -- assuming that we don't have a volcano between now and licensing coming through the repository, -- (laughter) -- we can see nothing that -- right now that would cause us to think that we'd end up with a value higher than that for any type of meaning.

So we're really, like I said, talking about the work that we're doing, etc., to support this issue resolution report, we're taking a look at some of these buried geophysical anomalies and trying to find out their significance. We're going ahead and starting to do some work on the probabilities of the indirect secondary effects and processes.

We're looking in detail on controls and conduit locations. We're checking these things out against and evaluating them against other volcanic fields. For instance, one of the good ones would be the work that was done at the San Francisco field where the models basically were compared to what's there.

MEMBER HINZE: John, if I could suggest, it would seem to me that first item there, the significance

of buried geophysical anomalies, at least from my viewpoint, I think it's clear from what we've heard today that another event down at Amargosa Valley may not be of any significance to the results.

But those that are further up in Crater Flat and towards Yucca Mountain would be the ones that I would give first priority to.

Does that sound reasonable?

MR. TRAPP: Yes and no.

There is a fear if we --

MR. HILL: Again, not knowing the age of these potentially buried -- or the anomalies that are potentially buried volcanos, we have to evaluate their location in addition to the age and how that would impact the present probability models.

When we're saying investigate significance, it's not a large, complicated exercise to do this, but a reasonable selection criteria and taking a look, like you're saying, at the closer in ones being more important.

But I'd still like to reiterate a point we made earlier about regular spatial shifts through time as another potentially significant development that isn't being adequately compensated for by current models. And whether or not we think this is something that needs to be worked on more than just in passing, a couple of

paragraphs in the annual report, depends again on how significant we think it is.

So it's very difficult to say whether these potential anomalies that we haven't really considered beyond brief passing are significant or not is very premature right now. That's why this is a planned activity.

MEMBER HINZE: And this will be done and analyzed and incorporated into the IRSR?

MR. TRAPP: To the extent possible, it will be in the IRSR. It may be in the annual report. It may be in the next year's annual report. It basically depends on a whole series of --

MR. HILL: It depends how significant it turns out.

MR. FOLAND: Could I ask one point of clarification, John?

I don't understand this -- that comment that it may be significant to the comment that you said earlier that you were willing to accept a 10^{-7} figure which seems to be the bounding limit now in the present data base from the PVHA.

MR. TRAPP: It's not the bounding limit on the PVHA number. It's a limit which has been stated here. But if you actually take a look at the number, the bound

is not 10^{-7}; it's something less than that.

MR. FOLAND: It's the determination most of the models go 10^{-7} to 10^{-10}, is that not true?

MR. TRAPP: It's a number that they have thrown out saying that they do. But if you look at PVHA, they don't. The numbers do not -- the models do not range to 10^{-7} that you'll see in the PVHA report.

MR. FOLAND: Okay. But having said that, what you're saying is that you're going to collect more data that may impact the 10^{-7} figure. What you said was you were content with --

MR. TRAPP: In some ways, it's basically supporting the 10^{-7}. There's also a question in some of these of basic assumptions going into the model. And there's a couple locations and a couple scratches of their heads you can take a look at which would cause you to totally change your whole model and the basis for putting these together.

Britt has been talking about one, this whole possibility of instead of talking about a kind of uniform area where we are talking about is a shift in fields through time. This could have an effect if you're talking about a shift that's coming up, say, for the Amargosa Desert area working its way on up north.

The amount that we'll do on here, very

honestly, is going to be extremely limited because we have got very little time and very little money. We're going to take a look at it.

MR. FOLAND: I guess I wouldn't argue with that. The point I find incongruous is your acceptance of 10^{-7}.

MR. TRAPP: Because taking a look at these various models, we -- aside from seeing something extremely major, we don't see, in going through these back of the envelope calculations, where these numbers will change if you keep the basic models that we've got.

And all that you are doing to the basic models is adding events, for instance. No, it's not going to change it. It's going to stay there. If you take a look at these models and you find some information that says the model itself that I've used in calculating probabilities is totally wrong, then we may have a problem.

I'm assuming right now on the 10^{-8}, 10^{-7} that what I think I know about the area, and it's damn little, that they won't change. We think that if you use these numbers, from everything that we understand or think we understand, that it will be a robust number.

I could be sucking wind.

MR. PATRICK: Wes Patrick from the center.

Are you saying, in other words, that you see the mean not shifting or however you choose to characterize it; you're just typing up your distribution. Is that what you're -- I'm trying to get it.

MR. TRAPP: Basically, yes; I see the mean as basically saying between this range, yes.

CHAIRMAN POMEROY: But John, is there something in the work that you are going to do that can result in a change like a factor of a thousand in the first numbers?

MR. HILL: I think we're stuck with a basic recurrence rate that precludes that unless you appeal to a rapidly waxing system relative to the rest of the basin in range. But I think it's also important to remember that my position would be we're all having a better licensing position if we have a good understanding of the uncertainty in our data in models that we've considered.

And this may turn out to be not significant, but we need to go through the calculations because we have a credible hypothesis that such things may occur. So let's see if it's significant enough to do a detailed investigation; or, in all likelihood, it's not necessary to do more than just say well, we can reasonably guess an age, reasonably guess the location, look at the models, consider it, and say we've considered it and it hasn't

affected our understanding much more than saying we've given due consideration to available models and hypotheses.

CHAIRMAN POMEROY: But if you say you'll accept 10^{-7} and nothing else needs to be done, how is that consistent then? I know you didn't say that.

MR. HILL: I think we're confusing an informal conversation with a position we're going to hold through licensing.

MR. TRAPP: And I think we're also confusing the 10^{-7} versus the actual PVHA range.

MR. COPPERSMITH: Can I comment on that?

CHAIRMAN POMEROY: Yes, please.

MR. COPPERSMITH: The calculated -- this is Kevin Coppersmith.

The calculations, in fact, do go down to 10^{-7} and do go to 10^{-10}. That is again given the definition of a dike intersecting. During the course of quality assurance, if someone wants to evaluate the calculations, there is a low weighted series of branches by a few experts that lead to that.

Therefore, it has extremely low weight; but it is the bounding calculation to the total set of calculations which are added together using equal weights across all ten experts.

MEMBER HINZE: Let's move ahead, John.

MR. TRAPP: Okay.

CHAIRMAN POMEROY: John, you're sitting there shaking your head. I can't let it go by. You're standing there while Kevin was making that statement shaking your head.

MR. TRAPP: No, I wasn't really shaking my head. There's a series of concerns that we've got, and there are some things that we are doing to try to resolve these concerns.

It really ranges more -- it's a combination of PVHA and it's a combination of some of the stuff that hasn't been presented which is the, you know, Smith-type papers where you are taking a look at -- and the state would probably object -- but here I would say worst case estimates of probability.

And you are getting numbers above 10^{-6}. You're getting numbers almost up to 10^{-5}. And you are then having to assume a structural control which basically almost forces every event to come straight through the repository, and you're basically having to assume a strongly waxing system.

We don't see that, okay? Is there something that could make us change? Yeah, it's possible. But no, we don't see these numbers. We want to just take a look

at a few of these things out there, these zones, and reinforce where we're sitting and maybe hope that we can tighten up our numbers in the range a little bit better.

If you take a look -- this is also to support the probability -- there's a series of planned peer review publications that are coming out; one that's talking about this whole integrated volcanism structural probability model that you've heard. We want to get this into the literature and review it.

The San Francisco volcanic field, this is also an area where we're talking about analog -- and there's a petrogenesis of the Yucca Mountain basalt system. In addition to this along the line of probability, we also would like to put together an issue resolution status report on the probability of silicic igneous activity.

This should be a very minor, minor thing. And we're not really sure if it's even worth doing. That's why it's really got a question mark if it's even going to come out.

With our present schedule, we are planning on writing an issue resolution report which basically would be a year later down this line on the consequence of basaltic igneous activity. Technical work is really going into the secondary, indirect effects on performance. We want to get a better handle on the area that can be

disrupted.

We want to do some work -- a little bit more work on this whole Suzuki model to test a few of the assumptions in the Suzuki model. I think both Britt and Chuck talked about that earlier. We want to also take a look -- and this could be the real joker. It's the effects of the repository on ascending magma.

Are we correct in making the assumptior that these analogs are the ones we should be dealing with, or is there something about the repository which can totally change this whole type of assumption? And we want to do some more work on this waste package/waste form behavior.

VICE CHAIRMAN GARRICK: John, have you been given a specific design for a waste package?

MR. TRAPP: What we are dealing with right now is the waste package that was in the CD advanced conceptual design, and that's the one that we are using.

Peer review publications that we plan on putting out: cooling and degassing of shallow basaltic dikes which are getting to the secondary effects. There's tephra dispersion and risk analysis on Cerro Negro. There's a lot of work that needs to be put together there.

And especially the one I think is most important is the development and evolution of the 1975 Tolbachik eruption because we're using that a tremendous

amount as far as an analog.

In the area of data quality subissue, basically we are going to be reviewing the DOE volcanism synthesis report. I think I heard the day that now this will be coming out at the end of this fiscal year. And again, looking at the significance of these buried geophysical anomalies.

Now this last one probably is of more interest to a whole bunch because now we start talking a few things that cross cut. This first one, the sensitivity/importance studies, volcanic system, total system, we've done a little bit of that. We have got at the present time the draft TSPA code in house.

We're trying to -- testing this thing out which does incorporate the Suzuki model. I believe at the end of this month we're supposed to get the final code, at which time we will really start running these things, doing a bunch of different sensitivity studies, vary parameters, and see exactly where we sit.

We also will be -- well, that also would be something, before I go on, to be discussed to whatever degree the ACNW wishes during the July meeting.

Yes, we're going to take a look at the DOE TSPA-VA plan. Obviously we're going to have to review the final TSPA-VA. We've got to take a look -- and this

really isn't a straight geology, etc., but it's more in the area of the performance assessment people, but we've got integration we've got to worry about, and let's take a look at the dose sensitivity to differing critical group locations to other possible pathways.

For instance -- and I'm not sure if we're going to, but we're planning, at least right now, on looking at these five kilometers effects of igneous activity which, you know, wouldn't be the well, but it would be somebody around there and maybe a rancher kicking up dust -- and really integrate all this stuff, waste package/waste form, structure, PA, etc., into one coherent package.

I was asked very strenuously to talk about reprioritization. You see as much as I'm really going to say on reprioritization at this time. We've got these sensitivity/importance studies. We're going to be taking a look at that. There's a whole series of budget exercises which are going on right now.

And basically, when we get done with the sensitivity/importance studies, we get done with the budgeting, what I've shown you here may have been a total misleading representation. I don't think so, but it's basically -- it's going to be reprioritized, so we'll be putting the money where -- and the effort where we see the

most benefit.

MEMBER HINZE: Thank you, John.

Questions? Ambitious.

CHAIRMAN POMEROY: John, let me ask you a question about resources.

What's the level of FTE involvement combined in this particular activity?

MR. TRAPP: FTE level?

Right now, we are running at 1.4 NRC which covers basically myself as portion for project management, all the managers, and the on-site reps; and roughly 2.5 people at the center which is basically Chuck and Britt, some support from Conway, and the other support from their agency.

CHAIRMAN POMEROY: So four FTE's is a rough number?

MR. TRAPP: Four FTE's. Roughly, you've got Chuck, Britt, myself -- we're doing it, for all practical purposes, almost full time. And then --

CHAIRMAN POMEROY: And is that the planned level of effort in the next fiscal year?

MR. TRAPP: As far as I presently know, and I'm seeing Mike shake his head yes.

I haven't been in the budget, so I'm glad to see that.

CHAIRMAN POMEROY: Thank you.

MR. TRAPP: I've got a job next year.

MEMBER HINZE: Tim, if you could just give us a brief review, that would be helpful.

MR. SULLIVAN: I'm going to take a quick look here at the two subissues. First, hazard analysis or probability estimate. As I've reiterated several times here, the PVHA was sponsored and is still intended to provide a sound, defensible basis for licensing.

As described by Kevin, DOE has fulfilled it commitment to evaluate the new information, the new center information; and we have concluded that these data do not significantly impact the results of the PVHA. Based on this and as well statements by the experts at the end of the final workshop, we believe the results of the PVHA are robust.

And new information, short of dramatic paradigm shifts, are unlikely to change the disruption probability document in the PVHA. As I've said earlier, no further site characterization data collection is planned for the reasons I already discussed.

As Abe discussed earlier today, we intend to use the PDF from the PVHA and the reference case and do selected sensitivity studies for TSPA-VA. We plan to address numbers three, four, and eight on the list of

agreements that John and I went through and document that in an upcoming letter which we anticipate will be coming forth soon.

In our view, the next step is subissue closure. Although the schedule is not clear to us any longer if data collection is anticipated by the NRC and the center through the end of FY97, we're no longer sure what the issue closure schedule is.

In short, this indicates that the results of consequence analyses that will be performed next year will be documented in the TSPA-VA. Consistent with the agreements from the technical exchange, the next step in this should say after completion of the TSPA-VA an associated documentation is consequence to subissue closure.

Now I think this simply reiterates some points that Abe made. We will, in terms of the letter and future presentations, be rethinking and rewording that third bullet following John Garrick's suggestions.

MEMBER HORNBERGER: How about the second --

MR. SULLIVAN: Yes, the second as well. So let me leave it there, unless there's any questions.

MEMBER HINZE: Thank you very much.

Any questions for Abe?

MR. SULLIVAN: Tim. Or Abe.

MEMBER HINZE: I'm sorry. It's early in the evening.

VICE CHAIRMAN GARRICK: One DOE is just like another.

(Laughter.)

MR. SULLIVAN: Well, we hope we all give you the same answer.

CHAIRMAN POMEROY: Very good.

MEMBER HINZE: I want to -- before we go any further, I want to thank -- make certain this gets on the record thanking Lynn Deering for putting together what I have found to be a very helpful, helpful workshop with the excellent presentations from the DOE and the NRC, as well as from the state and participation of the consultants.

I think it's been very useful to us.

We are supposed to go into a round table at this point. I feel as if I've been in a round room most of the day. I frankly had 13 questions, and I don't know if there's something ominous about the 13, but 13 questions that I could kind of isolate to trigger some conversations during the round table.

And I've found, in going through them, that every one of them had been discussed probably to that. I wonder if any members of the committee have questions that they would like to kick out into a round table for a

conference?

VICE CHAIRMAN GARRICK: I've heard enough about volcanos for one day.

(Laughter.)

MEMBER HINZE: Well, I think it's all caught up with us.

I'm wondering if our consultants would have some general statements or specific concerns that they would like to raise as a result of hearing this barrage of information.

MR. FOLAND: Not so much burning issues. Unfortunately, Bruce is not here, and we probably should -- had a brief conversation at lunch, and I think the PVHA analysis is very, very useful and was quite successful.

But I'm reminded that about 25 years ago, a colleague of mine stood up at a professional meeting and talked about water on Mars and never did publish the paper on the water on Mars because all the expert opinion was unanimous against there being any water on Mars. And right now, we're trying to figure out when the water on Mars was frozen into polar caps.

And so, with that comment, I hope that puts part of it into perspective. And that is, consensus doesn't mean that that's in fact the right answer. And this is what worries me, that the analysis is being driven

by the fact that there aren't many events that we know about.

I see John Trapp and the center people saying are there other unknown events, and then I hear the answer is well, they don't make any difference. I guess that troubles me a little bit that there may in fact be unknown events and do they -- will they have some significance.

And I would be interested to know if the experts in fact expressed an interest in seeing any more data that would have been particularly useful for them to be able to use to reduce their uncertainty.

That's not a question for anybody. That's just a comment.

MEMBER HINZE: Thank you very much, Ken.

Mike, are there any general comments? Any concerns about the differences that we have between -- that we've seen between the DOE and the NRC?

MR. RYAN: I don't really have any large, huge, general comment to offer by way of a conclusion. I've been delighted in the forum of the meeting and being able to nickel in, so to speak, as we've gone along from virtually 8:30 this morning until 5:30 in the evening.

It's been my pleasure to be here.

I think I've either directly or indirectly hinted or simply explicitly talked about my concerns as

we've gone along. I tend to think of volcanic systems as large, integrated, three dimensional structures with processes and dynamics that are ongoing in them, and the surface activity we see are really skin blemishes of larger beasts at depth.

Of course, we don't have real time geophysics in terms of real time micro-earthquakes that are -- that would have been -- that were generated in this that would help us map out, for example, in three dimensions what these -- what the morphology of these bodies look like at depth when they were actually operating.

We don't have the deformation fields that we have at active volcanoes. So we're -- it's been very much a post mortem kind of exercise. I frankly applaud the folks that have been involved in this and the models they've come up with to assign probabilities as based on, frankly, exceedingly sparse data sets.

MEMBER HINZE: One question that has come up here in the last conversation is the possibility of a waxing volcanic regime. Is there -- are there methods -- are there protocols in place for looking at this in a -- other than in a reoccurrence interval among these relatively poorly data'ed and sparse volcanic activity?

Are there other techniques that could be used to give that warm, fuzzy feeling that we are in a static

situation and can use that for the next hundreds of thousands of years?

MR. RYAN: What immediately comes to mind is in a somewhat different tectonic context perhaps, the fact that magma can come into a system sometimes aseismically and be resident without triggering any surface awareness, so to speak, of its presence.

Now typically, this is not the case in basaltic systems. More typically, it's the case in silicic systems. And Mt. St. Helen's is a very good example. The USGS was, of course, aware that St. Helen's was going to kick off or do something potentially explosive very early in the year 1980.

But we had no idea really of when that might happen. And of course, once it did, then we developed a series of models that used both seismicity and deformation to rather closely anticipate when the next dome forming eruption was going to occur and when the next ash plume eruption was going to occur.

But these were, of course, very much after the fact. Before the fact, there can be a lot of -- in the case of the silicic system, a lot of aseismic creep going on and aseismic magma migration. And of course, the trade offs between deformation amplitude and depth of the pressure source are such that pressure sources of

significant volume can be five, six, seven kilometers deep and really not have much expression on the surface.

MEMBER HINZE: Is the technology of crystal size distribution, CSD, in terms of looking at waxing and waning situations, is that something that's applicable here?

MR. RYAN: Well, Bruce Marsh and Cathy Cashman have been two of the key players in this, and it's been basically Bruce and Cathy's child, so to speak, in terms of petrologic application. Basically we're just talking about crystallization processes and igneous bodies which are experiencing substantial cooling.

My sense is that -- is still in need of laboratory quantification in terms of calibrating what crystal growth rates would look like in olefines and pyroxens and feldspars and so forth. Now some of that's gone on in the last 25 years, but I think more needs to be done.

MEMBER HINZE: I didn't mean to put you on the spot here.

MR. RYAN: I would hate to hang --

MEMBER HINZE: This was a question for Bruce --

MR. RYAN: Sure, sure.

MEMBER HINZE: -- and he escaped on us.

MR. RYAN: But you can see even -- in exceedingly active systems like Kilauea in Hawaii, you can see dike like bodies that have stagnated at depths of two to four kilometers that have had really rather large excursions of plagioclase growth so that the plagioclase crystals are megascopically obvious from hand sample.

And yet, all else indicating that these systems are extremely active with lots of through going magma and so forth. So simply the chance intrusion of a body slightly off axis, slightly off a conduit's axis, will set the stage for stagnation.

MEMBER HINZE: Yes, understand.

Would Tim -- I'll get it right -- Tim, are there any comments that -- or any concerns that you would like to raise -- that DOE would like to raise?

MR. SULLIVAN: No, I'll pass.

Kevin, do you have anything?

MR. COPPERSMITH: One comment on your last comment.

About the time history issue, there's a couple of ways for hazard -- well, let me make an overall comment that -- going back.

The issue of what's important is a very difficult one. This is a hazard analysis, and not all things are important to hazard. For example, the

arguments that went on prior to the time that we came in to do the hazard assessment on Lathrop Wells and whether or not that was polygenetic or monogenetic were downright knock down, drag out fights.

Very important from a scientific point of view. When you boil down -- it boils down to the basic difference between whether or not there's one event there or four, that's contributing to a total regional -- say ten to 20 events. It doesn't have a major impact.

That isn't to say it isn't important; just that its impact on hazard is not significant. But when we talk about the difference right now of saying well, whether or not there are buried events doesn't make a difference, that's in the very narrow world of hazard analysis because there's only two things that matter to hazard analysis: where future events will occur and what their average rate is.

And those two things, unless they're dramatically different, don't have a big impact on the result. Now the result is couched in terms of orders of magnitude. It's 10^{-8}, 10^{-7} orders of magnitude annual likelihood. If we were talking about this happening three times a year, it would be a totally different likelihood.

So we're dealing with extremely low likelihood of something occurring. And that's why this is a strange

hazard world of very low annual probabilities leading to only certain things having any impact.

I just wanted to make that point.

Now the issue of time migration dealing first with the spatial migration issue, that is something that Bruce Crowe and the LANL people looked to in a lot of detail. Where do the -- where do the individual events occur post ten million years? Where are those centers, what's the distance to the next one going through a time series, what is the azimuth to various events over a time period?

We found no statistically significant distribution on migration over that time period. So they did look into that. In terms of the time history of change of recurrence, there was waxing and waning which is changing frequency. We did -- Dr. C.H. Ho from UNLV made a presentation, provided papers on modeling using the Weibul distribution.

And he has shown that he thinks -- show indicate that there is a slightly waxing system that was presented. The experts in general saw that in fact it's very, very sensitive to your start time. You start right after -- right before five million years, you get a different result than if you go back, let's say, nine million years or just after the oldest or the youngest

miocene volcanics, there's a big period of no production; and then you go into a period of production.

That's obviously going to be a waxing. With few events, again we're plagued by the absence of events. Some say that's a wonderful thing to have. Volcanologists say it's a horrible thing to have. Those models were available and were considered.

In general, I think the only experts who really looked intó a model is Rick Carlson. He used a volume predictable model that says that volume production through time may change. What volume predictable says is if you know the amount of time it's been since the last one, you predict the volume of the next one. It's like a model for an earthquake.

Then you can use that and the average size of an event or volume and you're basically decreasing the time between them. So he used a time history of volume production as an indicator or a change in the situation. The volumes have changed little so that that model is very close to a homogenous model, but slightly different.

They are -- I think spatial and temporal migration was considered in this project. Models were very simple as dictated by the absence of very few actual volcanic centers.

MR. HILL: If I may interject for just a

minute?

Both of those statements, Kevin, really lay the groundwork for the potential impact of new information. Not that we're saying that there are ten new volcanoes and it has this effect. But for the spatial migration, certainly Bruce in association with, I believe, Kevin Walnum and Goldner Associates looked at migration through time at trying to take that vector-based approach and found that there was no regular spatial migration.

But the presence of new volcanoes and, admittedly, one or two new volcanoes, probably won't change that. But if we're dealing with more than a couple of volcanoes, we need to take a close look and evaluate whether that new information may give us a discernable spatial pattern, especially if we look only within the volcanic system of interest for PVHA, the five million year and younger, rather than the entire post-caldera episode.

By the same token, a lot of the experts had a problem with Ho's model because of the picking the starting time. But the new information that would also make it difficult to support a waxing system would be inclusion of the Funeral Formation within the Yucca Mountain system.

Regardless of your starting time, as long as

you're around five million years within the period that most of the experts considered relevant, including the Funeral Formation, gives you a steady state recurrence rate even if you wanted to use Ho's definition of start times, spacing, and how he manipulates the beta function.

And this would be the point of new information may not set the world on fire with order of magnitude differences in results, but it certainly gives us confidence and an ability to make an informed decision about how we ultimately use or discard models in the licensing process and our absolute confidence.

You know, our comfort level is the term that's been used quite often in how well these mathematical simplifications of a sparse data set represent the system we're trying to understand for a ten thousand year period.

So I'm not disagreeing with you at all; but just that the new information needs to be considered and --

MR. COPPERSMITH: I agree with you.

From my point of view, it's DOE's responsibility to make the best quantification of uncertainty knowledge about this issue as possible. The regulator needs to feel as comfortable.

MEMBER HINZE: I will call on John for any last comments.

MR. TRAPP: Just a couple very minor points.

If you take a look at --

MEMBER HINZE: In fact, I'm wrong.

THE WITNESS: Most of -- I've probably said what I had to say about ten times today.

If you take a look at the new information issue, I'd like to stress one point there. The new information, in some ways, really has been better in getting some of the integration and the integration of the various models. For instance, if you take a look at what we gathered dealing with the Little Cones area, we started being able to talk about the total basin subsidence or relationship of volcanism to basin subsidence in the faults in a better tie in of the structure of volcanism and ability to put together a more comprehensive, tectonic model that included all the things.

So the fall off of some of these things is more -- and it has to be looked at more than just in the straight volcanism standpoint. The next one I would bring up -- it's my last point, and I'll say it kind of tongue in cheek.

If you're looking at the geophysical data, if you take a look at some of the microseismic, there's an interesting warn and small microseismic at Lathrop Wels.

MEMBER HINZE: You have to keep those goats

out of there.

Well, with that, Mr. Chairman, I recommend that the committee just -- the information they've received and try to bring it together in the future.

And thanks to everybody.

CHAIRMAN POMEROY: Okay, I'd like to first of all thank Bill and Lynn both for a really excellent interchange of ideas and discussion today. I really think it's been extremely useful.

I want to remind people that they -- we are going to discuss the use of expert judgement in this process and other processes at our May meeting. We'll try to do it on May 22nd. And as I've said, we would very much like to have -- and we'll work with Carol Hanlon to have both you and Kevin participate, as well as Steve, in that discussion.

And we'll certainly try to have some appropriate NRC people here also to participate in that discussions. We've looked at two concerns that John and management have brought forth. And I think we've gotten some really interesting information both on the question and the concern regarding new information and the second concern that we discussed.

There clearly are other concerns . We will take those up with the appropriate people.

I's like to thank you all for coming and lasting until 10 minutes or 15 minutes of 6:00. We won't return until 8:30 tomorrow morning.

(Whereupon, the proceedings were adjourned at 5:53 p.m.)

C E R T I F I C A T E

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: 91ST ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW) MEETING

Docket Number: N/A

Place of Proceeding: ROCKVILLE, MARYLAND

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.

Corbett Riner

CORBETT RINER
Official Reporter
Neal R. Gross and Co., Inc.

United States
Nuclear Regulatory Commission

INTRODUCTORY COMMENTS

Presented: ACNW APRIL 22, 1997

By:
John Trapp, Senior Geologist
Engineering and Geoscience Branch
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INTRODUCTION

- FOCUS OF TODAYS PRESENTATION IS SUMMARY/HIGH POINTS OF FEBRUARY 25-26 DOE/NRC TECHNICAL EXCHANGE
  - MUCH OF NRC PRESENTATIONS COVERED IN ANNUAL REPORT
  - MAIN PORTION OF DOE PRESENTATION IS SUMMARY OF PVHA REPORT
  - BOTH DOE AND NRC WILL SUMMARIZE PLANNED WORK
  - STATE WILL PARTICIPATE

-MAY MEETING WITH ACNW AND PLANNED NRC/DOE MEETING WILL DISCUSS EXPERT ELICITATION CONCERNS

-JULY ACNW MEETING TO GO INTO DETAILS OF TOTAL SYSTEM PERFORMANCE ASSESSMENT AND (AS NEED BE) RELATIONSHIP OF IGNEOUS ACTIVITY TO TOTAL SYSTEM

IMPACT OF NATIONAL ACADEMY OF SCIENCE RECOMMENDATIONS ON IGNEOUS ACTIVITY KTI

OLD EPA STANDARD BASED ON PROBABILITY OF SCENARIO AND RELEASE TO ACCESSIBLE ENVIRONMENT

NAS RECOMMENDATIONS FOCUS ON DOSE OR RISK TO AVERAGE MEMBER OF CRITICAL GROUP

WORK HAS SHIFTED FROM EMPHASIS ON PROBABILITY TO EMPHASIS ON TRANSPORT AND DOSE

- **PRELIMINARY WORK SUGGEST PRIMARY EFFECTS IMPORTANT, HOWEVER, MAIN SECONDARY EFFECT MAY BE JUST SHIFT OF TIME OF PEAK DOSE**

ISSUES RELATED TO PVHA

CONCERN WITH EFFECT OF NEW INFORMATION

CONCERN WITH GEOLOGIC BASIS OF ZONES

STAFF RECOGNIZES

- DOE FREE TO USE WHATEVER INFORMATION THEY DESIRE TO MAKE LICENSING CASE

- NRC WILL CONSIDER FULL RANGE OF INFORMATION AVAILABLE

GEOLOGIC SETTING AND PROBABILITY OF VOLCANISM IN THE YUCCA MOUNTAIN REGION AND DISRUPTION OF THE PROPOSED REPOSITORY

NRC DWM Program Element Manager - John Trapp

CNWRA Element Manager - H.L. McKague

PRESENTED BY

CHUCK CONNOR

April, 22, 1997

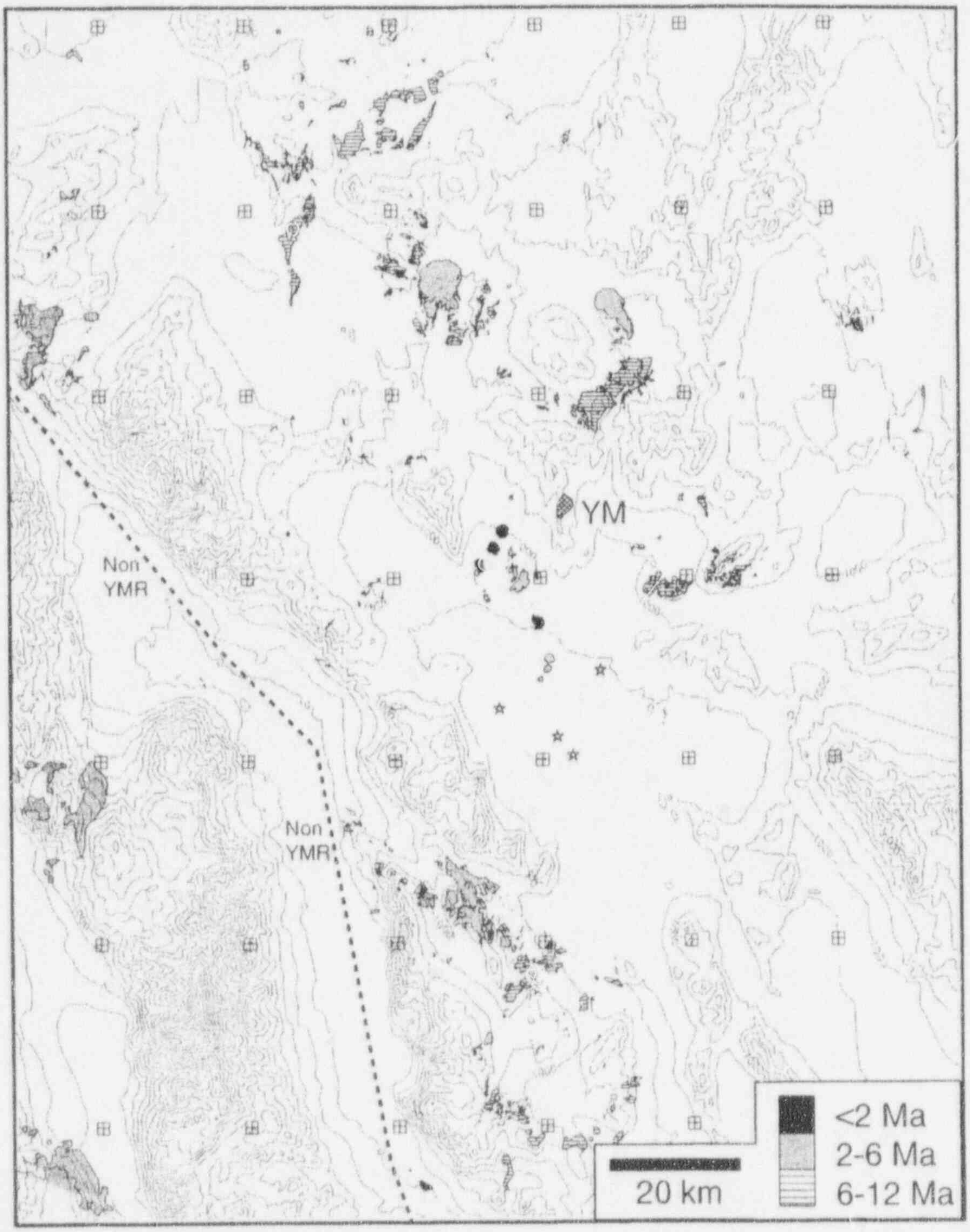
CNWRA Contributors

C. Connor, M. Conway, D.Ferrill, S. Greenon, B. Henderson, B. Hill, M. Jarzemba, P. La Femina, S. Magsino, L. McKague, R. Martin, J. Stamatakos

OUTLINE

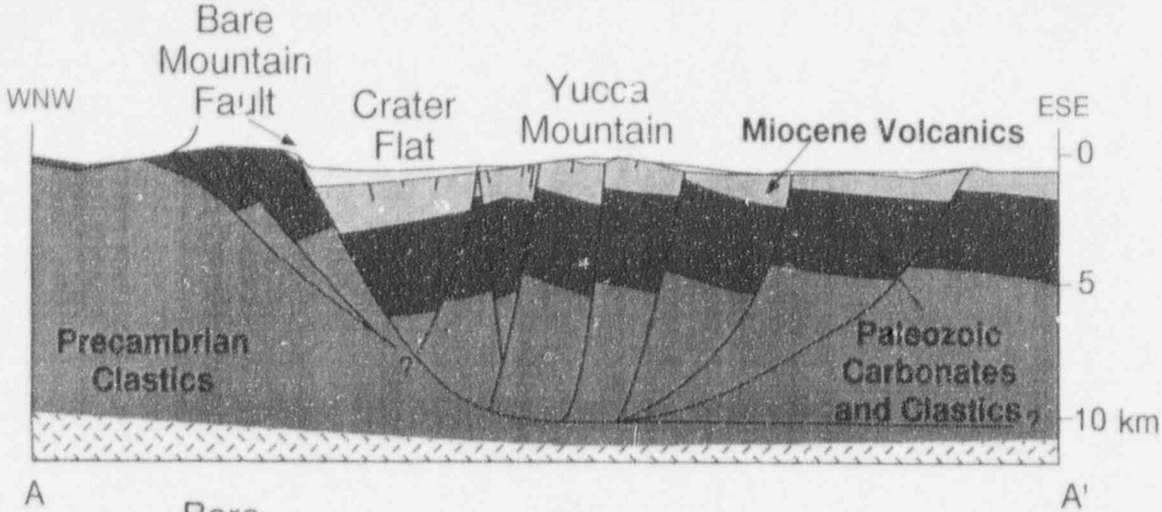
- Regional Structural Setting Of Basaltic Volcanoes Near Yucca Mountain
- Recent Geophysical Surveys And Their Significance
- Summary of Geologic Factors to be Included in Probability Models

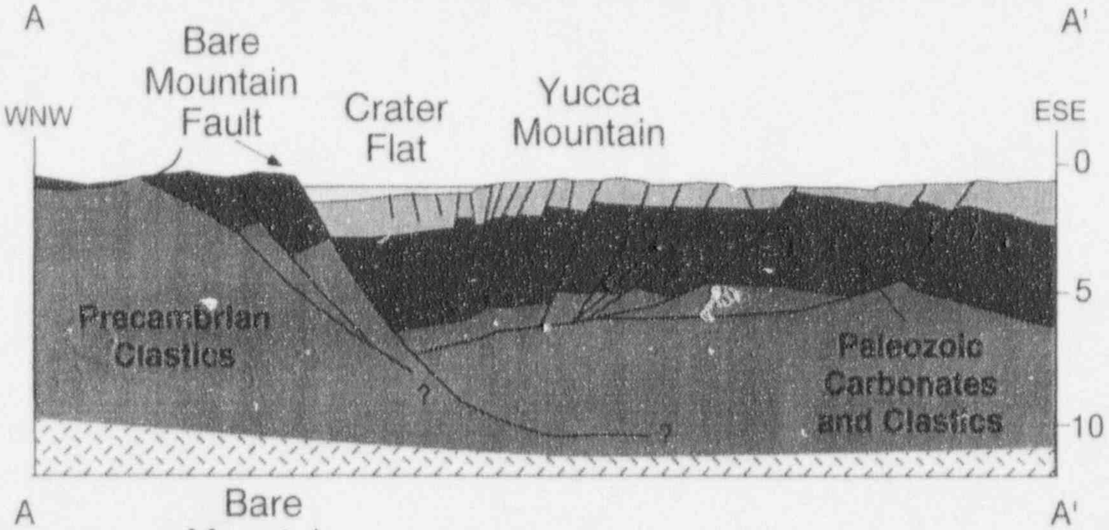
EXTENT OF YMR VOLCANIC SYSTEM

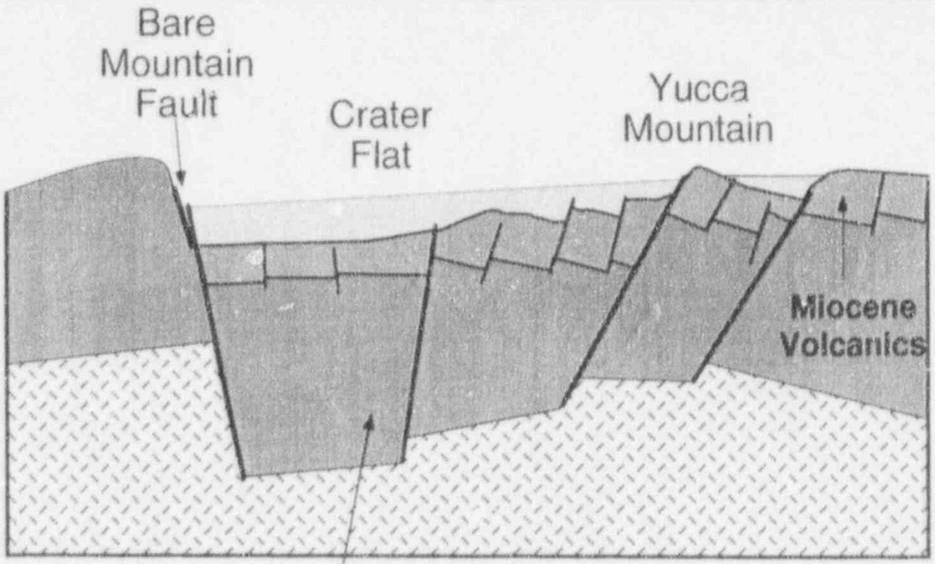


- Need a geologic basis for system definition: spatial, temporal, structural, isotopic
- Funeral Fm. basalts = steady-state recurrence <6 Ma

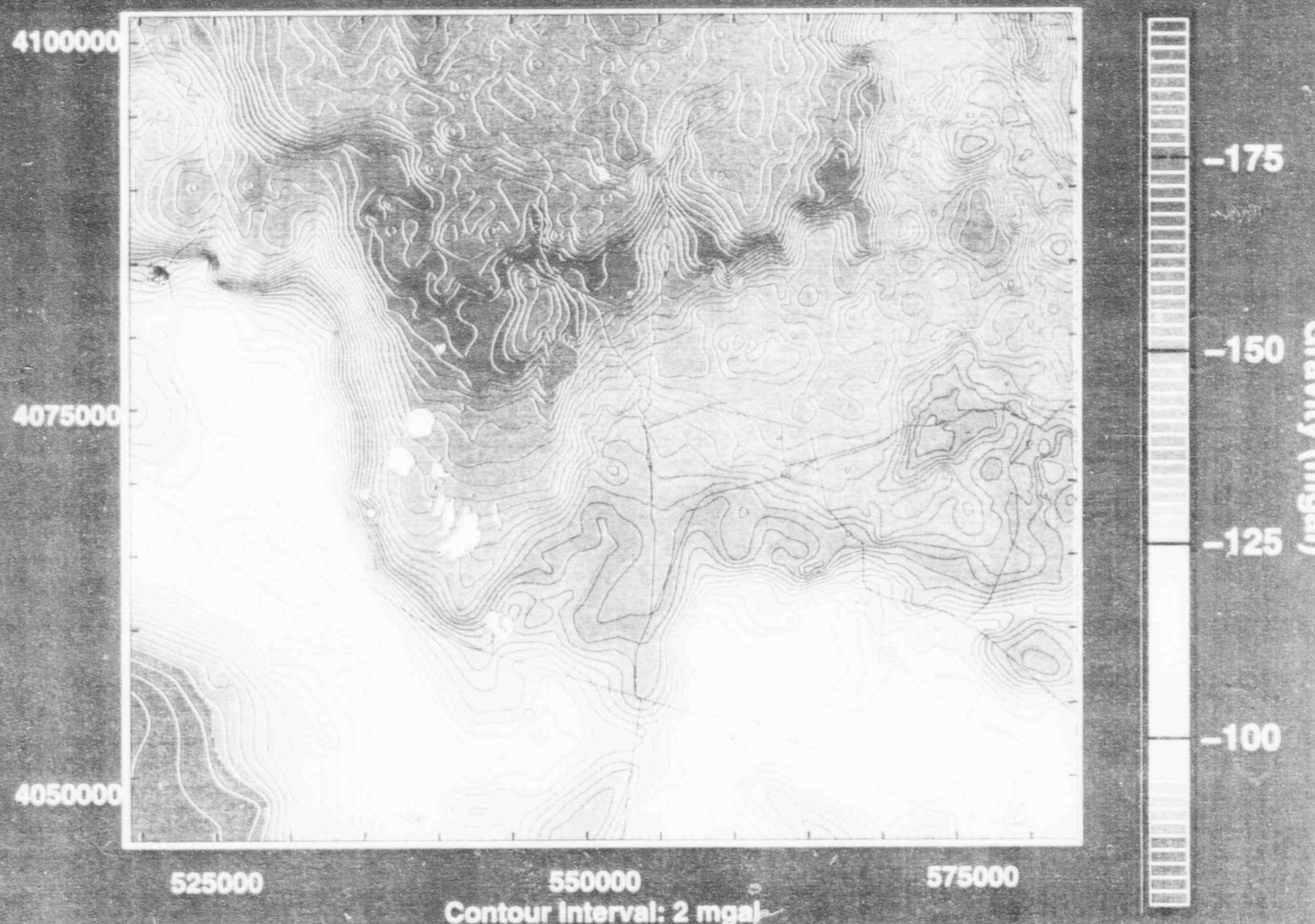
Cross-Sections Through Crater Flat and Yucca Mountain



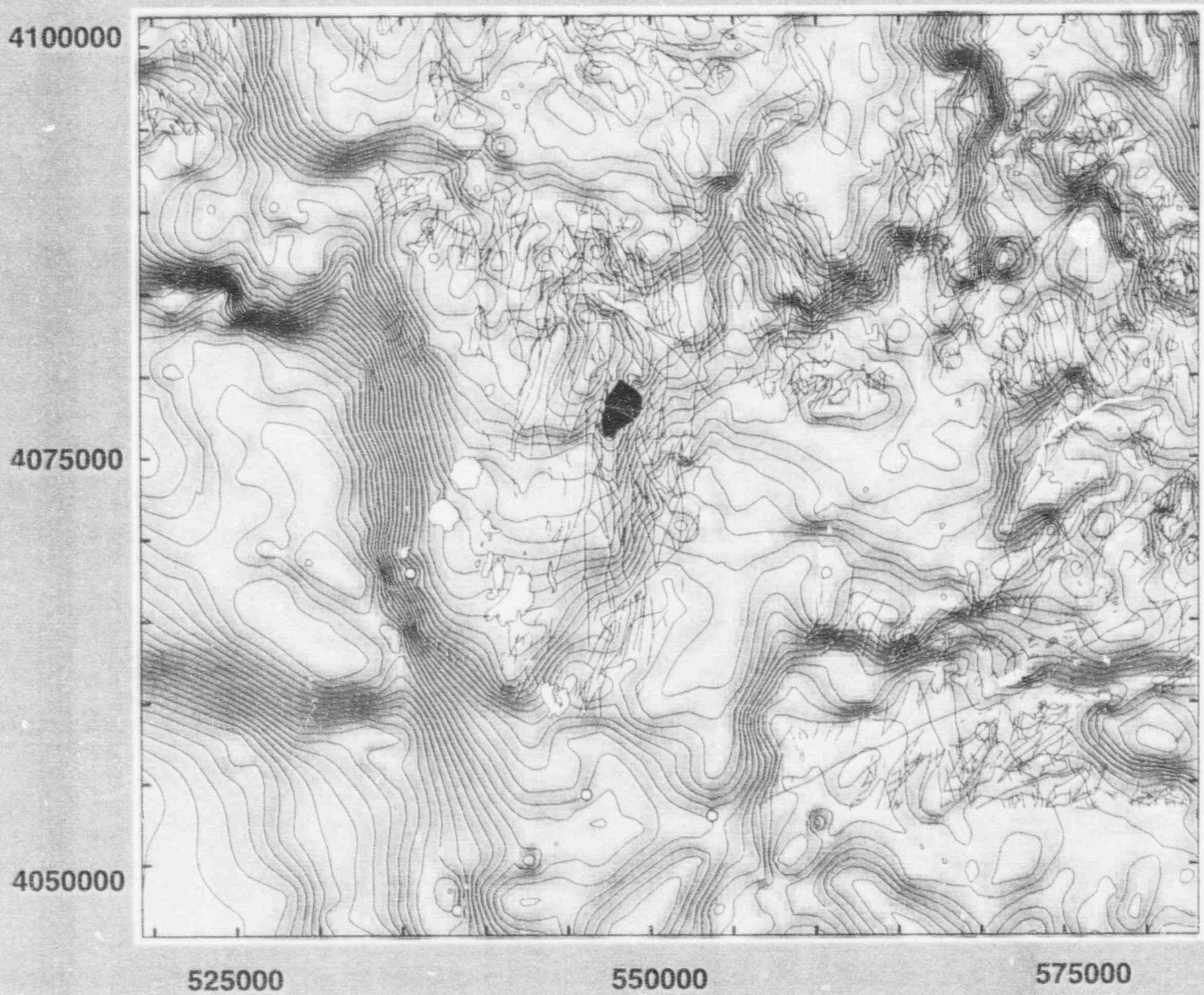




Comparison of Gravity Anomalies and Volcano Locations in the Yucca Mountain Area



Comparison of Horizontal Gravity Gradient, Volcano and Fault Locations in the Yucca Mountain Area



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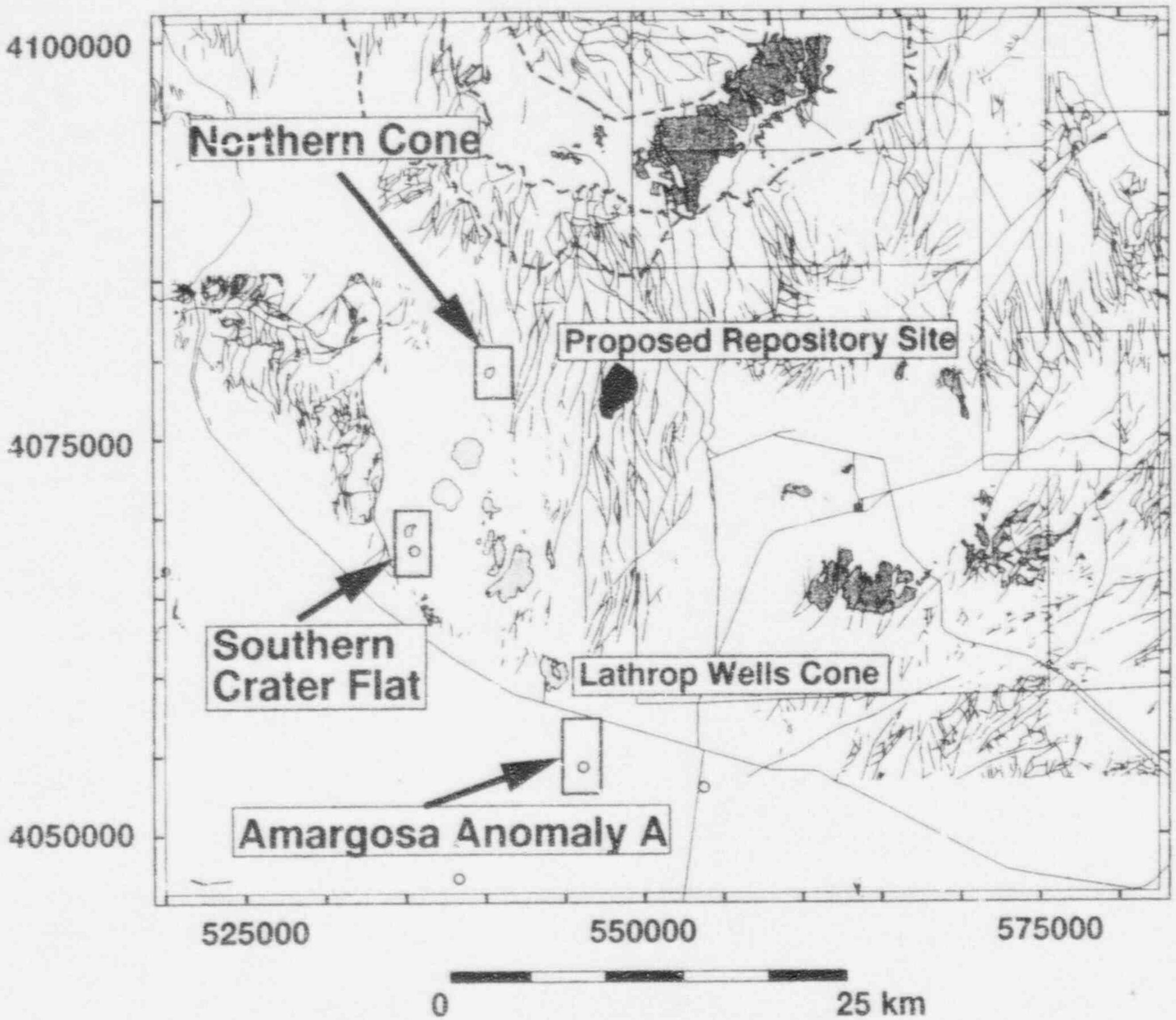
CNWRA Ground Magnetic Surveys



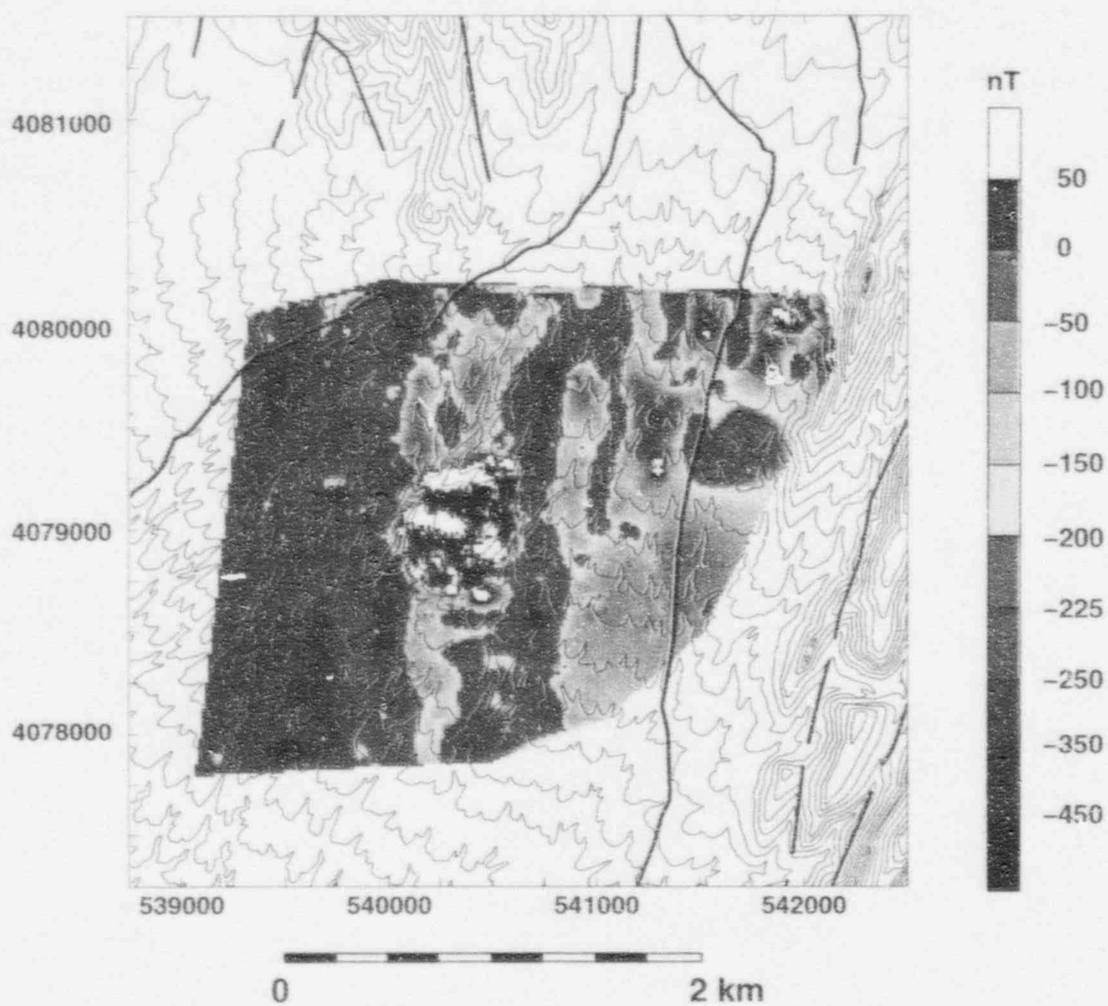
Three detailed surveys:

Southern Crater Flat

Amargosa Valley Anomaly A

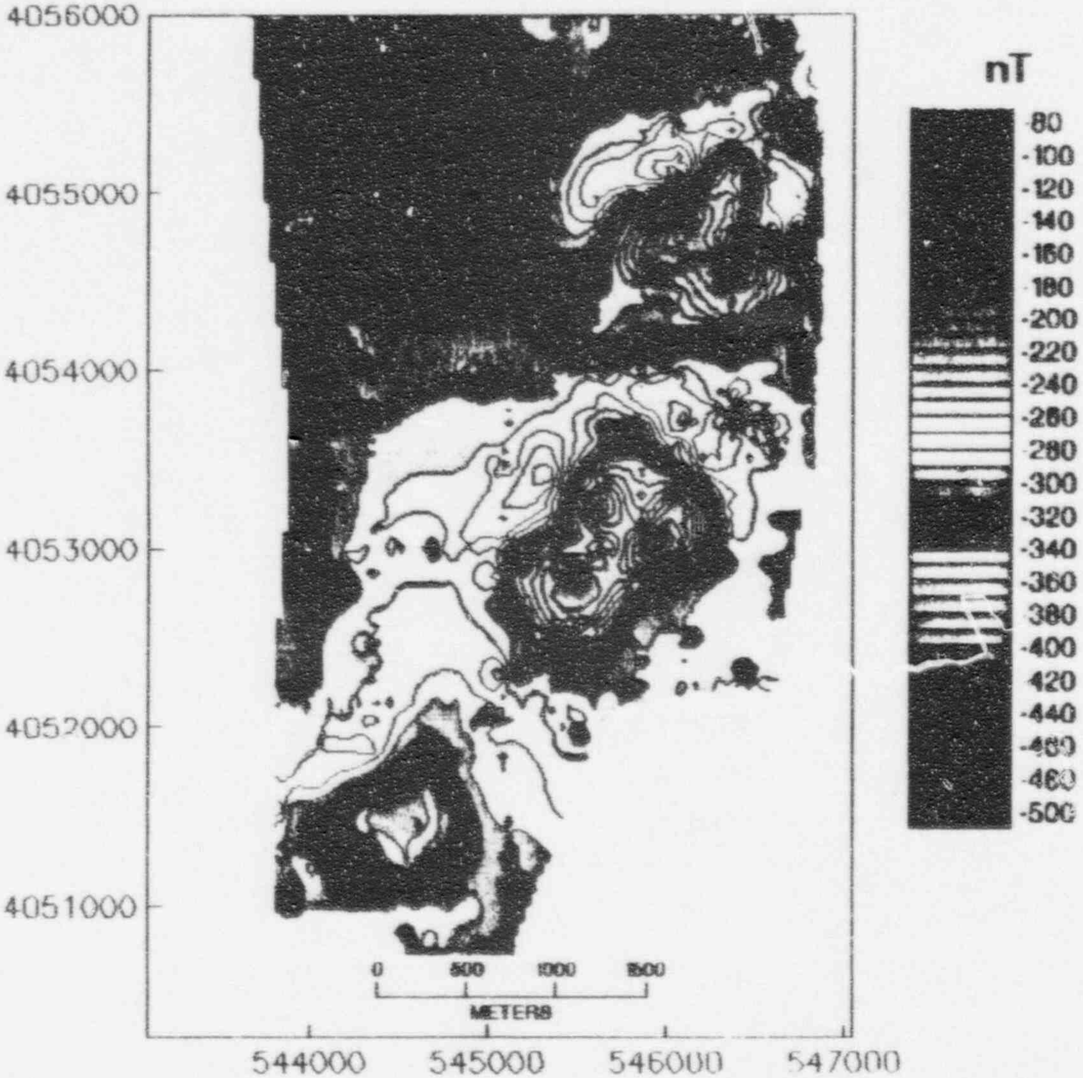
Northern Cone

CNWRA Ground Magnetic Surveys



Northern Cone Survey illustrates the relationship between basaltic volcanoes and faults

CNWRA Ground Magnetic Surveys



Ground Magnetic Survey of Amargosa Anomaly A

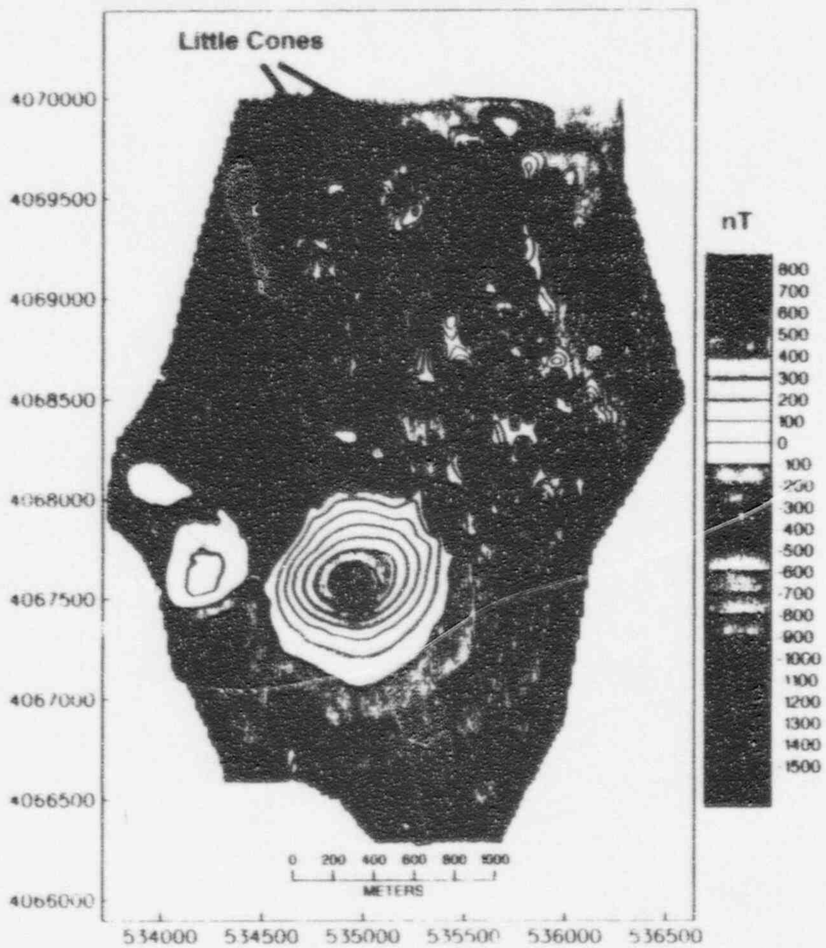
Reveals three distinct reversely polarized anomalies

Interpreted as buried basaltic volcano edifices

Further evidence of the importance of NE trending alignments

CNWRA Ground Magnetic Surveys

Southern Crater Flat:



Three magnetic anomalies identified and mapped in southern Crater Flat

Little Cones comparable in volume to Red and Black Cones

Possible buried volcano at 150 to 200 m

Dike trending parallel to Bare Mountain Fault

SUMMARY OF GEOLOGIC SETTING OF VOLCANISM

PROBABILITY MODELS OF VOLCANISM SHOULD ACCOUNT FOR THE FOLLOWING FEATURES:

- CLUSTERED VOLCANISM IN CRATER FLAT
- ASSOCIATION OF VOLCANOES AND FAULTS
- PREVALENCE OF NE-TRENDING VENT ALIGNMENTS
- LOW AND PERSISTENT RECURRENCE RATE

λ_n * λ_s^w * λ_d

Estimate nonhomogenous model

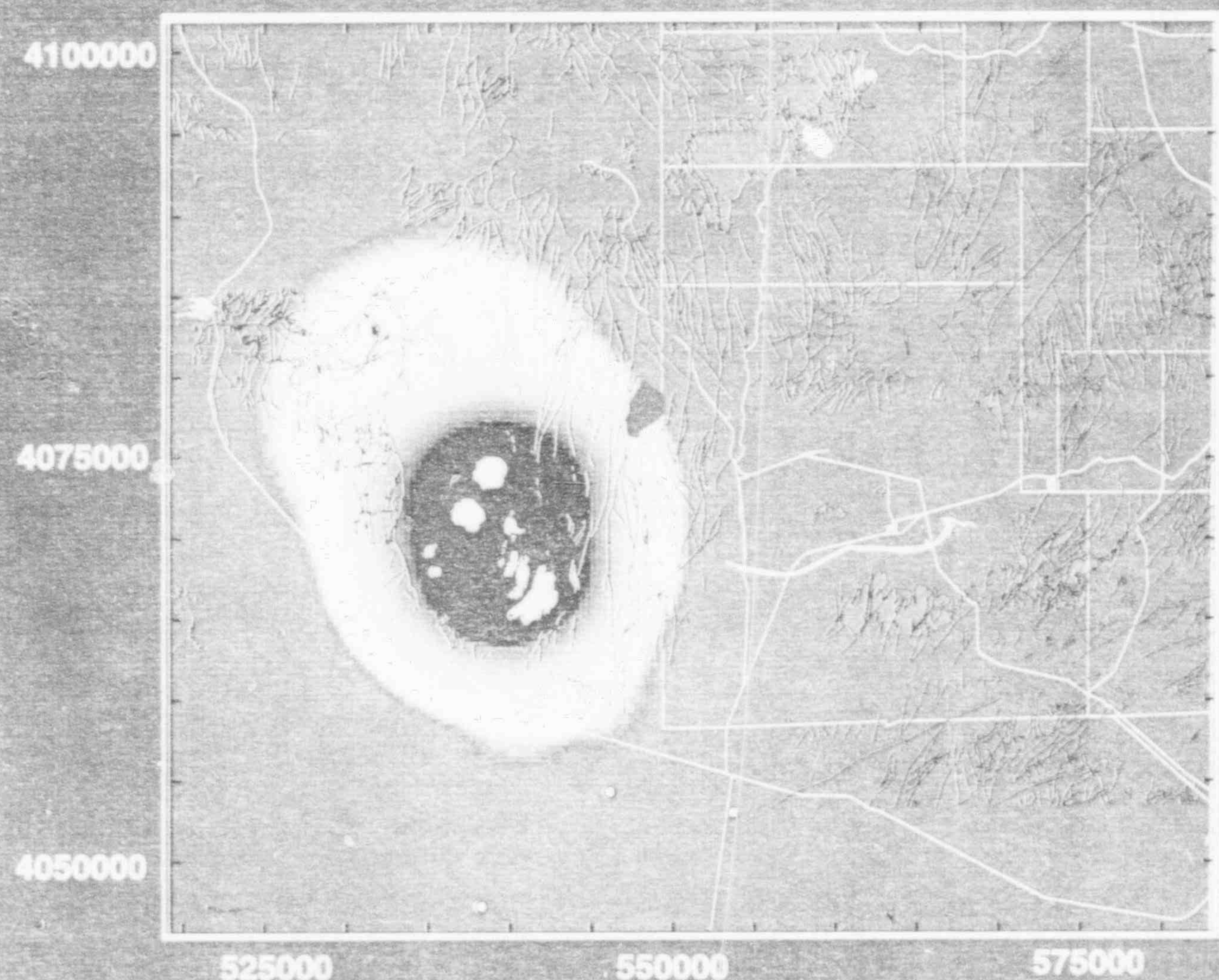
Estimate of structures that enhance magma ascent, weighted by *w*

Estimate of dike geometries that can intersect repository

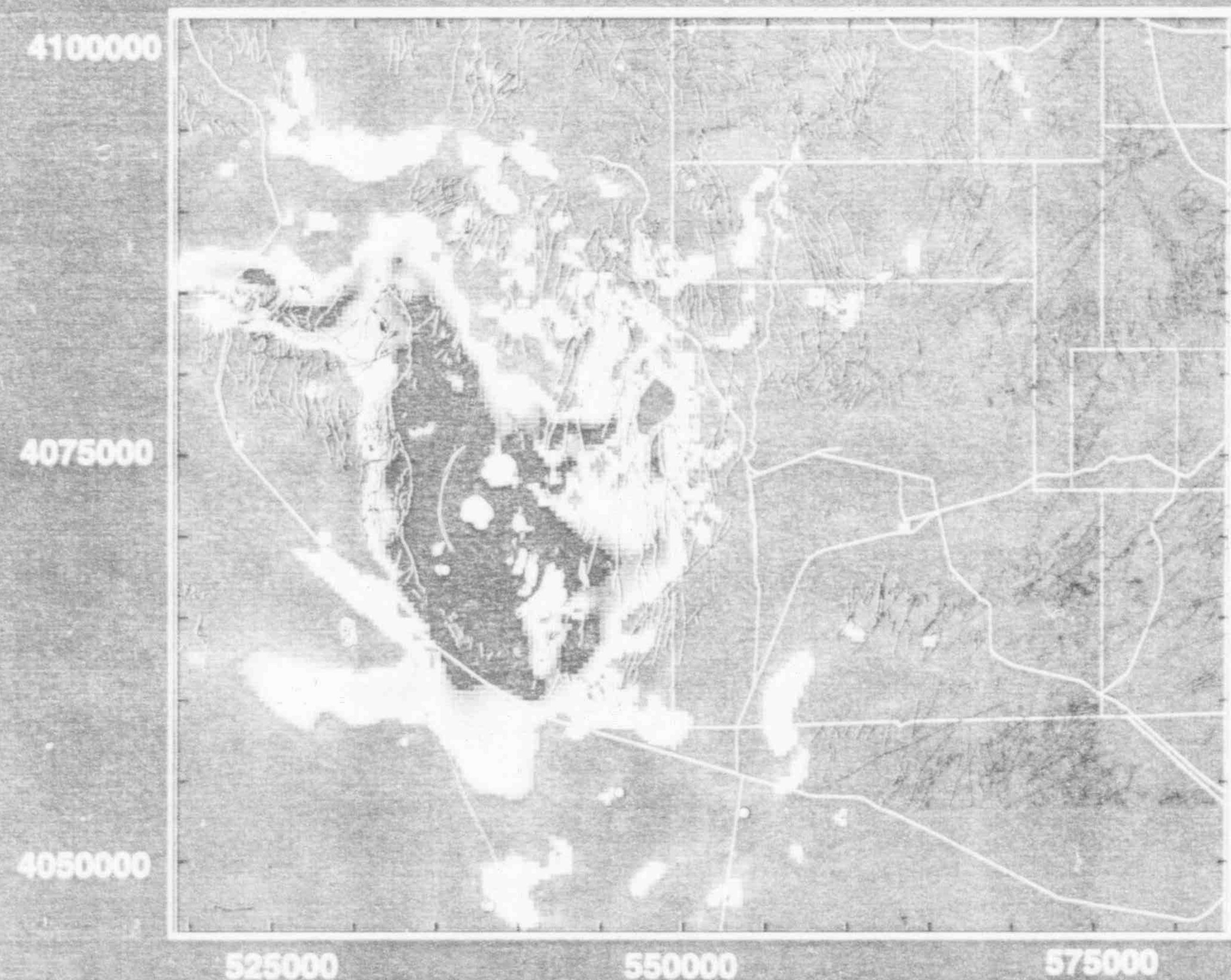
* λ_t → P[Volcanic Disruption]

Estimate of the regional recurrence rate of volcanism

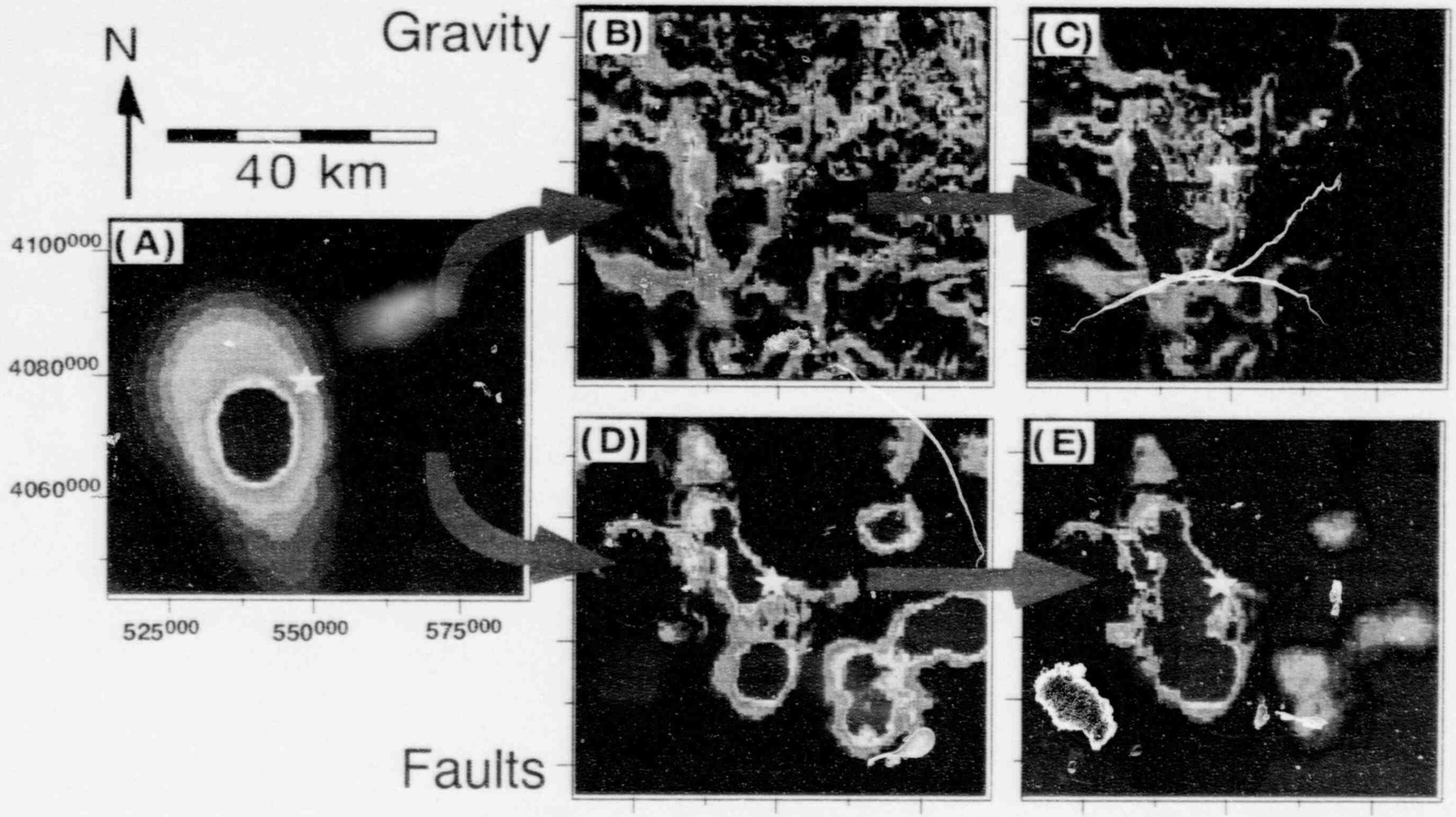
Comparison of Near-Neighbor Probability Model, Faults, and Volcano Locations in the Yucca Mountain Area



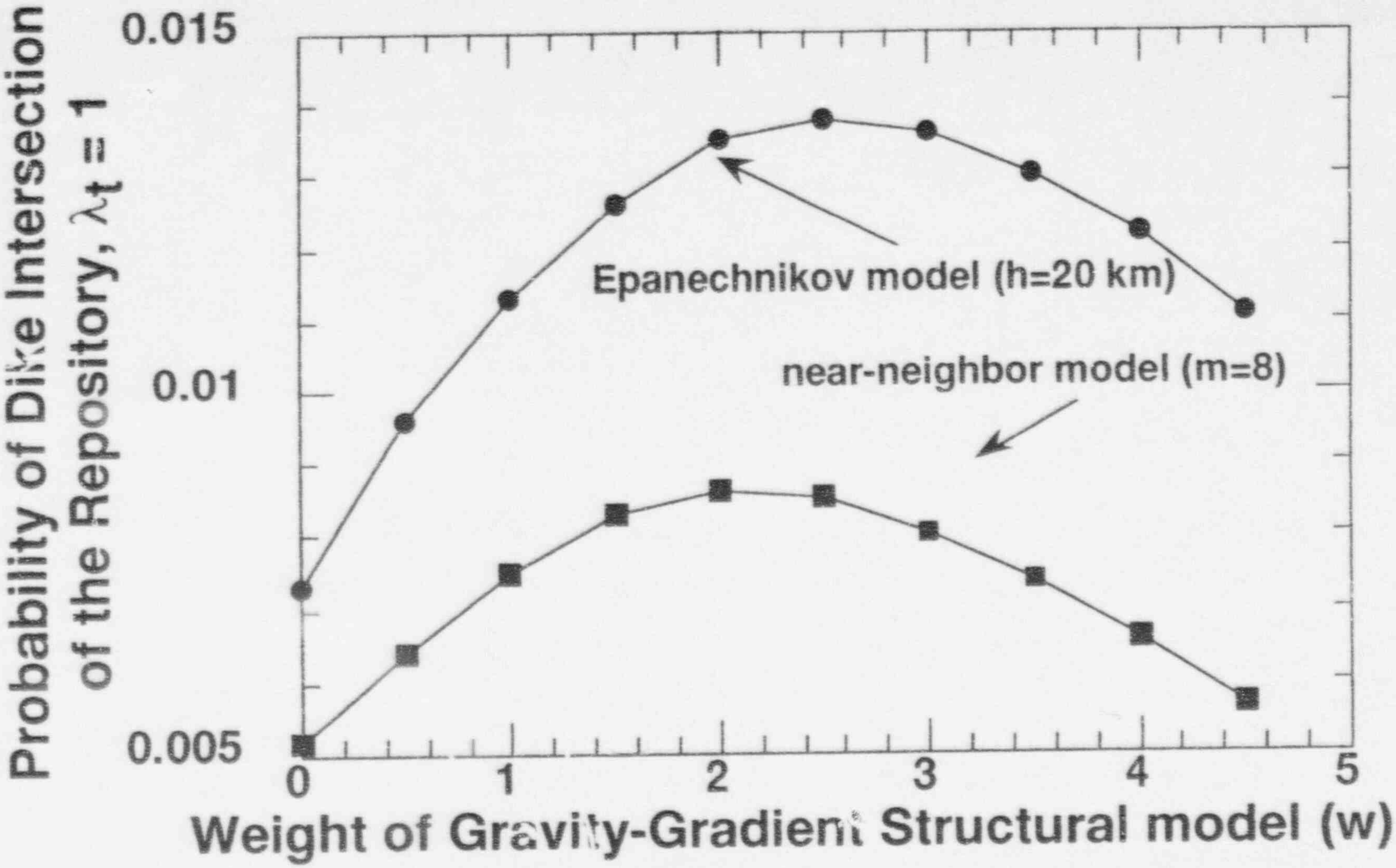
Near-Neighbor Probability Model Weighted by Horizontal Gravity Gradient



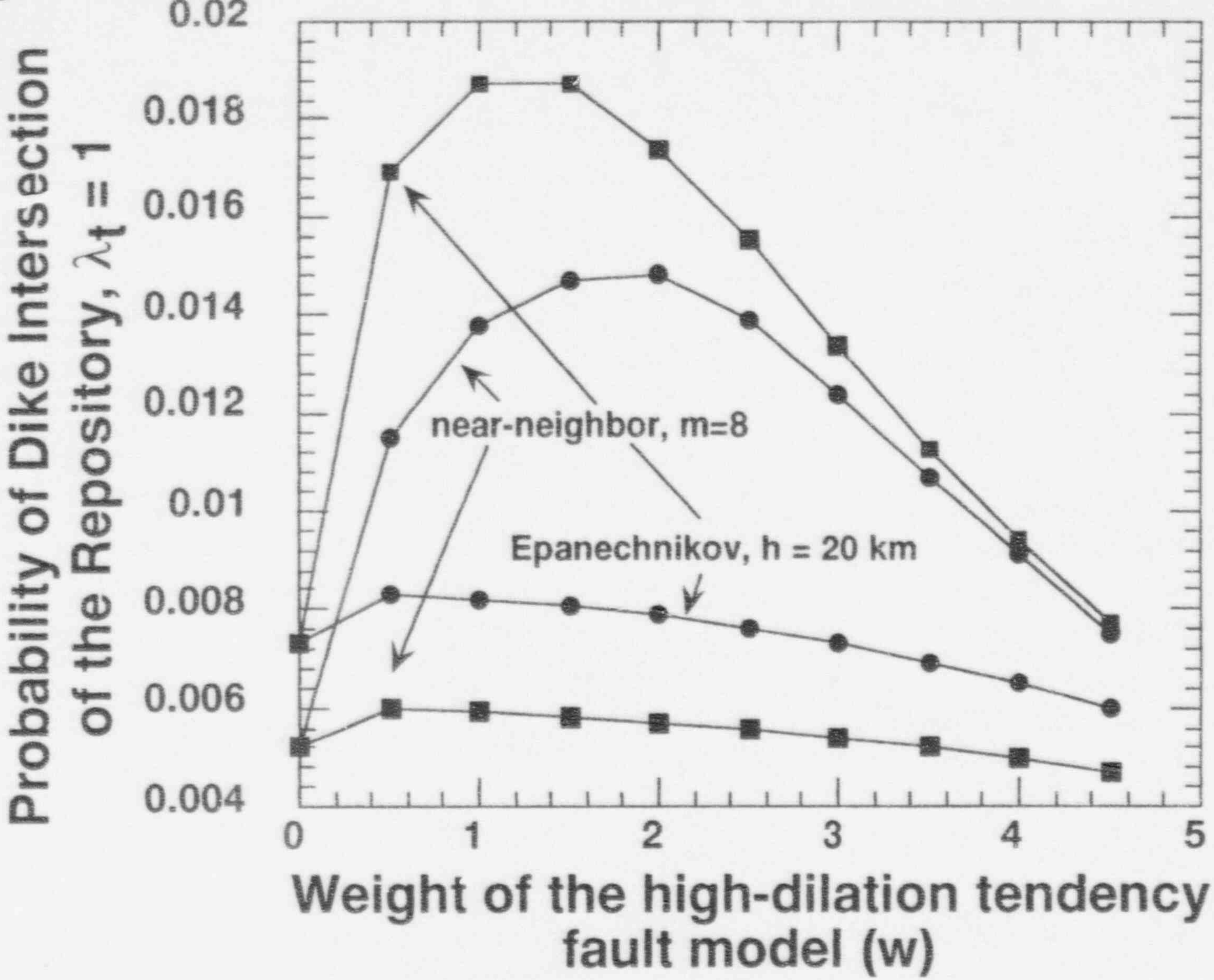
INTEGRATED VOLCANO-STRUCTURE PROBABILITY



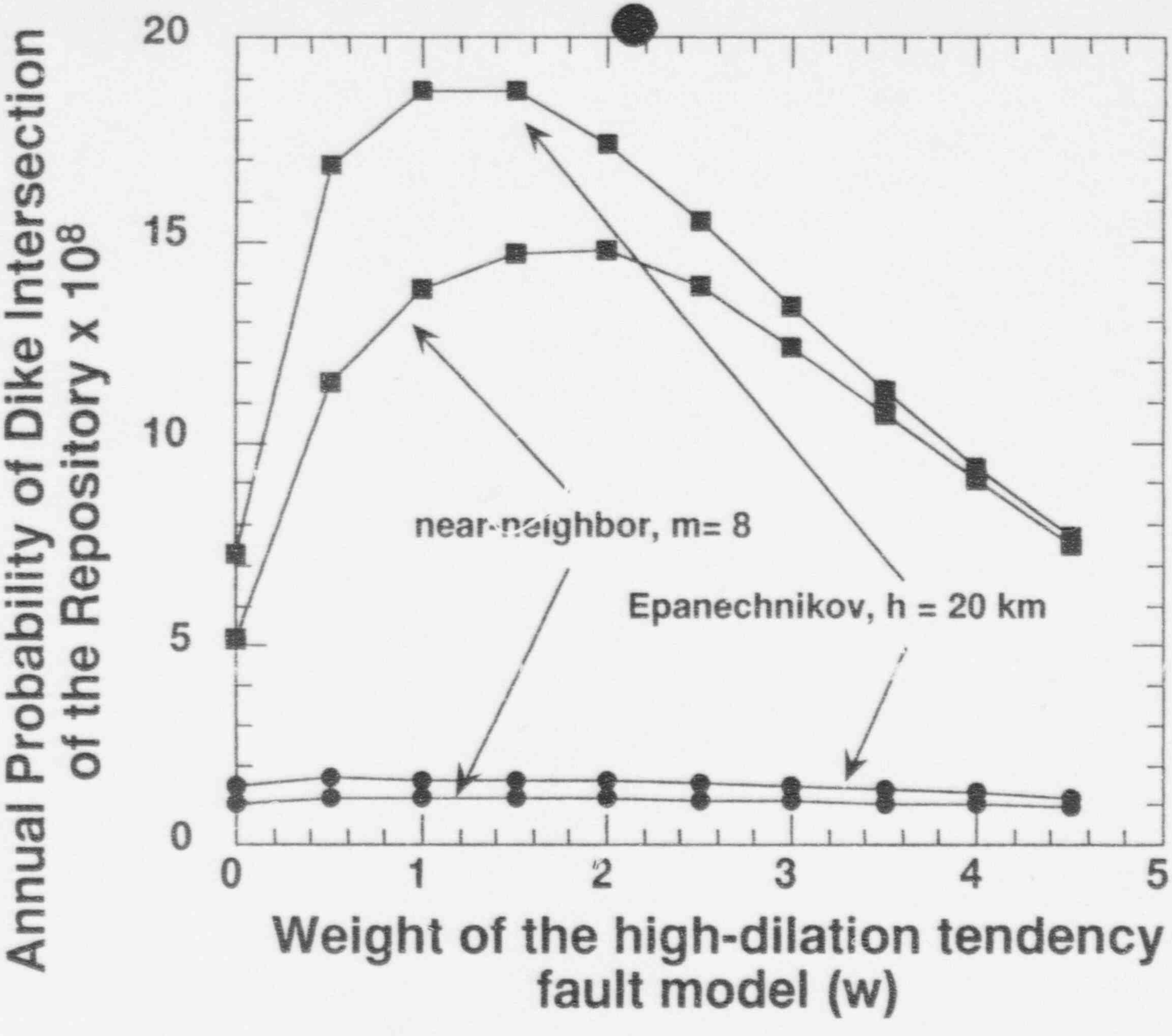
Nonhomogeneous probability models (A) are convolved with the amplitude of the horizontal gravity-gradient (B), and the density of high-dilation tendency faults (D) to make geologically dependent probability maps (C and E).



Probability of Dike Intersection
of the Repository, $\lambda_t = 1$
0.015
0.01
0.005
0
1
2
3
4
5
Weight of Gravity-Gradient Structural model (w)
Epanechnikov model (h=20 km)
near-neighbor model (m=8)



Probability of Dike Intersection of the Repository, $\lambda_t = 1$
0.02
0.018
0.016
0.014
0.012
0.01
0.008
0.006
0.004
near-neighbor, m=8
Epanechnikov, h = 20 km
0
1
2
3
4
5
Weight of the high-dilation tendency fault model (w)



Annual Probability of Dike Intersection of the Repository x 10^8
20
15
10
5
0
0
1
2
3
4
5
near-neighbor, m= 8
Epanechnikov, h = 20 km
Weight of the high-dilation tendency fault model (w)

CONCLUSIONS

- Plio-Quaternary basaltic volcanoes near YM correlate well with high-dilation tendency faults and regionally with vertical displacements in basement, indicated by gravity gradients

- Including structural control in volcanic hazard models increases the probability of volcanic disruption of the proposed repository compared to models that do not include structure

- These models indicate a range of probability of volcanic disruption of the repository between 1×10^{-7} and 1×10^{-8} /yr

3

YUCCA
MOUNTAIN
PROJECT

Studies

Igneous Activity Program

Introduction

Presented to:
91st ACNW Meeting

Presented by:
Tim Sullivan
Viability Assessment Team Leader
Yucca Mountain Site Characterization Office

April 22, 1997

U.S. Department of Energy
Office of Civilian Radioactive
Waste Management

acwn4_97

VOLCANISM PROGRAM STATUS

- Volcanism Status Report was issued in 1995
- Data analysis was completed in 1996 and will be reported in the Volcanism Synthesis Report, currently in final revision
- Site Description (PISA) will provide an integrated discussion of our understanding of the regional and site Geology including all information relevant to igneous activity

VOLCANISM PROGRAM STATUS

- DOE has decided to close out site characterization data collection for volcanism based on
  - low disruption probability results from PVHA
  - insensitivity of PVHA results to new data
  - DOE performance assessment results are not sensitive to direct volcanic disruption of the repository because of the low annual probability
  - modeling of the direct and indirect effects of eruption and intrusion to date have indicated little effect on site performance

VOLCANISM PROGRAM STATUS

- DOE will evaluate new information and conduct additional analyses as needed to ensure we maintain a sound defensible position
- Additional consequence analyses based on available information are planned as part of TSPA-VA
- TSPA-VA plans will be described in a later presentation

VOLCANIC HAZARD ANALYSIS

- DOE has completed evaluation of the probability of disruption of the repository and quantified the associated uncertainties based on elicitation of 10 subject matter experts in volcanism
  - final report completed in June 1996
  - intended to provide a sound defensible basis for licensing
- PVHA results
  - mean disruption probability -- 1.5×10^{-8}
  - 90% confidence interval -- 5.4×10^{-10} to 4.9×10^{-8}
  - bounds -- 10^{-10} to 10^{-7}

IGNEOUS ACTIVITY TECHNICAL EXCHANGE

- NRC presented data and analyses conducted after the completion of the PVHA
- DOE agreed to evaluate this new data through hazard sensitivity studies
- Preliminary results will be presented today

IGNEOUS ACTIVITY TECHNICAL EXCHANGE

- NRC presented basis for concluding probability of future volcanic events ranges between 10^{-8} and 10^{-7}
- This range differs from DOE's PVHA result but is included within the bounds of the full probability distribution
- It remains DOE's position that PVHA provides a defensible basis for characterizing the disruption probability

IGNEOUS ACTIVITY TECHNICAL EXCHANGE

- DOE will describe how this probability distribution will be used in performance assessment
- This should support closure of the probability subissue

IGNEOUS ACTIVITY TECHNICAL EXCHANGE

- NRC presented dose calculations for a volcanic eruption through the repository with associated tephra and radionuclide dispersion
- Risk result indicated an average annual individual dose of 0.5mrem/yr
- DOE has also concluded risk of volcanism is not significant
- DOE/NRC results are converging--this should lead to closure of consequence subissue

PROBABILISTIC VOLCANIC HAZARD ANALYSIS FOR YUCCA MOUNTAIN, NEVADA

Kevin J. Coppersmith

Geomatrix Consultants
San Francisco, CA

91st ACNW Meeting
April 22, 1997
Rockville, MD

PROBABILISTIC VOLCANIC HAZARD ANALYSIS (PVHA) FOR YUCCA MOUNTAIN, NEVADA

OBJECTIVES OF THE STUDY:

1. To assess the probability of disruption by a volcanic event of the proposed repository
2. To quantify the uncertainties associated with this assessment

Disruption: the physical intersection of magma with the potential repository volume
Volcanic event: both eruptive and intrusive features
Probability: annual frequency

ASPECTS OF PVHA STUDY

Quantification of uncertainty was a central focus of the PVHA study. Important aspects of the PVHA uncertainty characterization are the following:

- **Attempting to capture all important elements of uncertainty, including expert-to-expert diversity of interpretation**
- **All of the experts were exposed to all pertinent data, researchers, and methods**
- **Ultimately, the reliance that an expert places on certain data, methods, experience is their prerogative**
- **Experts encouraged to consider full range of methods, data, analogue experience in addressing issues**
- **Focus is on uncertainty not "preferred" estimates; when in doubt, don't choose, weight the alternatives**
- **Formal process of expert elicitation followed**

PROCEDURAL GUIDANCE ON EXPERT ELICITATION

The PVHA was conducted in accordance with all recent guidance for expert elicitation studies, including:

NRC

Branch Technical Position on the Use of Expert Elicitation in the High-Level Radioactive Waste Program: Kotra, J.P., Lee, M.P., Eisenberg, N.A., and DeWispelare, A.R., 1996, U.S. Nuclear Regulatory Commission, NUREG-1563, 35p.

DOE

Principals and Guidelines for Formal Use of Expert Judgment by the Yucca Mountain Site Characterization Project: Department of Energy, Office of Civilian Radioactive Waste Management, Yucca Mountain Site Characterization Office, May, 1995, 10p.

DOE, NRC, EPRI

Recommendations for Probabilistic Seismic Hazard Analysis: Guidance on Uncertainty and Use of Experts: Senior Seismic Hazard Analysis Committee (SSHAC), U.S. Nuclear Regulatory Commission NUREG/CR-6372, 170p.

MEMBERS OF EXPERT PANEL
PVHA PROJECT

EXPERT	AFFILIATION
Dr. Richard W. Carlson	Carnegie Institution of Washington
Dr. Bruce M. Crowe	Los Alamos National Laboratory
Dr. Wendell A. Duffield	U.S. Geological Survey
Dr. Richard V. Fisher	Univ. California, Santa Barbara (Emeritus)
Dr. William R. Hackett	WRH Associates
Dr. Mel A. Kuntz	U.S. Geological Survey
Dr. Alexander R. McBirney	University of Oregon (Emeritus)
Dr. Michael F. Sheridan	State University of New York, Buffalo
Dr. George A. Thompson	Stanford University
Dr. George P.L. Walker	University of Hawaii

SPECIALISTS (NOT ON THE EXPERT PANEL) INVOLVED IN PVHA WORKSHOPS AND FIELD TRIPS

• Duane Champion	USGS
• Chuck Connor	CNWRA
• Allin Cornell	Stanford
• Paul Delaney	USGS
• Jim Faulds	U. of Iowa
• Robert Fleck	USGS
• Chris Fridrich	USGS
• John Geissman	UNM
• Brittain Hill	CNWRA
• C.-H. Ho	UNLV
• Bruce Judd	SDG
• Vicky Langenheim	USGS
• Les McFadden	UNM
• Chris Menges	USGS
• Scott Minor	USGS
• Peter Morris	ADA
• Paul Orkild	USGS
• Frank Perry	LANL
• Gene Smith	UNLV
• Richard Smith	INEL
• Jack Stewart	USGS
• Brent Turrin	USGS
• Steve Wells	UCR
• John Wesling	Geomatrix
• Gene Yogodzinski	UNLV

WORKSHOPS AND ACTIVITIES
PVHA Project

ACTIVITY	TOPIC/FOCUS	DATE
Workshop #1	Data Needs	February 1995
Field Trip #1	*Crater Flat*	*March 1995*
Workshop #2	Alternative Hazard Models	March 1995
Field Trip #2	*Sleeping Butte/Lathrop Wells*	*April 1995*
Workshop #3	Alternative Interpretations	May 1995
Elicitations	Individual Interviews	June-July 1995
Workshop #4	Feedback of Interpretations	December 1995

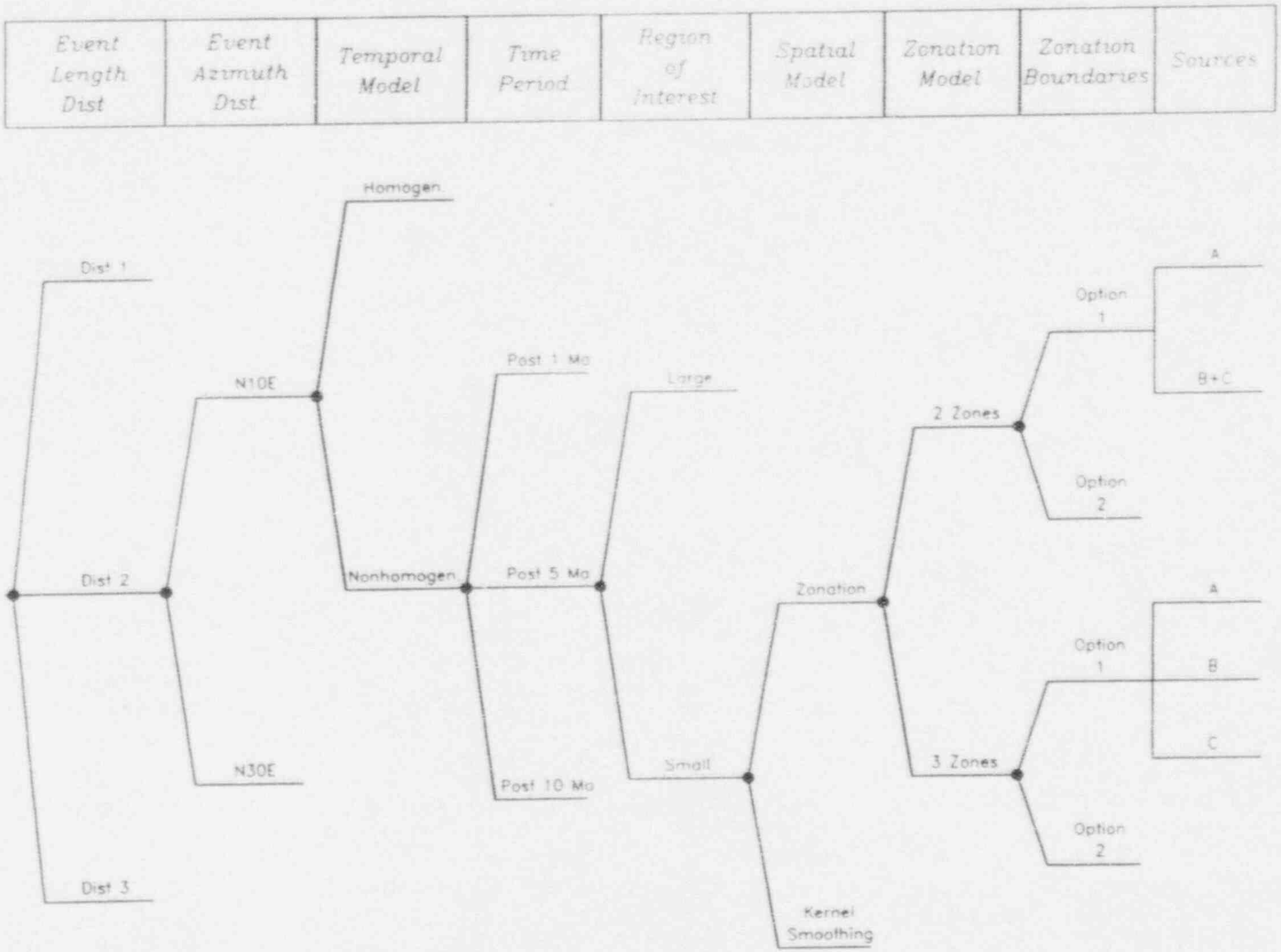


Figure 3-15 General logic tree structure used to construct PVHA computation model.

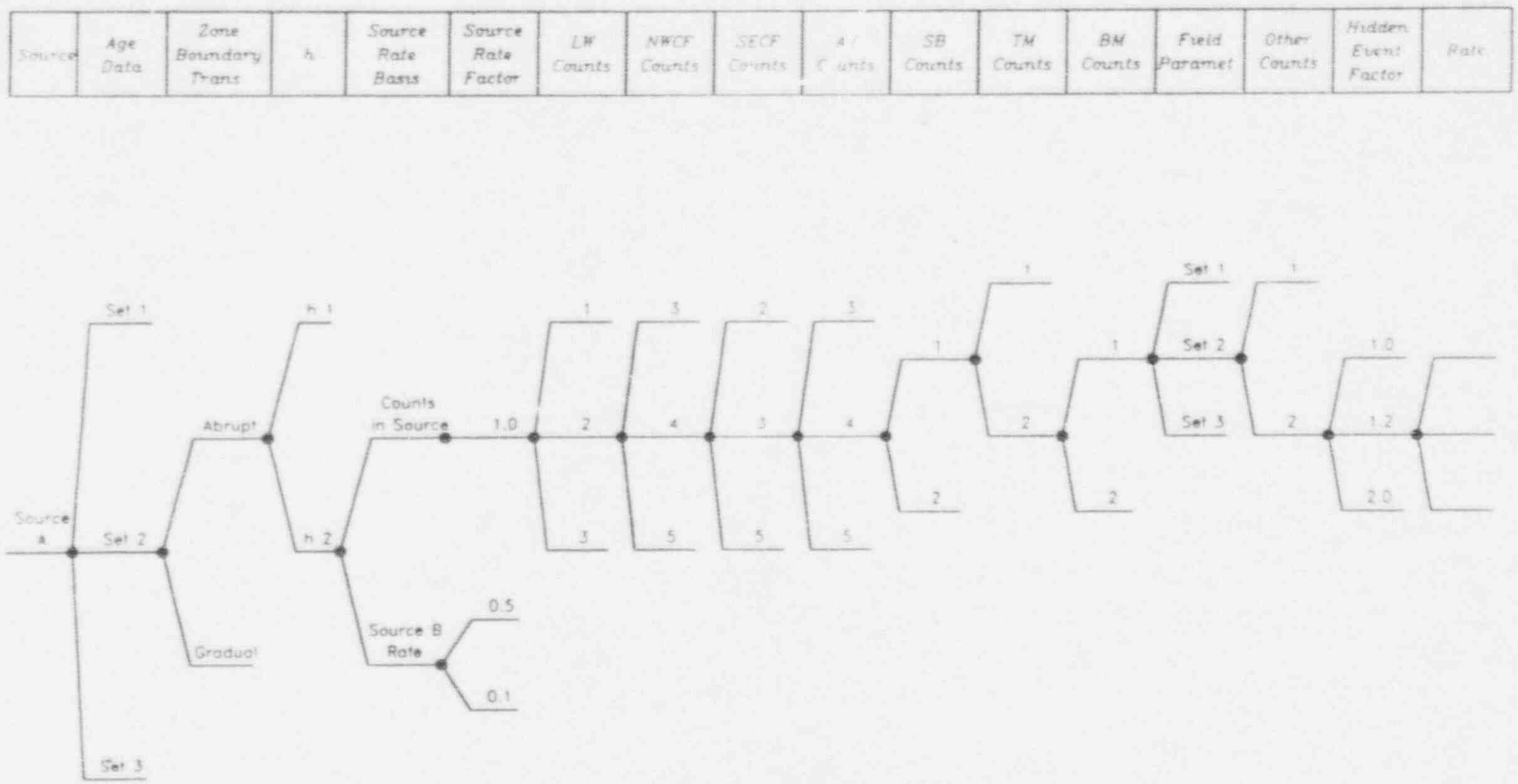


Figure 3-16 Logic tree structure for subtrees addressing the uncertainty in modeling the hazard from specific sources. These subtrees are attached to the overall logic tree shown on Figure 3-15.

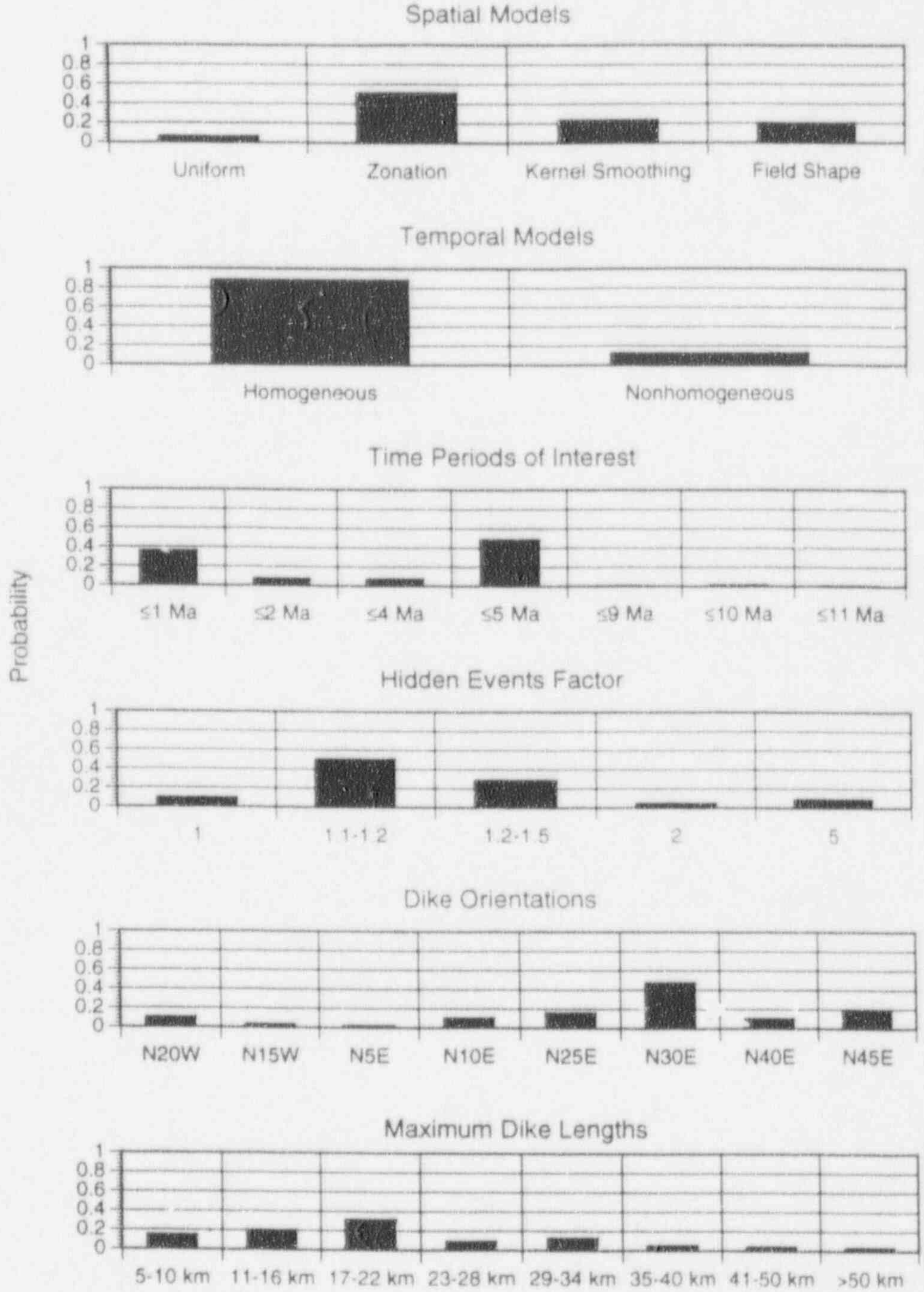


Figure 3-62 Summary of experts' assessments for components of the PVHA model.

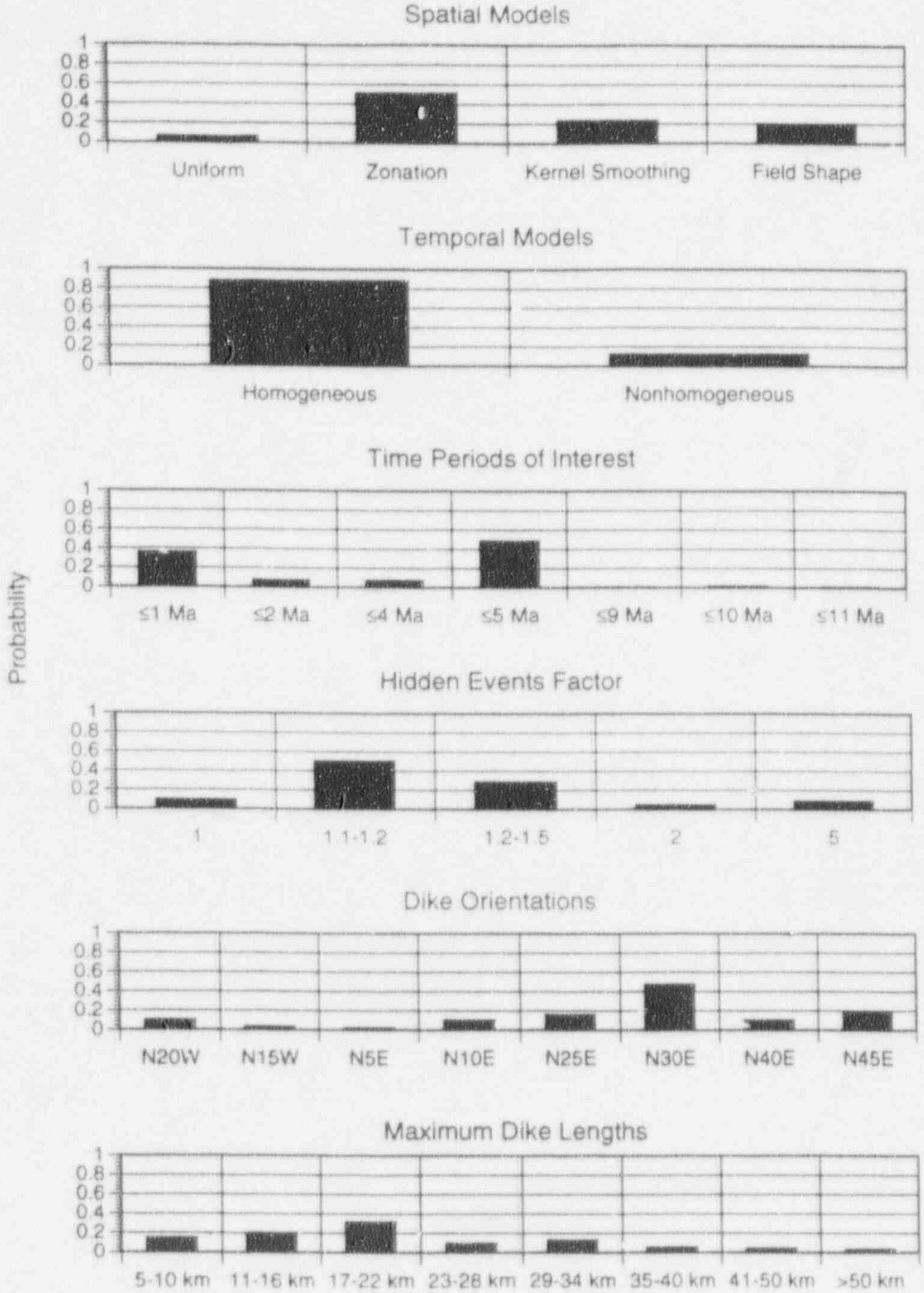


Figure 3-62 Summary of experts' assessments for components of the PVHA model.

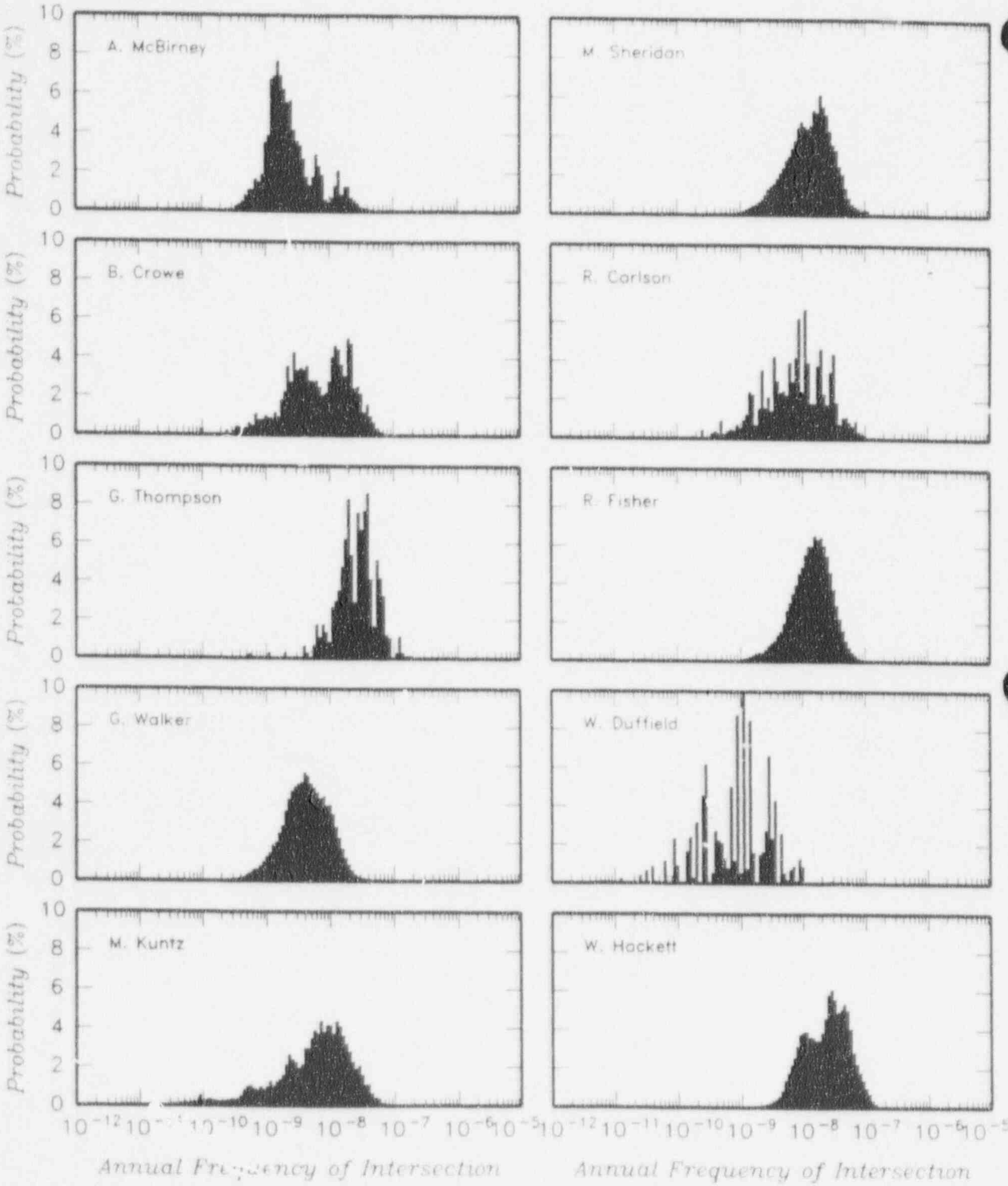


Figure 4-31 Comparison of individual expert distributions for frequency of intersection.

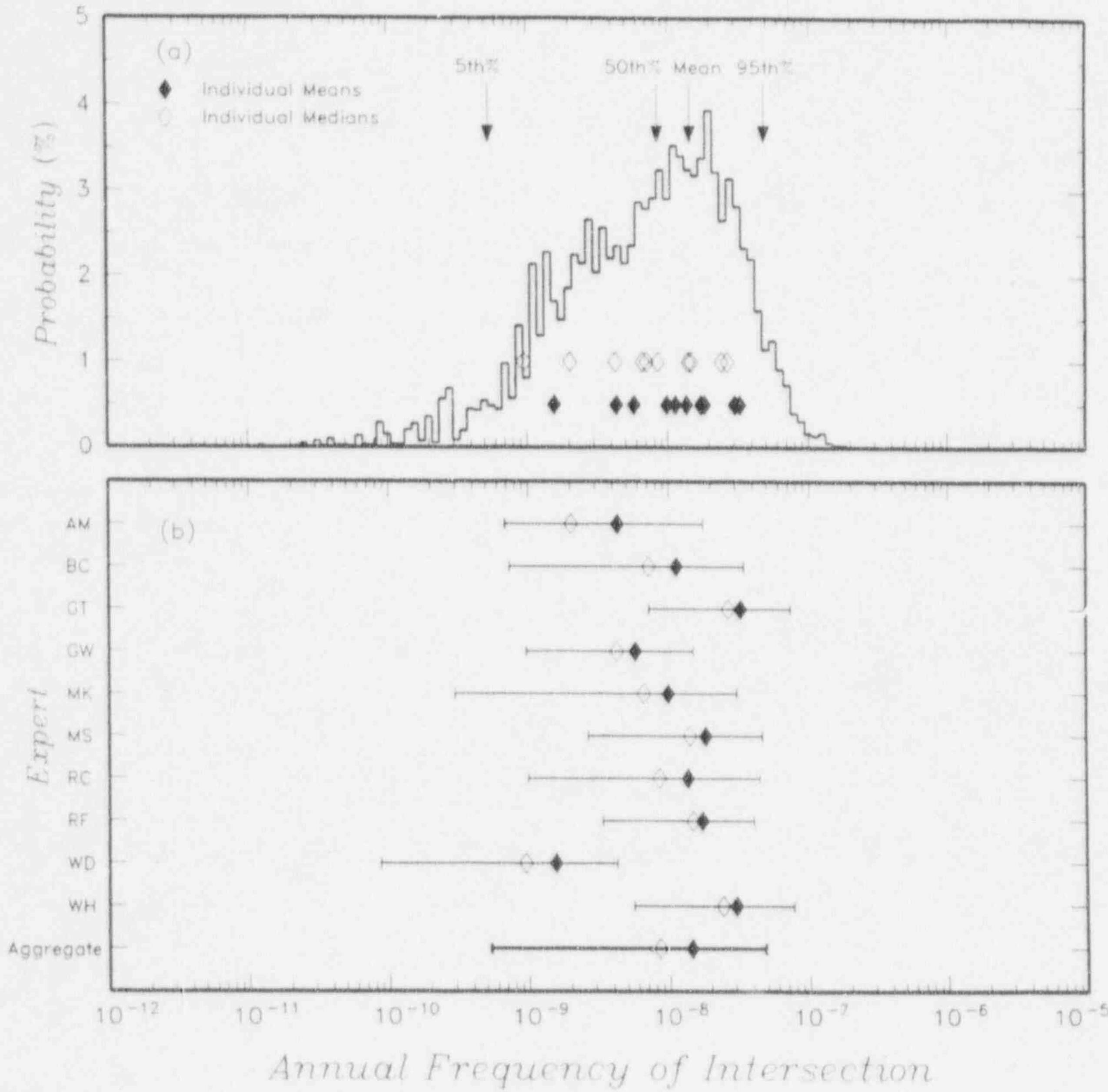


Figure 4-32 Aggregate results for frequency of intersecting the Yucca Mountain repository foot print by a volcanic event. (a) Aggregate distribution for frequency of intersection. (b) Individual and aggregate means, medians, and 90-percent confidence intervals (horizontal bars).

PROBABILISTIC VOLCANIC HAZARD ANALYSIS (PVHA) FOR YUCCA MOUNTAIN, NEVADA

CONCLUSIONS

- The PVHA was conducted to evaluate the probability of intersection of the repository by a volcanic event, including the uncertainty in the assessment
- Uncertainties were quantified using a panel of ten experts and incorporating alternative models and parameter values
- Consideration of available data, methods, analogues, and processes led to alternative spatial models, temporal models, and procedures for their calculation
- Annual frequency of intersection spans 1.5 - 2 orders of magnitude for each expert; 3 orders of magnitude for entire panel
- Rate parameter, choice of spatial model, smoothing constant, and counts in NW Crater Flat are significant contributors to uncertainty
- Mean frequency of intersection is 1.5×10^{-8}, with a 90% confidence interval of 5.4×10^{-10} to 4.9×10^{-8}

EVALUATION OF NEW DATA BY PVHA SENSITIVITY ANALYSIS

PREMISE: The focus of the PVHA was to quantify the knowledge and uncertainty in the annual frequency of dike intersection. The result is a robust, quantitative assessment. The significance of new data will be evaluated using sensitivity analyses relative to the PVHA probability distribution function (PDF).

- Multi-expert studies emphasizing uncertainty quantification are inherently robust
- PVHA experts devoted considerable effort to evaluating the existing data, testing alternative models and hypotheses, and incorporating modeling and parameter uncertainties
- Data collection for DOE's volcanism program has ended; new data gathered by other groups will be evaluated by sensitivity analysis for their significance to the PDF (changes in the mean estimate)
- New data are evaluated for 1) their implications, 2) comparison to the assessments by the experts, and 3) their quantitative implications to the PDF.

EVALUATION OF NEW DATA
BY PVHA SENSITIVITY ANALYSIS (cont'd)

DOE has evaluated the significance of two new data sets provided by the NRC in the February Technical Exchange on Igneous Activity:

1. An increased volume estimate of Little Cones, based on modeling of ground magnetics data.

2. An additional buried volcanic feature in Amargosa Valley, based on modeling of ground magnetics data.

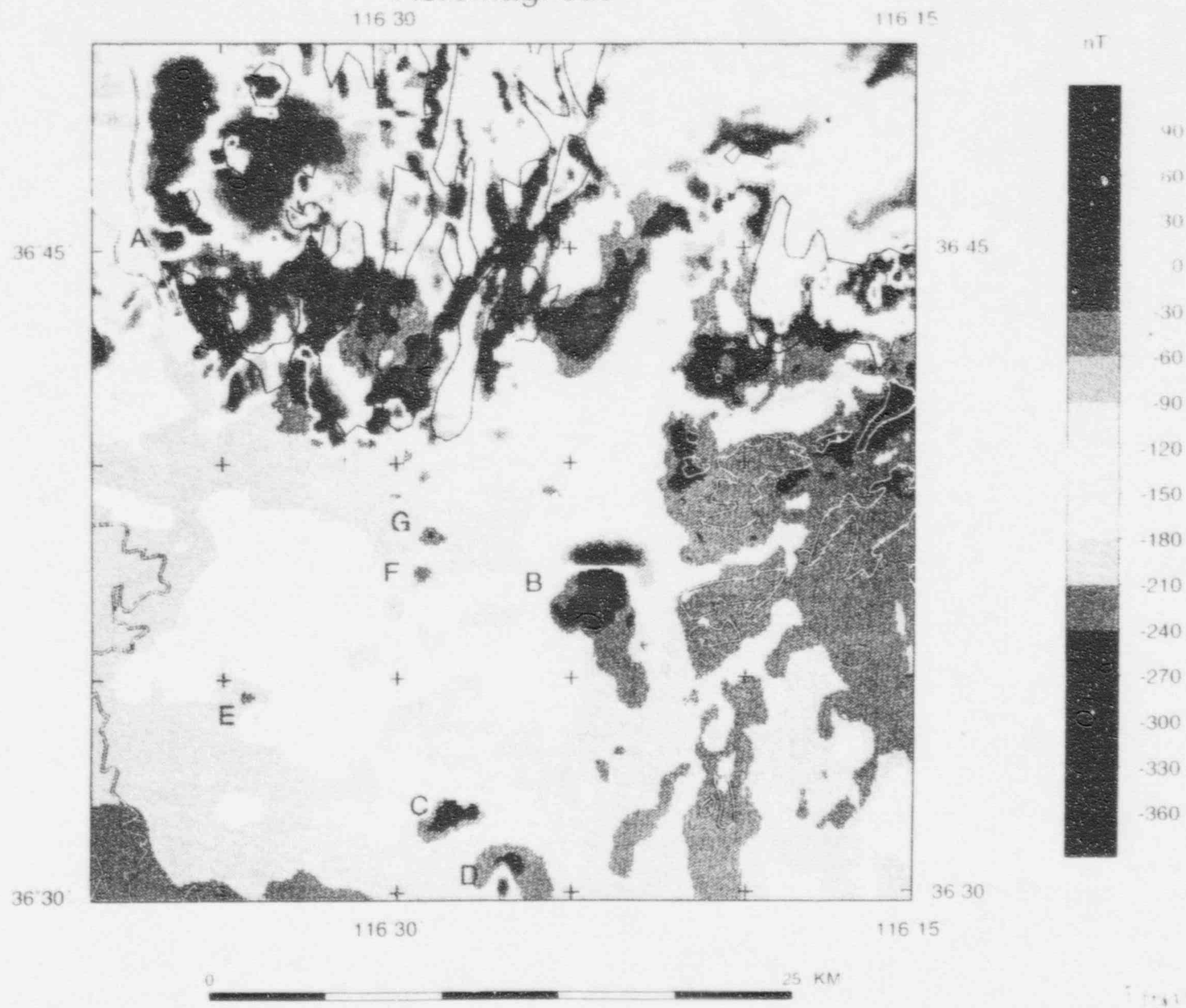
VOLUME OF LITTLE CONES

- Volume estimate of Little Cones was used increased to 0.03 km^3
- Total volume estimate of the northwest Crater Flat centers increased from 0.23 km^3 to 0.26 km^3
- Revised fit of cumulative volume data results in change in volume production rate from 0.316 km^3/yr to 0.320 km^3/yr, a 1% change
- Only impacts the assessment for Dr. Richard Carlson, who used volume-predictable approach for temporal distribution
- Calculated result is a <1 % change in mean annual frequency of intersection

AMARGOSA VALLEY ANOMALIES

- Ground magnetic data give higher credibility to anomaly A (near Little Cones) and anomaly "A" near F and G being buried volcanic cones
- 8 of 10 experts gave significant weight to time periods that include the estimated ages of the buried anomalies (~3.8 Ma)
- Anomaly A: addition of another event, or increasing weight on the higher event counts
- Anomaly "A": event-count increased by 1 to 3, depending on event definition by expert and number of events previously considered
- Mean event count in Amargosa Valley increased from 4.7 to 6.1 events
- Hazard recomputed using the revised event count
- Results in 4 percent increase in the mean annual frequency of intersection
- Revised assessment of mean annual frequency of intersection remains approximately 1.5×10^{-8}

Aeromagnetic



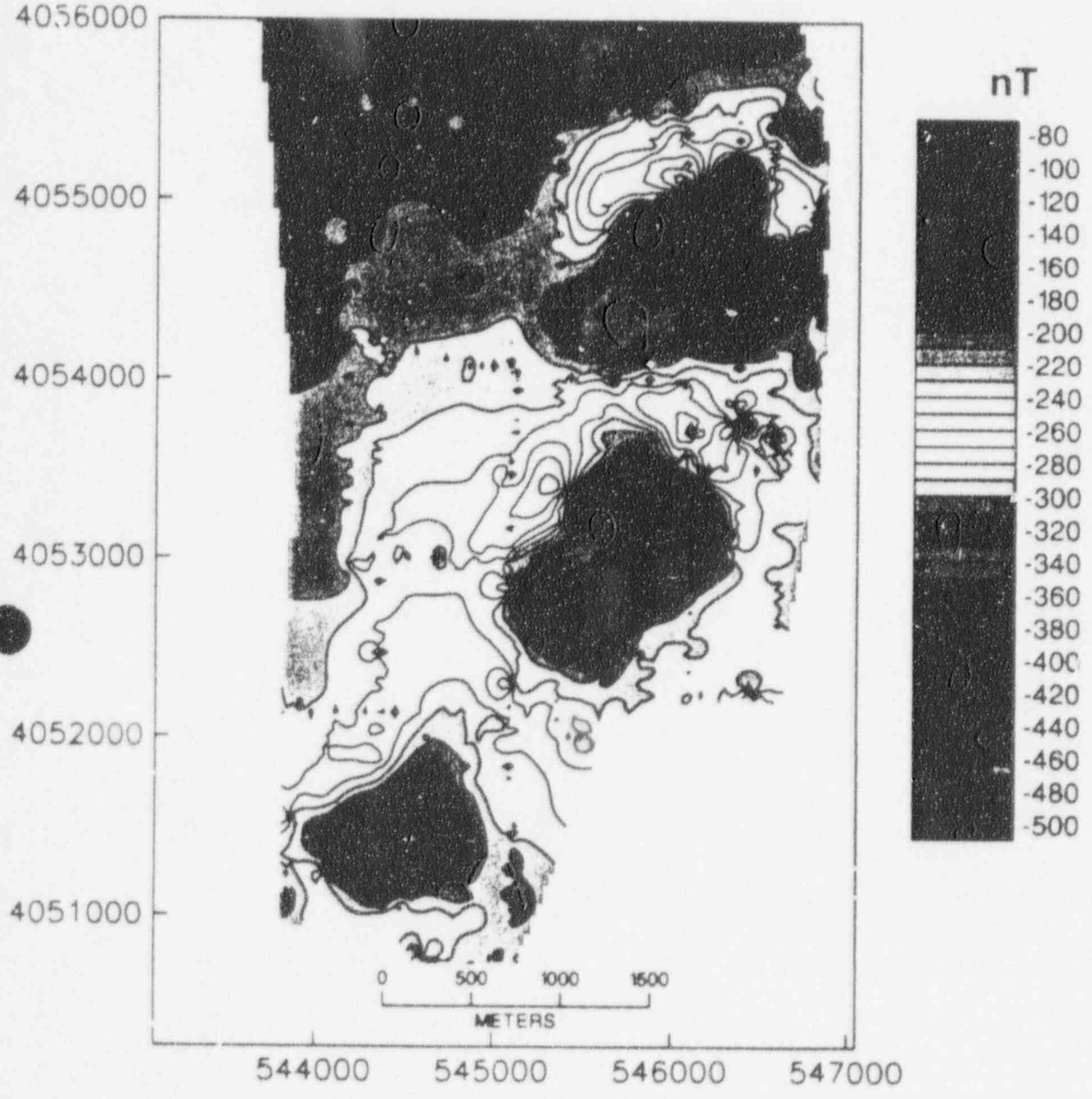
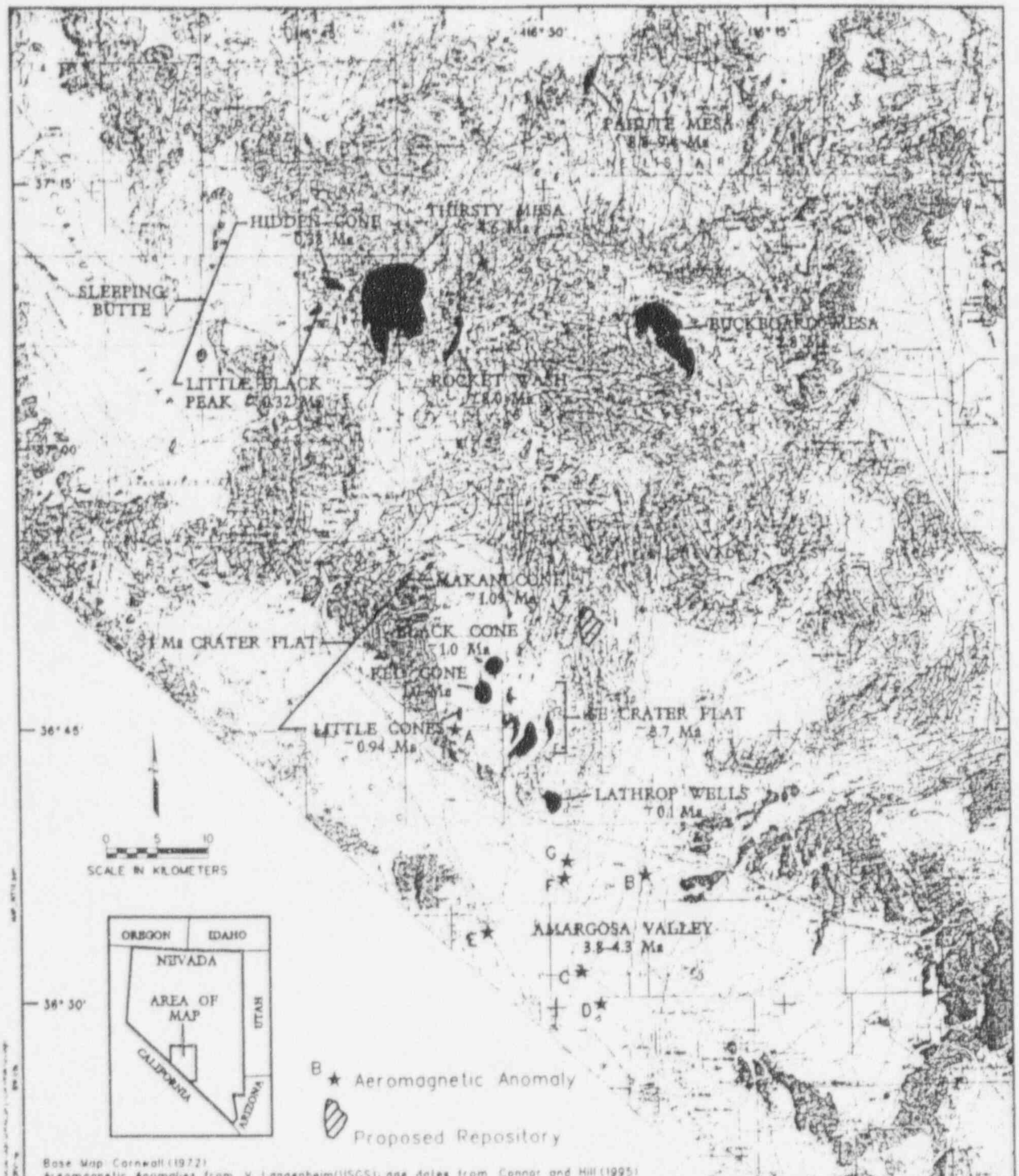


Fig. 2. Ground magnetic map of Amargosa Anomaly A showing three aligned anomalies. The high resolution magnetic survey elucidates structure in the magnetic anomalies, such as their reversed magnetization with steep inclinations and the elongate character of individual anomalies. Such detail in the magnetic map supports the interpretation that these anomalies are produced by a buried alignment of three volcanoes. Contour interval is 10 nT. These data and those shown on the following maps (Figures 3 and 4) are drift-corrected and the International Geomagnetic Reference Field (IGRF) is removed.



GEOMATRIX

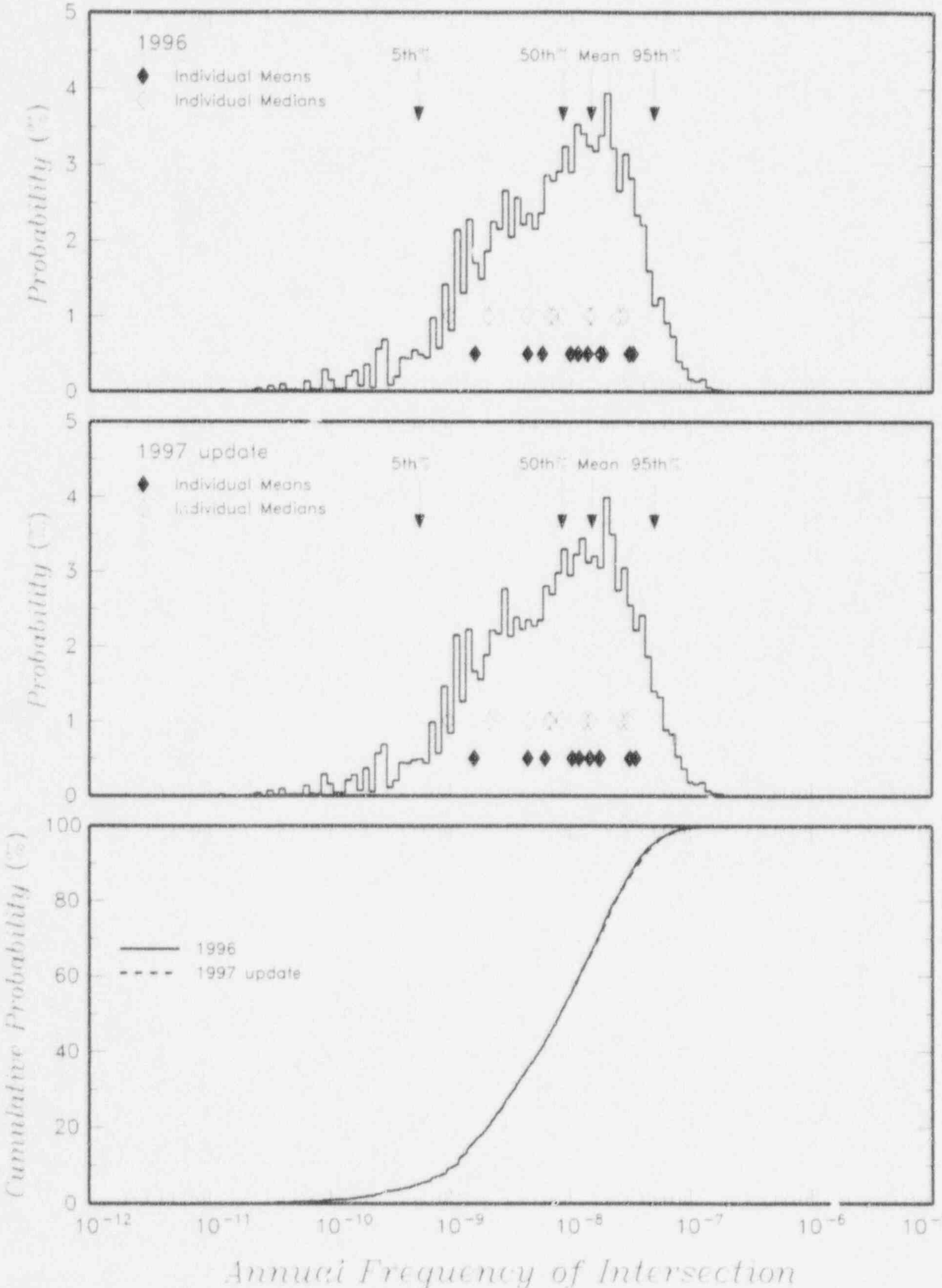
BASALTIC VOLCANIC CENTERS
AND AEROMAGNETIC ANOMALIES
IN THE VICINITY OF YUCCA MOUNTAIN, NEVADA

Figure 1-1

PVHA Project

Resulting increase in mean event count

Expert	Old Mean Counts	Updated Mean Counts
Alex McBirney	5.8	5.9
Bruce Crowe	6.3	7.6
George Thompson	5.2	6.6
George Walker	3.5	5.1
Mel Kuntz	3.3	5.3
Mike Sheridan	6.0	6.1
Richard Carlson	4.0	6.4
Richard Fisher	NA	NA
Wendell Duffield	NA	NA
William Hackett	3.3	5.7
Average over 8 experts	4.7	6.1



1996
5th%
50th%
Mean
95th%
Individual Means
Individual Medians
Probability (%)
1997 update
5th%
50th%
Mean
95th%
Individual Means
Individual Medians
Probability (%)
1996
1997 update
Cumulative Probability (%)
Annual Frequency of Intersection

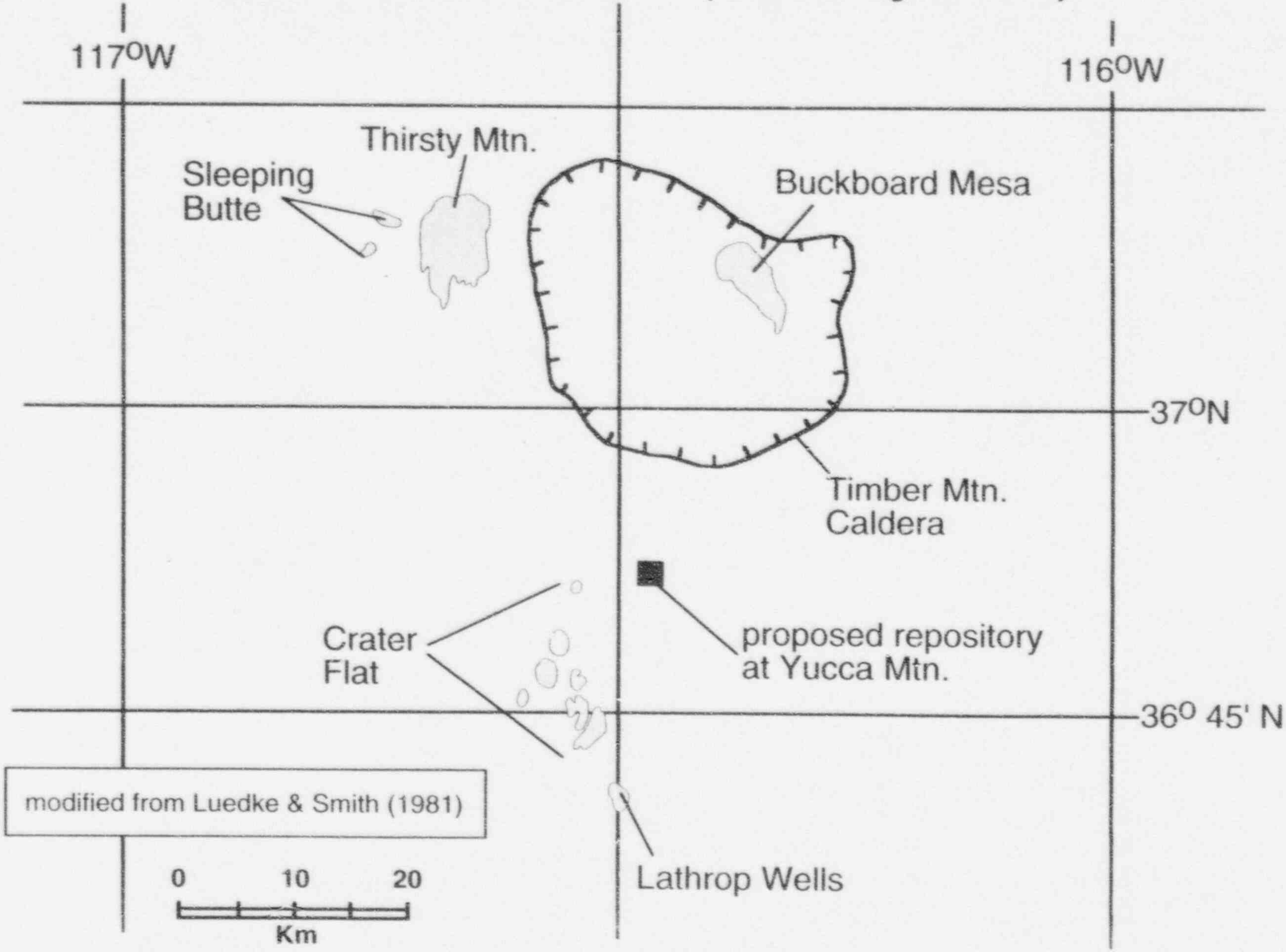
PVHA AT YUCCA MOUNTAIN STATE OF NEVADA

- THE AREA OF INTEREST FOR PVHA AT YUCCA MOUNTAIN
  - Gene Yogodzinski (UNLV, Dickinson College)
  - Eugene Smith (UNLV)
- EVOLUTION OF MAFIC VOLCANISM AT CITADEL MTN., CENTRAL NV.
  - Eugene Smith (UNLV)
  - Lori Dickson (UNLV)

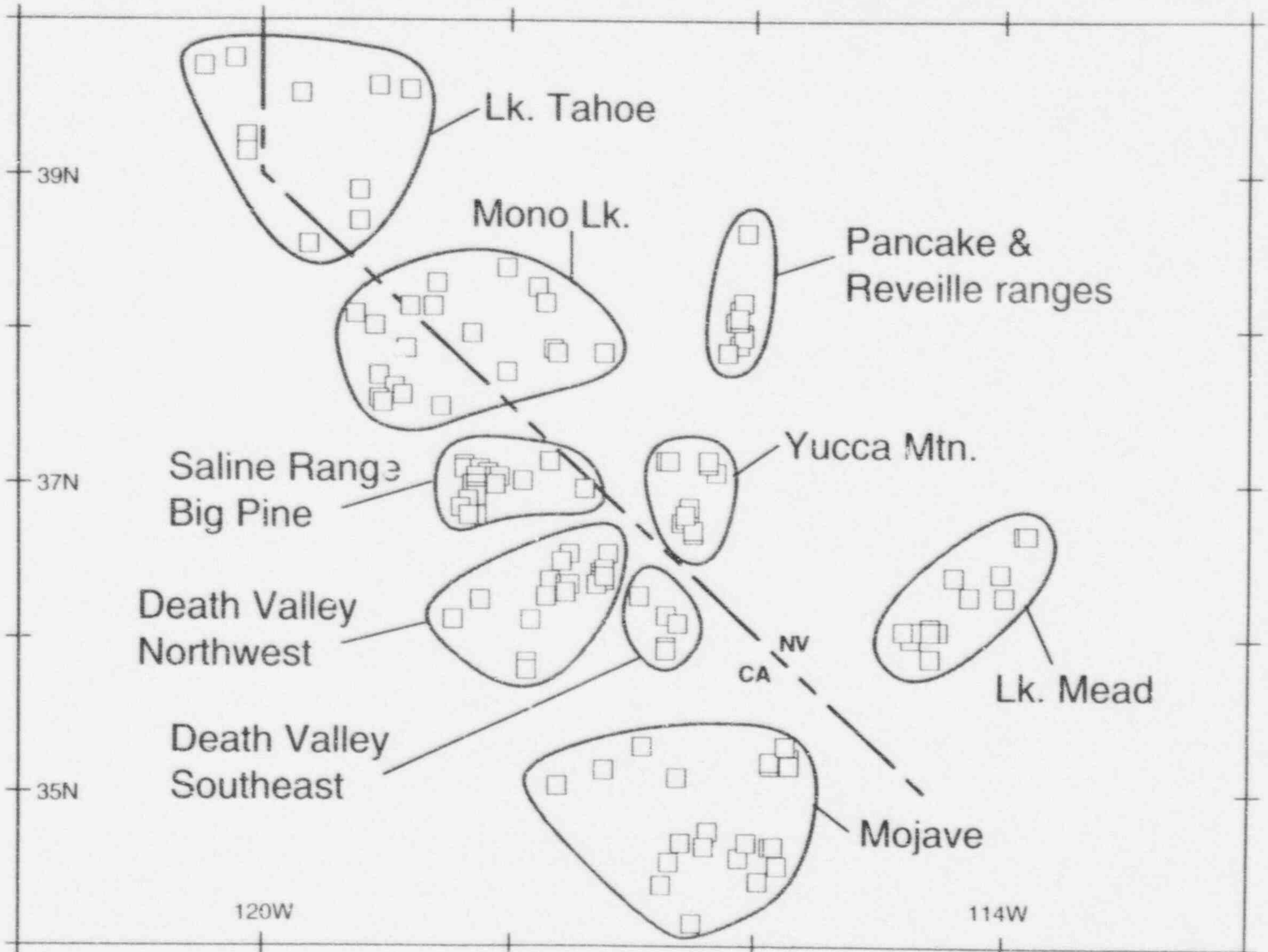
The Area of Interest for PVHA at Yucca Mtn.

- <u>OBJECTIVE:</u> Define the natural boundary that outlines the mafic magmatic system for the the Yucca Mtn. area.
- <u>ASSUMPTION</u>: The distribution of Pliocene-and-younger mafic volcanism is in some way tied to the distribution of melting anomalies in the mantle.
  - This assumption places importance on mantle source chemistry for basalts.
  - The Sr and Nd isotopic systems provide the best source of information.

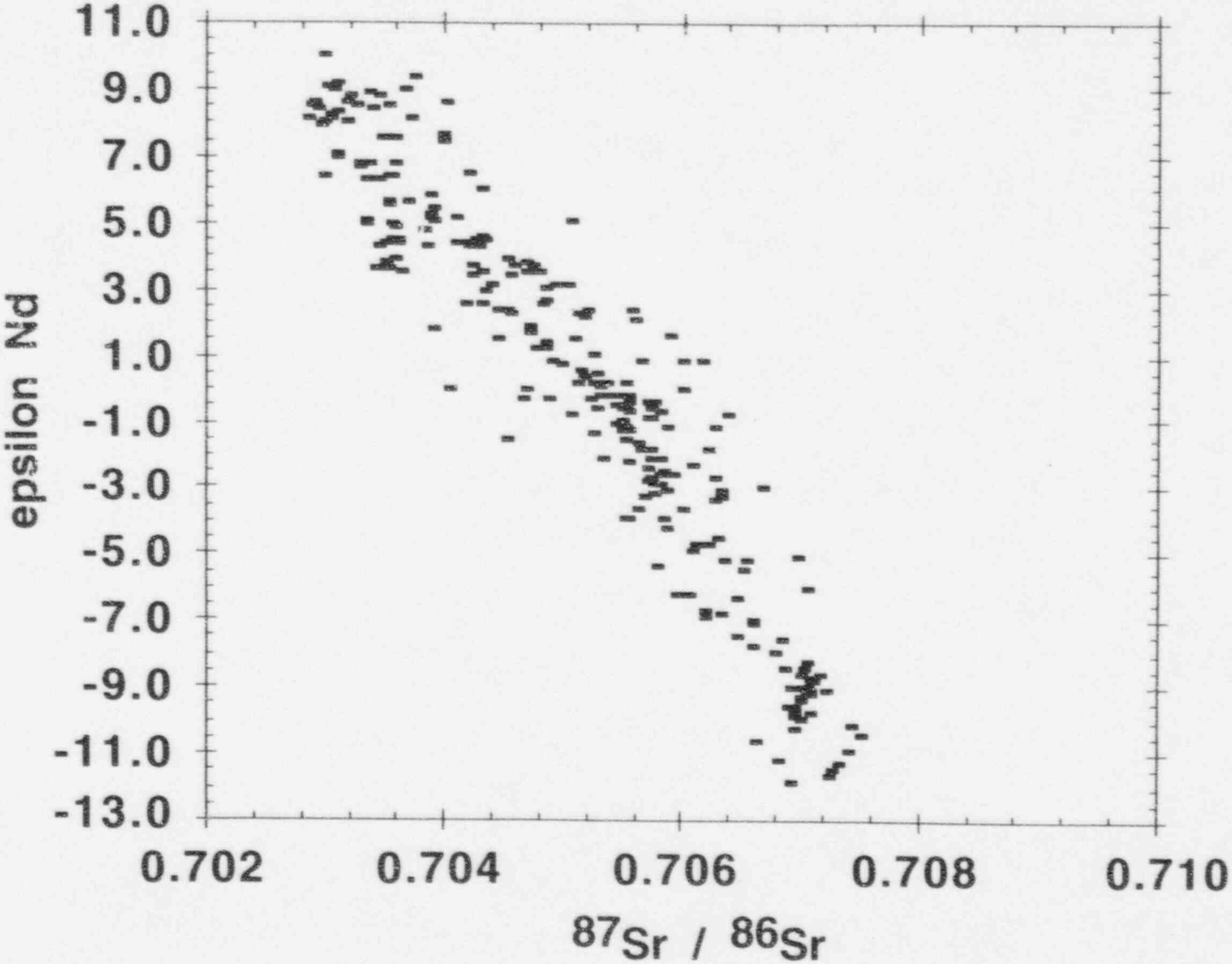
Distribution of Mafic Volcanism Yucca Mtn. Area (<6 m.y.-old)



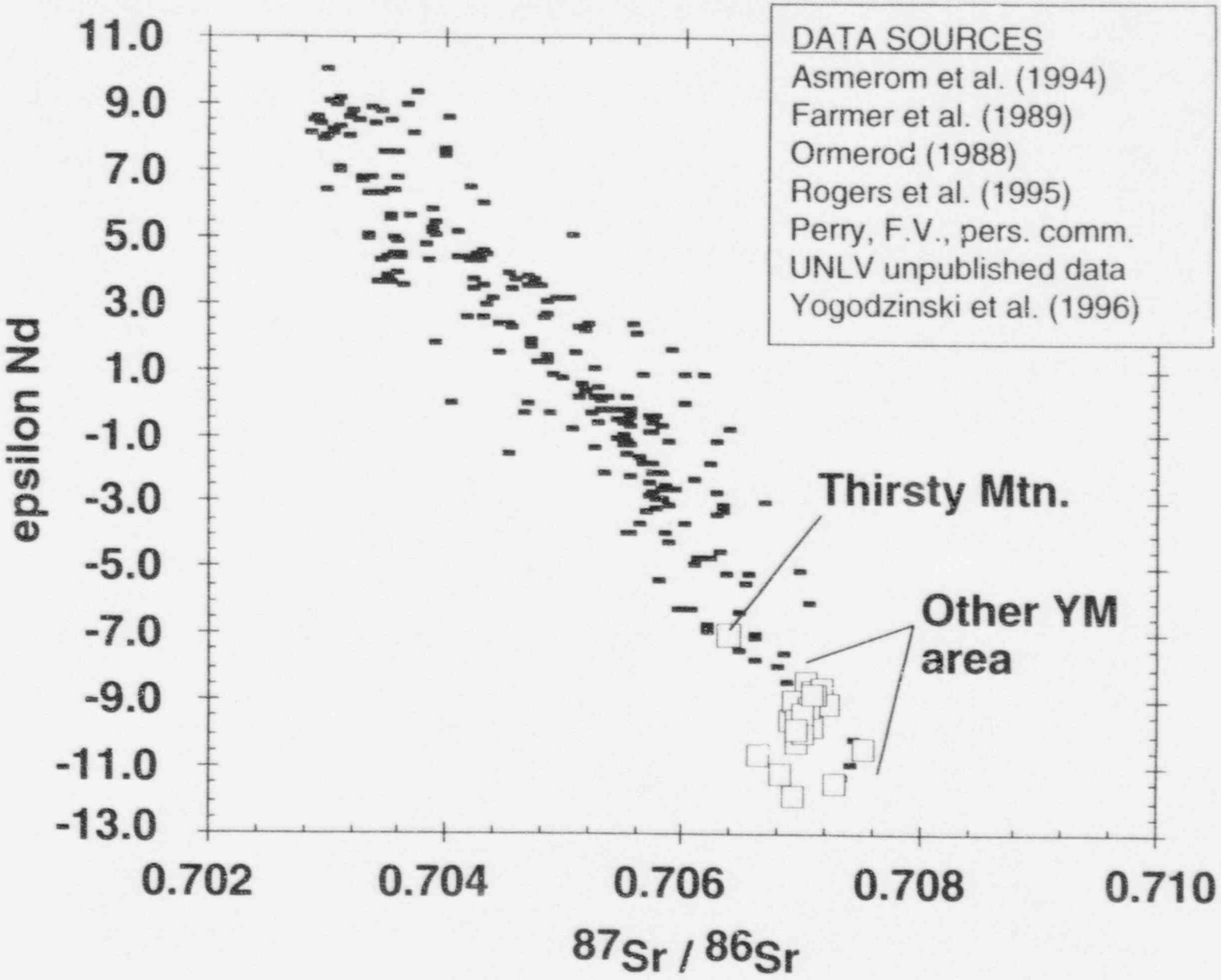
DISTRIBUTION OF BASALT SAMPLES: WESTERN GREAT BASIN



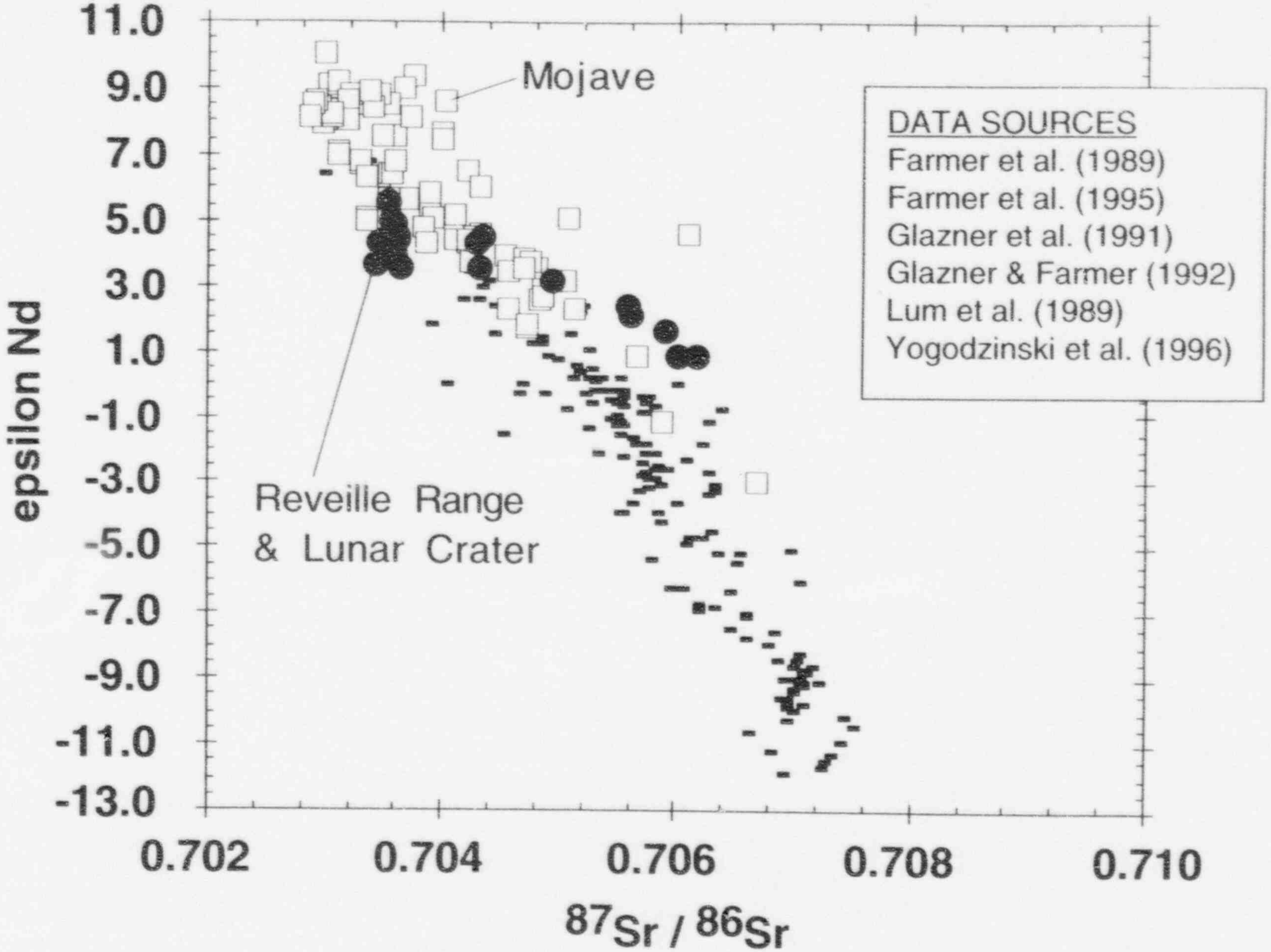
WESTERN GREAT BASIN BASALTS
(all data n=277 5 outliers excluded)



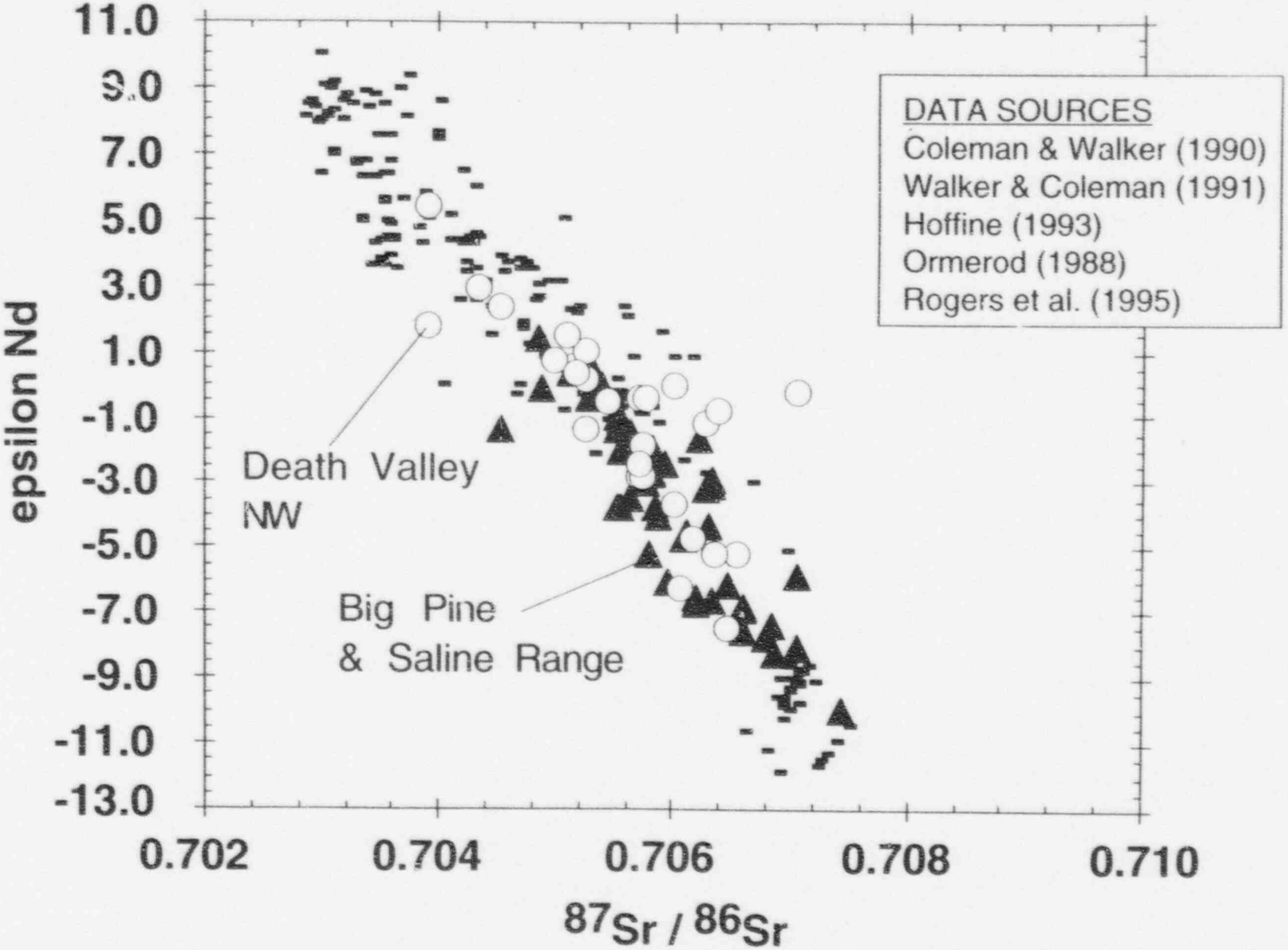
YUCCA MTN. AREA



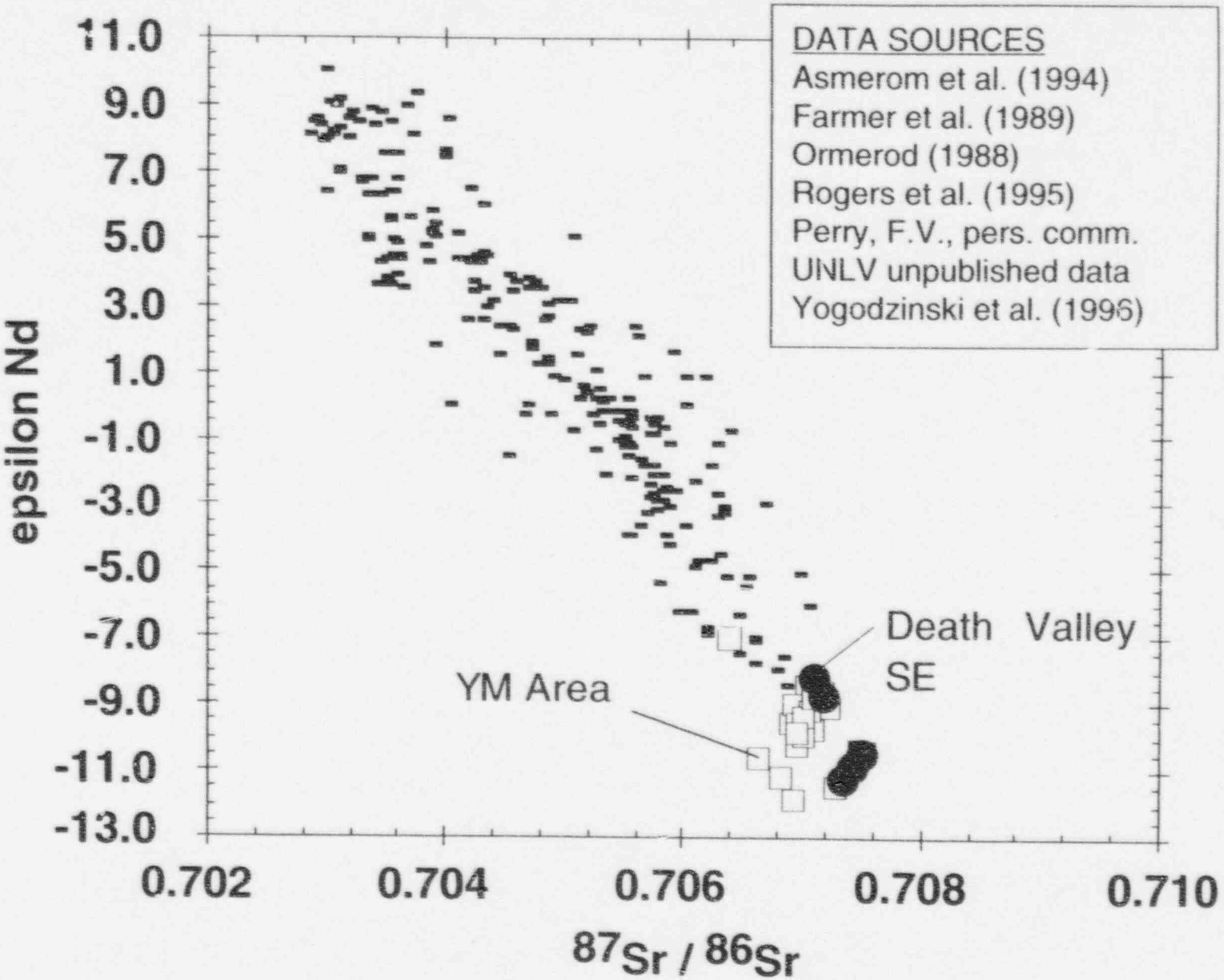
MOJAVE & RR-LUNAR CRATER



DEATH VALLEY NW - BIG PINE - SALINE



DEATH VALLEY SE



SUMMARY OF ISOTOPIC DATA

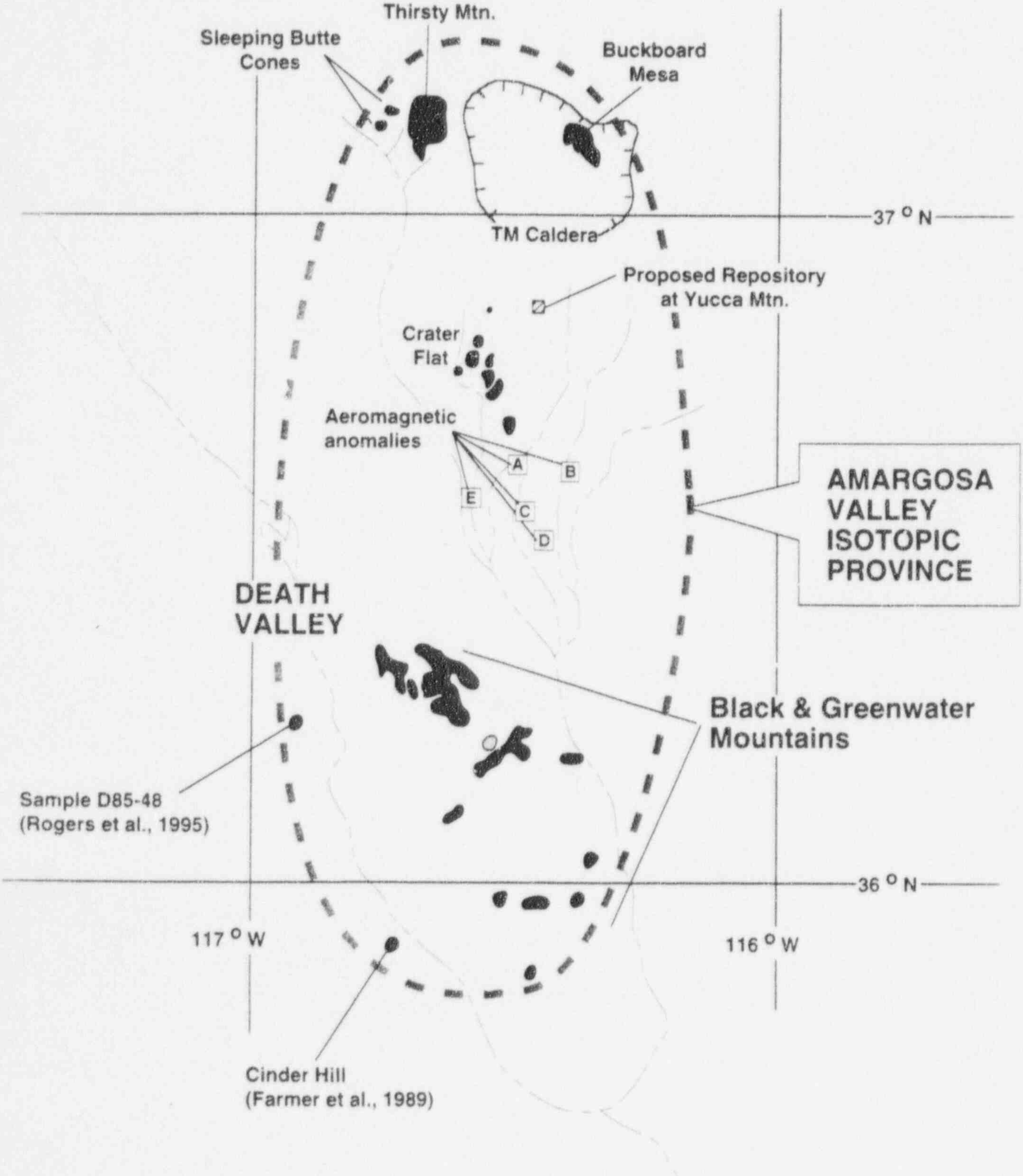
Basalts of the Yucca Mtn. area define a distinctive regional isotopic endmember.

Isotopically similar basalts are found in the Saline Range and possibly in NW Death Valley, but these fall within data arrays that are highly variable.

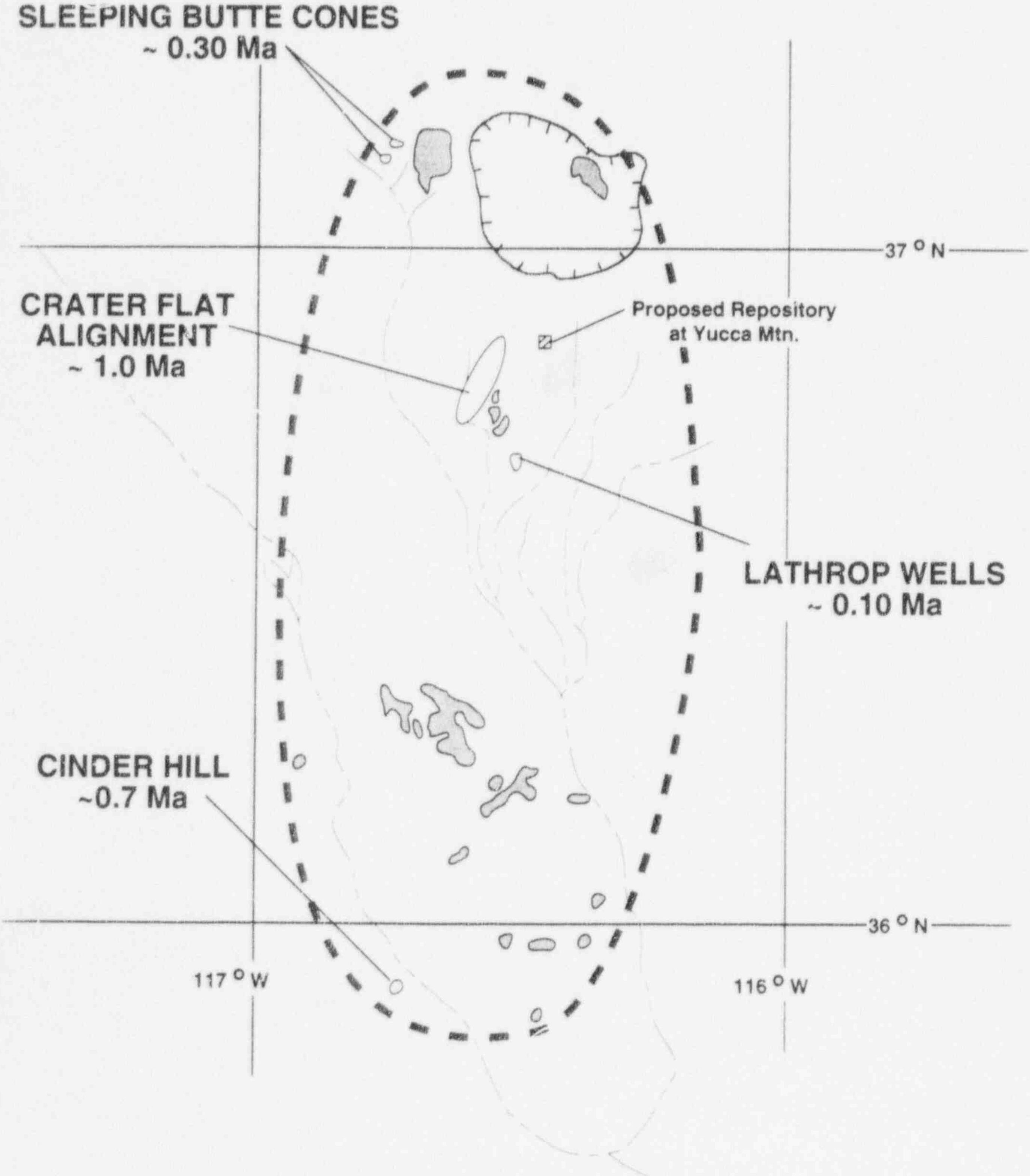
Pliocene-and-younger basalts in SE Death Valley are identicle to those in the Yucca Mtn. area.

Basalts of the Yucca Mtn. area and SE Death Valley form an isotopic province centered on the Amargosa Valley

THE AMARGOSA VALLEY ISOTOPIC PROVINCE AND THE MAGMATIC SYSTEM IN THE YUCCA MTN. AREA



MOST RECENT VOLCANIC ACTIVITY IN THE AMARGOSA VALLEY ISOTOPIC PROVINCE



AVIP CONCLUSIONS

The boundary around the Amargosa Valley Isotopic Province encompasses the magmatic system that has produced the Pliocene-and-younger basalts in the Yucca Mtn. area.

This natural boundary and the magmatic system it encompasses should be considered in probabilistic volcanic hazard assessment modeling at Yucca Mtn.

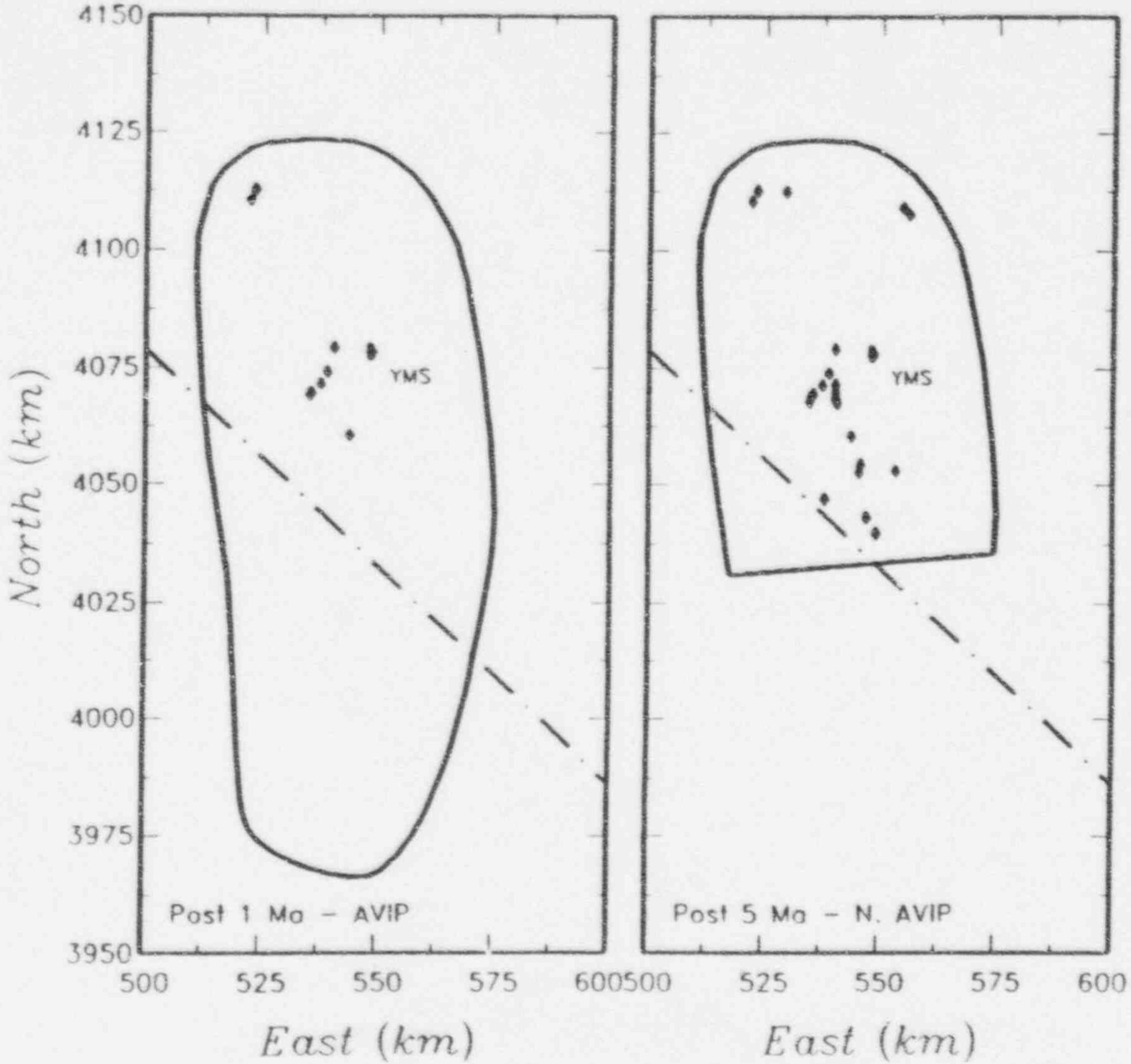


Figure 3-46 Regions of interest specified by Richard Carlson. Diamonds represent volcanic events for the post-1 Ma and the post-5 Ma time periods. YMS refers to the proposed Yucca Mountain repository site and the dash-dot line is the Nevada-California border.

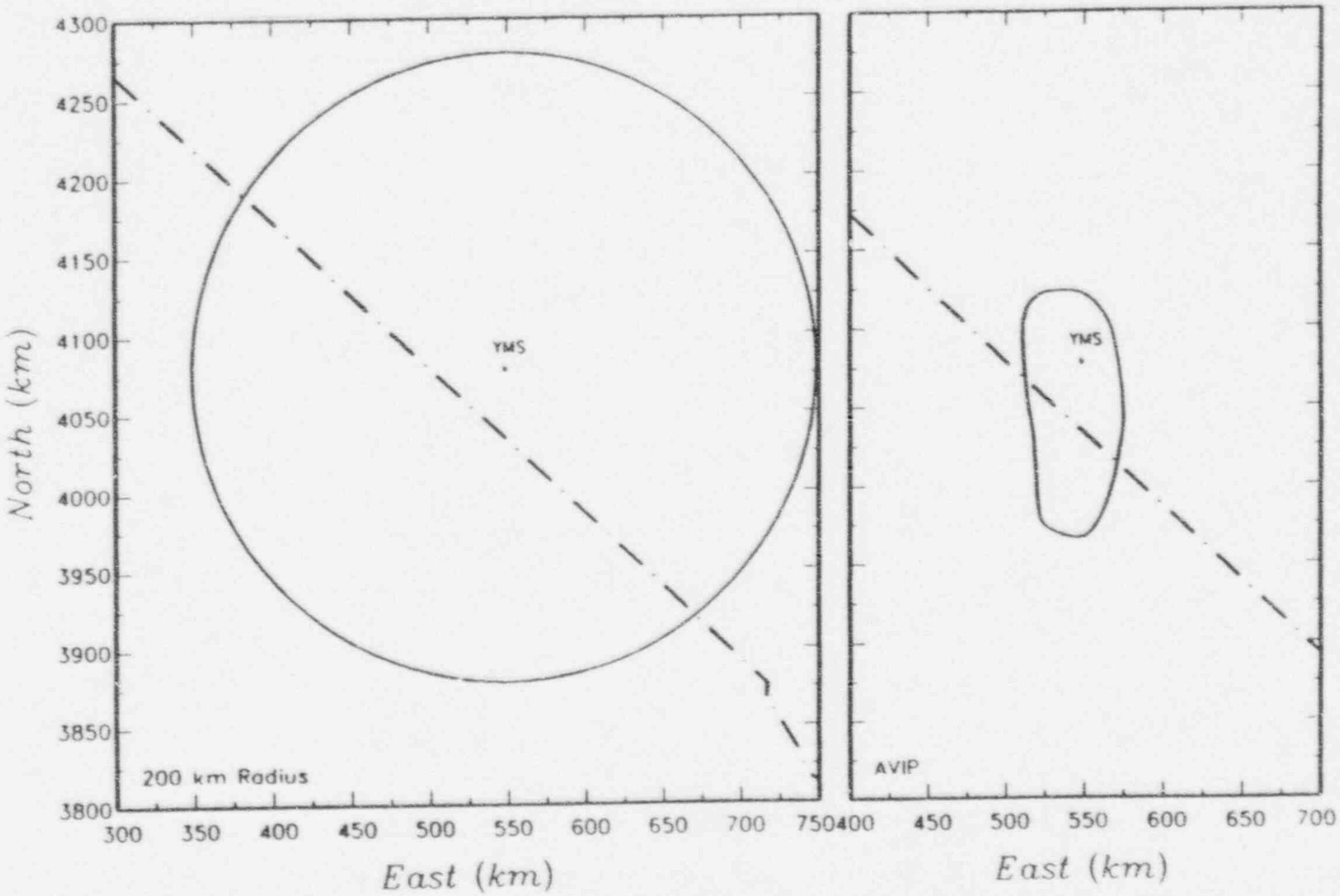


Figure 3-28 Alternative regions of interest used as background source zones in George Thompson's PVHA model. YMS refers to the proposed Yucca Mountain repository site and the dash-dot line is the Nevada-California bou[illegible]

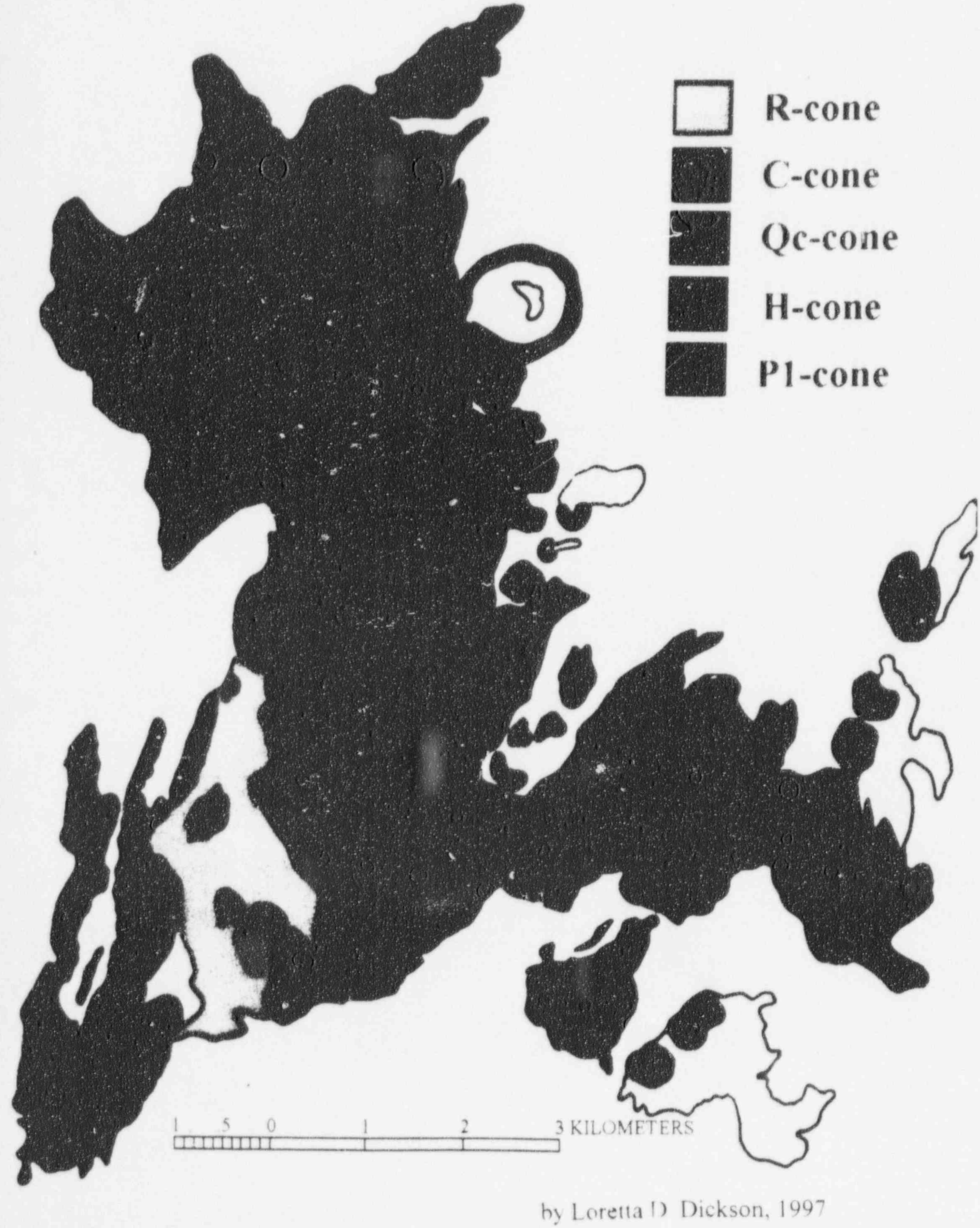
5A

CITADEL MOUNTAIN AS AN ANALOGUE FOR YUCCA MTN.

- Tilted fault block cored by Oligocene ash-flow tuffs
-
- Uplift of fault block mostly on the NW & SE w/dip slope to the NE
-
- Cinder cones erupted along the range crest as well as in the adjacent basins

5A

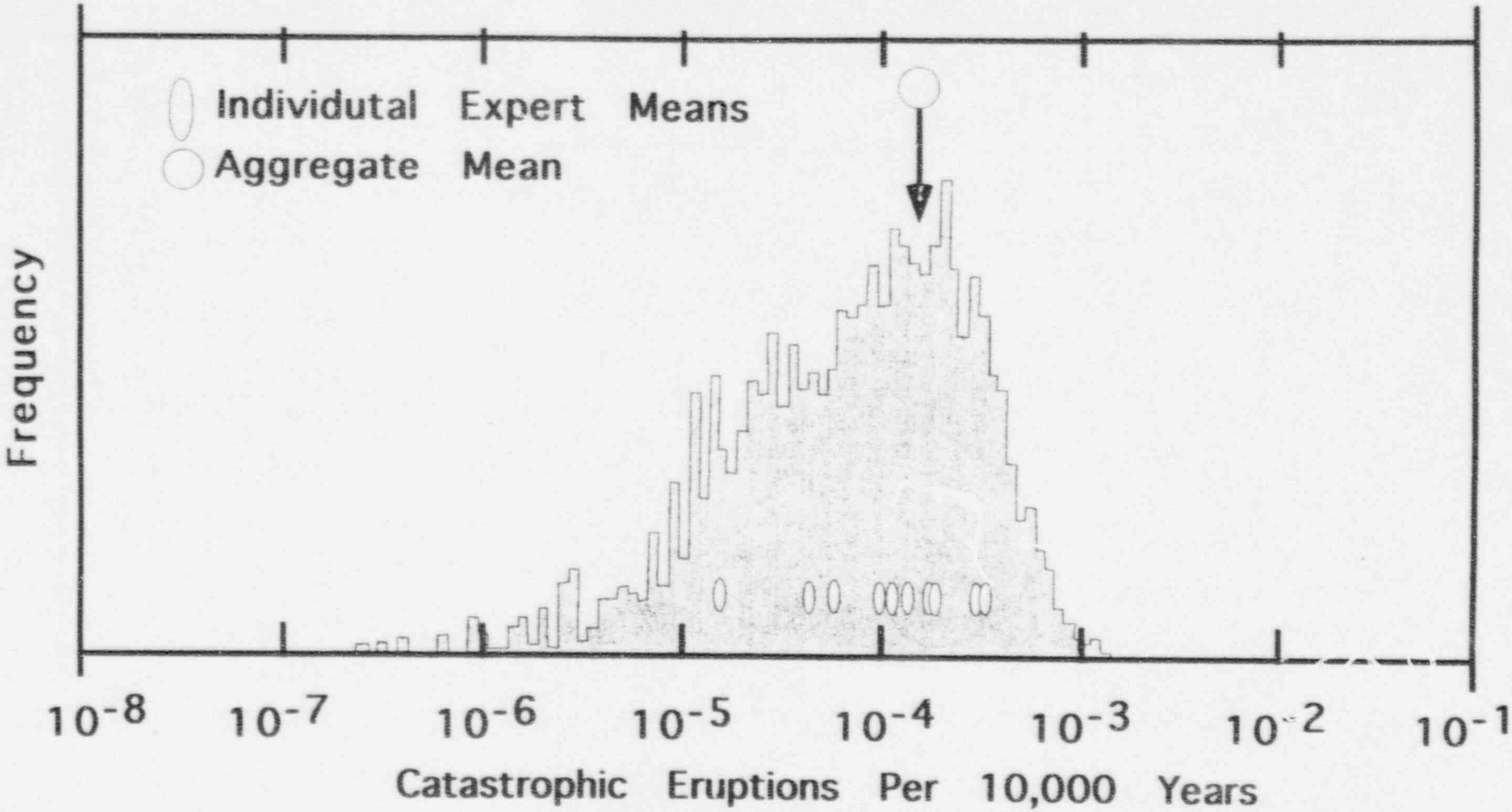

Lunar Crater
South Kidney Butte
Buckskin Pt
Rim
MOUNTAIN
CITADEL
PANCAKE
Qbc
Qbd
Qac
Qb
Tbw
Tbp
Tgw
Tar
Tsp
Tbb



by Loretta D Dickson, 1997

CITADEL MTN. CONCLUSIONS

- **Analogue study at Citadel Mtn. suggests that ideas about magma barriers west of Yucca Mtn. (ideas like those that arose in the course of the Geomatrix PVHA process) should be carefully scrutinized.**



from Science, v274 p913 '96

6

CNWRA INVESTIGATIONS OF IGNEOUS ACTIVITY CONSEQUENCES ON REPOSITORY PERFORMANCE

FIN D1035
NRC DWM Program Element Manager: J.S. Trapp
CNWRA Element Manager: H.L. McKague

Presented at the April 22,1997
91st ACNW Meeting

by

Dr. Brittain Hill
Research Scientist, CNWRA
(210) 522-6087, bhill@swri.edu

CNWRA Contributors
C. Connor, M. Conway, S. Greenan, B. Henderson, B. Hill,
M. Jarzemba, P. LaFemina, L. McKague, R. Martin

CONCEPTUAL MODEL FOR WASTE INCORPORATION INTO ERUPTION

- MAGMA: 1100 °C, density 2600 kg/m^3, viscosity 10–100+ Pa s (sucrose 1000 Pa s @ 100°C), velocity 1 m/s initial to 100 m/s eruption, acidic gas.

- WASTE PACKAGE BEHAVIOR UNDER MAGMATIC CONDITIONS POORLY KNOWN
  - Waste package fails between 10^{-2} s to 1 year from simple thermal effects of magma. This is the duration range for most basaltic eruptions.

- WASTE BEHAVIOR UNDER ERUPTION CONDITIONS POORLY KNOWN
  - Waste particles incorporated into ascending magma. Volatile expansion, temperature, and shear may further reduce particle grain-size.

- DISPERSAL CAPABILITIES OF YMR VOLCANOES POORLY KNOWN
  - Waste ejected ballistically from volcano vent or convectively in the eruption column and transported downwind in eruption plume.

Data, Models, and Assumptions Are Necessary to Evaluate Dose Associated with Volcanic Eruption Through Repository

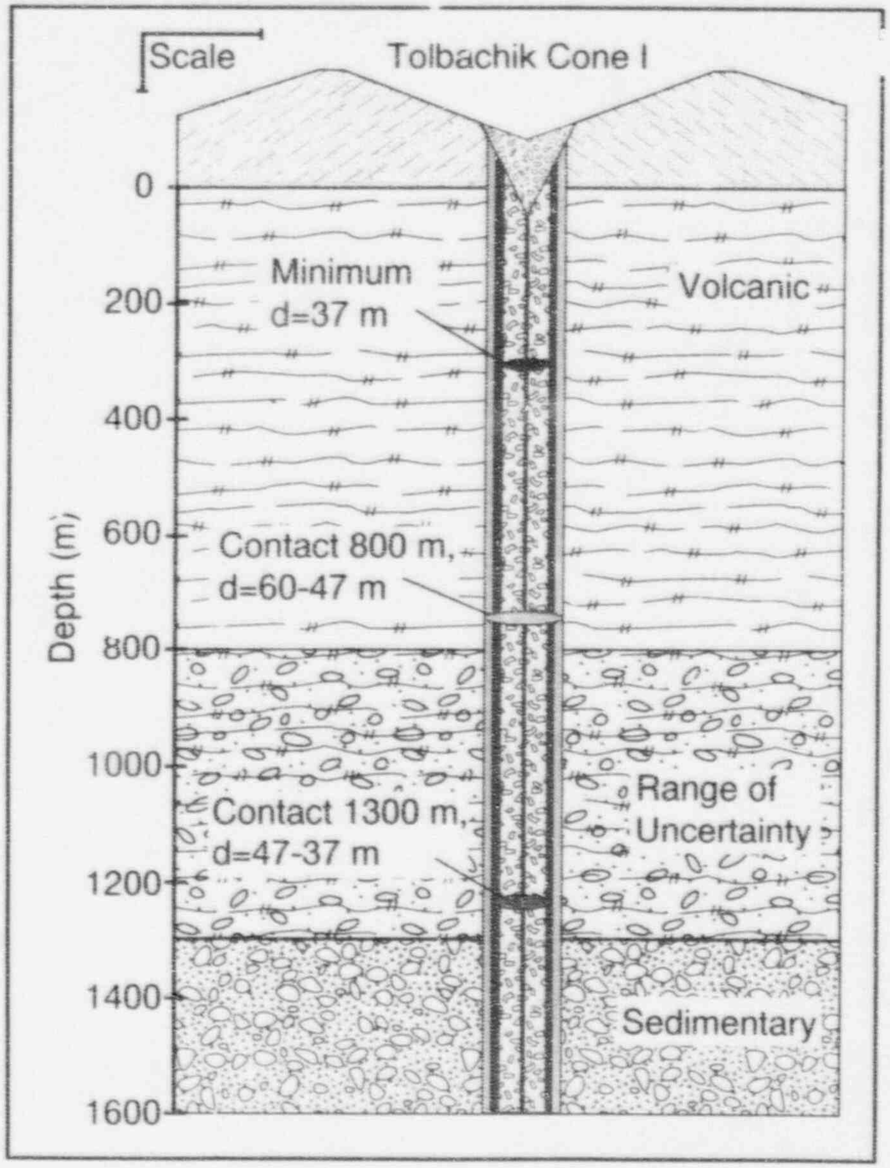
ANALOG VOLCANISM YIELDS YMR INSIGHTS

	Avg LW	Avg CF	C1 Early	C1 Late
# of analyses	20	32	21	11
Wt. % oxides				
SiO_2	48.80	50.20	49.80	50.00
TiO_2	1.95	1.59	1.02	1.30
Al_2O_3	16.81	17.30	13.48	15.32
Fe_2O_3	2.33	2.15	3.06	3.47
FeO	8.37	7.72	6.99	6.88
MnO	0.17	0.17	0.16	0.17
MgO	5.86	5.09	9.88	7.69
CaO	8.57	8.71	11.60	9.83
Na_2O	3.62	3.41	2.44	3.14
K_2O	1.80	1.76	1.03	1.62
P_2O_5	1.22	1.10	0.25	0.35
Estimated H_2O	2	2	2	2
Estimated T (°C)	1100	1100	1200	1200
Density (kg m^{-3})	2590	2570	2600	2640
Viscosity (Pa s)	17	30	4	6
Crystal Fraction	0.03	0.03	0.03	0.03
Viscosity with crystals (Pa s)	20	34	5	7

YMR data from Hill and Luhr (unpub. res), 1975 Tolbachik from Volynets et al. (1983).

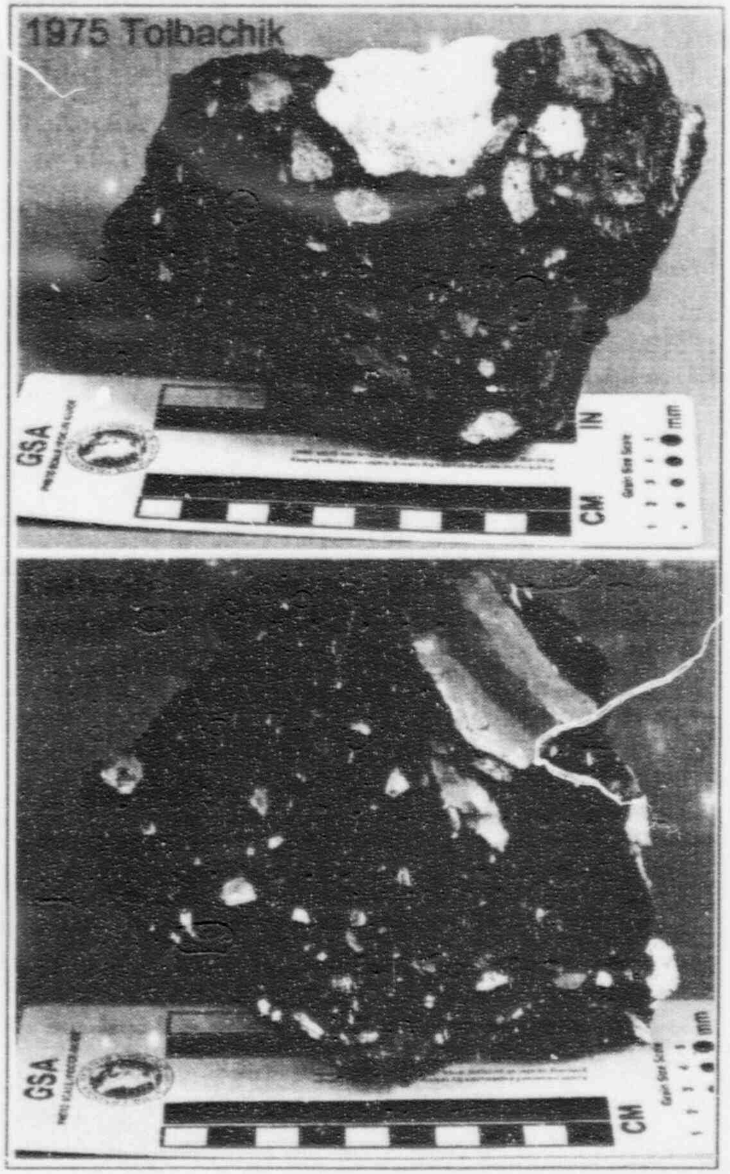
- 1975 Tolbachik eruption in Kamchatka, Russia, analogous for processes relevant to understanding Quaternary YMR volcanoes.
- Extensional tectonic settings with magma equilibration depths around 40 km, remarkably similar basalts derived from hydrous mantle lithosphere
- - Cone 1 = 0.14 km^3 DRE
  - Lathrop Wells = 0.14 km^3 DRE
  - Quat YMR = 0.06-0.23 km^3 DRE
  - 1975 Tolbachik = 0.45 km^3 DRE
- Insights into poorly preserved YMR eruption deposits and processes

SUBSURFACE AREA OF DISRUPTION



- 12 hr at end of 1975 Tolbachik Cone 1 eruption, 2.8×10^6 m^3 of shallow subsurface rock disrupted.
- Main contact between volcanic and sedimentary rocks 800-1300 m, water table at 500 m
- Volume of subsurface rock equals widening conduit from initial 1-3 m diameter to 49±7 m diameter
- Late-stage reduction in magma flow rate allowed dilation of fractures below 500-m-deep water table, aiding brecciation
- Sparse rock-ash on cone, but abundant (1-2%) xenoliths on cone flanks

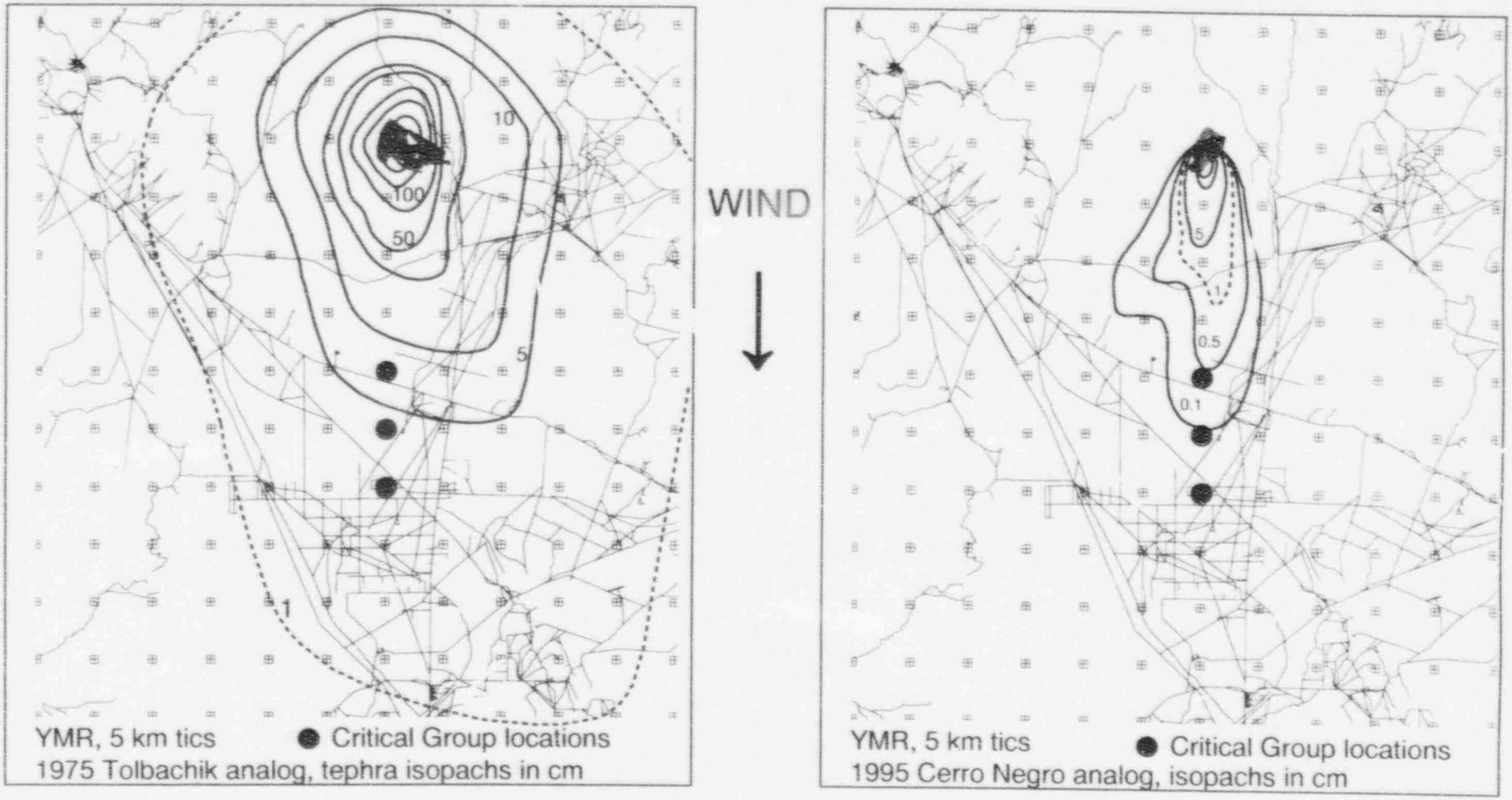
SUBSURFACE AREA OF DISRUPTION



- Upper part of tephra fall deposit at Lathrop Wells and all other YMR volcanoes eroded
- Proximal xenoliths unusually abundant on outer flanks of Lathrop and Little Black Peak
- Same type of unusual polylithologic, polythermal volcanic pyroclasts at Lathrop and Tolbachik produced during a late-stage disruptive event (Figure)
- Both volcanoes disrupted similar range of crustal sections, up to 0.5–2 km deep
- Scale of disruption for Lathrop Wells most likely comparable to 1975 Tolbachik. May disrupt 4–10 waste packages if similar event occurs in 83 MTU/acre repository.

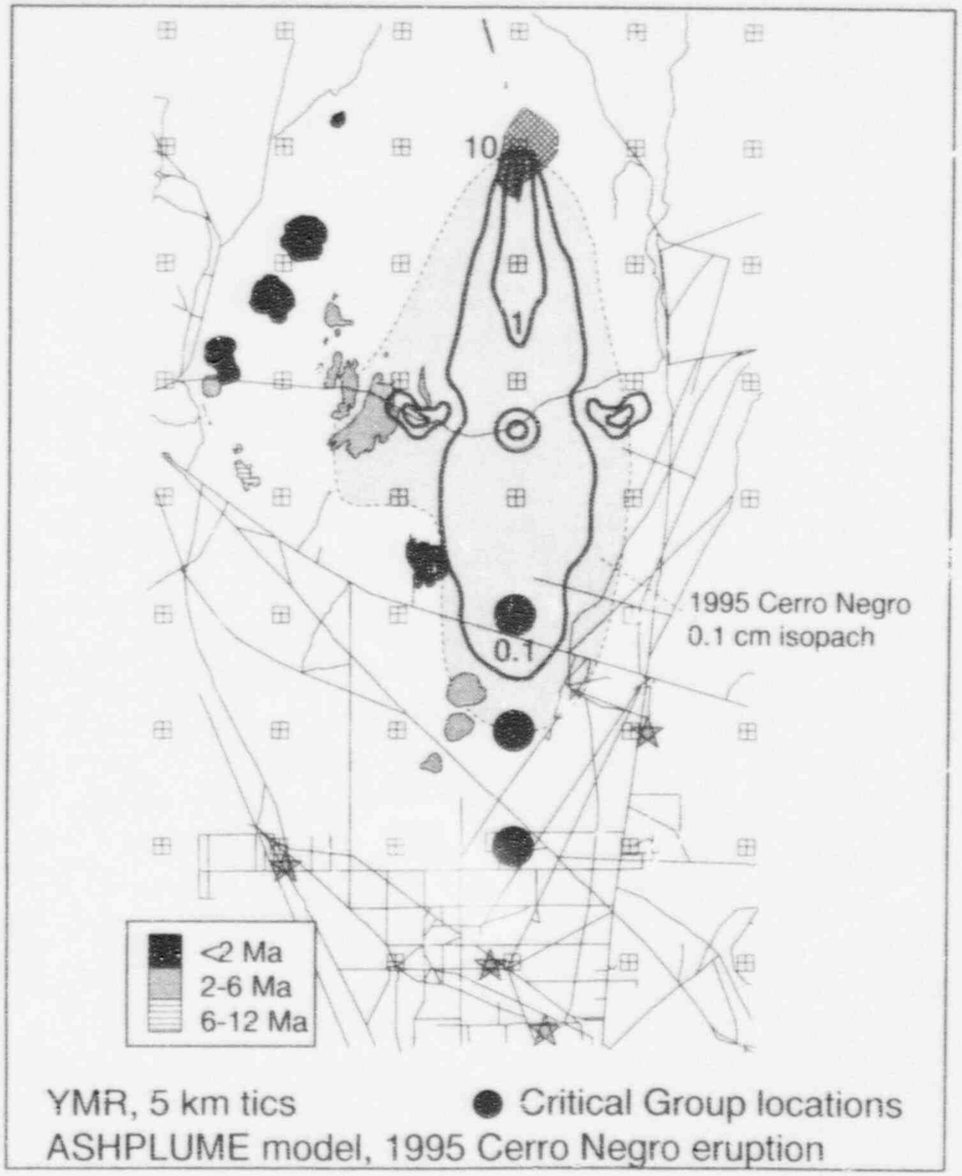
TRANSPORT OF MATERIAL FROM BASALTIC VOLCANOES

Size-range of analog eruptions superimposed on YMR site, wind to south.



1 cm isopach shown in dashed line. Tolbachik (0.45 km^3), Cerro Negro (0.008 km^3)

MODELING TEPHRA DISPERSION WITH ASHPLUME



- Dispersion model of Suzuki (1983)
- Accounts for vertical and lateral dispersion of particles
- Modeling 1995 Cerro Negro, calculated deposit thickness very sensitive to Wind Speed
- Moderately sensitive to column height, particle diameter & density
- Model reproduces measured deposit thicknesses within 50% at 20–30 km
- ASHPLUME used to calculate dose from volcanic eruptions

CNWRA DOSE CALCULATIONS FOR VOLCANIC DISRUPTION OF THE PROPOSED REPOSITORY

FIN D1035

NRC DWM Program Element Manager: J.S. Trapp

CNWRA Element Manager: H.L. McKague

Presented at the April 22,1997

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CNWRA Contributors

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CURRENT DOSE CALCULATIONS FOR VOLCANIC ERUPTIONS

BASIC ASSUMPTIONS

- Volcanic eruption occurs through repository between +200 and +10,000 yr
- One canister fails and all waste (10 MTU) available for magmatic transport
- Wind blows to south 14% of the time
- Critical group located 20–30 km south of repository
- Current TPA approach (DCF, GENII-S, etc.) used for dose calculations

CRITICAL PROCESSES

- Behavior of ascending magma in disturbed geologic setting
  - *Under investigation, assumed non-disturbed behavior*
- Canister response to ascending magma
  - *Failure assumed based on initial analysis*
- Waste particle-size distribution
  - *Mean values 10, 1, and 0.01 mm, log-triangular range ±1 log unit*
- Waste incorporation mechanisms into magma
  - *Tephra diameter must be 10x, 5x or 2x waste diameter to incorporate*
- Dispersal capabilities of YMR volcanoes
  - *As observed at historically active analogs, using ASHPLUME*

EVALUATE SENSITIVITY TO WASTE PARTICLE-SIZE AND INCORPORATION FACTORS

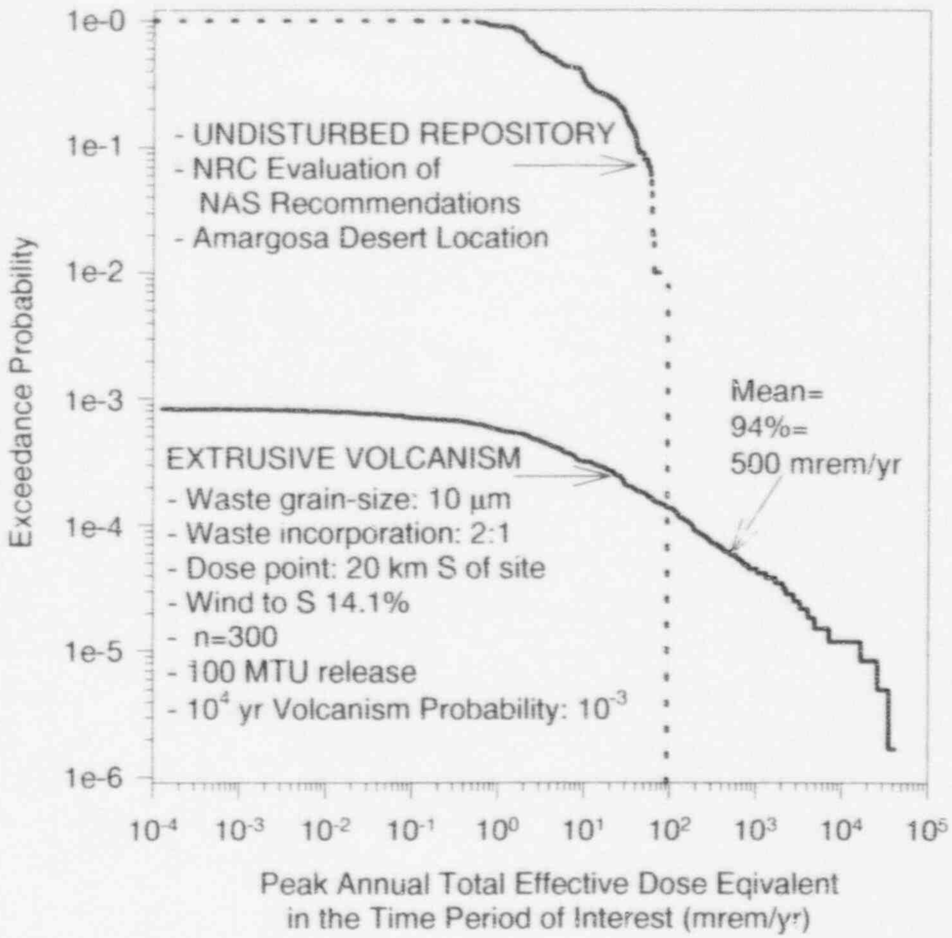
RESULTS OF 300 SIMULATIONS EACH, 10 MTU WASTE AVAILABLE FOR TRANSPORT
MEAN ANNUAL PEAK DOSE, 1 STANDARD DEVIATION (MREM/YEAR)
LOG-NORMAL DISTRIBUTIONS

Parameters sampled stochastically: Eruption power & duration (column height, eruption rate, mass), column shape, time, wind speed, tephra diameter

PARAMETER SENSITIVITY EXAMINED	CRITICAL GROUP LOCATION 20 KM	30 KM
• 0.01 mm diameter, 2x incorporation	50, 300	7, 40
• 0.01 mm diameter, 10x incorporation	40, 300	4, 40
• 1 mm diameter, 2x incorporation	9, 90	0.2, 3
• 1 mm diameter, 10x incorporation	0.3, 3	10^{-4}, 10^{-3}
• 10 mm diameter, 2x incorporation	10^{-3}, 10^{-2}	10^{-8}, 10^{-7}
• 10 mm diameter, 10x incorporation	10^{-18}, 10^{-17}	$\ll 10^{-36}$

CURRENT DOSE ASSESSMENT FOR VOLCANIC ERUPTIONS

Comparison of Current Undisturbed Repository Performance with Extrusive Volcanism



- CCDF shows mean values are reasonably conservative for these calculations
- Current calculations show this mean is $\leq$50 mrem/yr
- Vent widening may disrupt 10 canisters, thus 50 × 10 canisters = 500 mrem/yr
- Current CNWRA probability models range to 10^{-3} for 10^{4} years
- Risk = [Volcanism, 10^{-3}–10^{-4}, 500 mrem/yr]
- Still to consider: Timing of eruptions, waste incorporation mechanisms, system response to thermal, chemical and mechanical loads from igneous activity, dose point locations.

ANNUAL INDIVIDUAL DOSE ESTIMATES FROM "PRELIMINARY" PERFORMANCE CALCULATIONS

Tim McCartin
US Nuclear Regulatory Commission
Office of Nuclear Material Safety and Safeguards

Presented at:

ACNW Meeting on Igneous Activity
April 22, 1997

CONTEXT OF ANNUAL INDIVIDUAL DOSE ESTIMATES FROM PRELIMINARY PERFORMANCE CALCULATIONS

TSPA '95

Preliminary inclusion of a peak dose calculation in response to NAS Recommendation

NRC Staff Evaluation of NAS Recommendations

Identification of implementation issues with long term dose estimates and exposure scenario (reference biosphere and critical group)

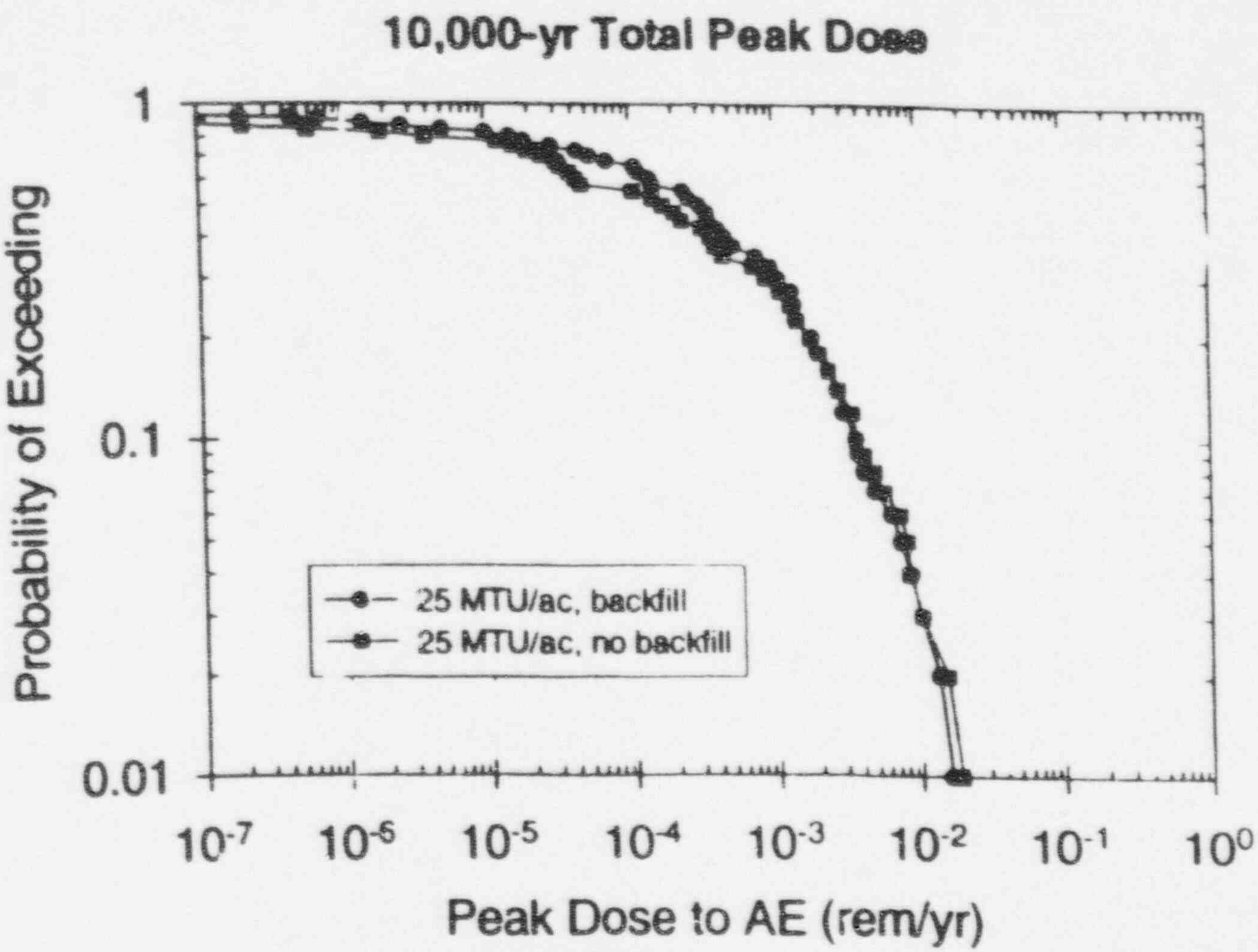
TSPA '95

BASIC ASSUMPTIONS

- Undisturbed performance only
  - volcanism not considered quantitatively
  - TSPA 91 considered direct release to surface from volcanic event
  - TSPA 93 considered indirect effects of volcanic event

- Drinking water dose at 5 km

DOE TSPA '95



CCDF of peak annual individual dose at 5 km down gradient (with and without backfill, high infiltration, 25 MTU/acre)

NRC Staff Evaluation of NAS Recommendations

BASIC ASSUMPTIONS

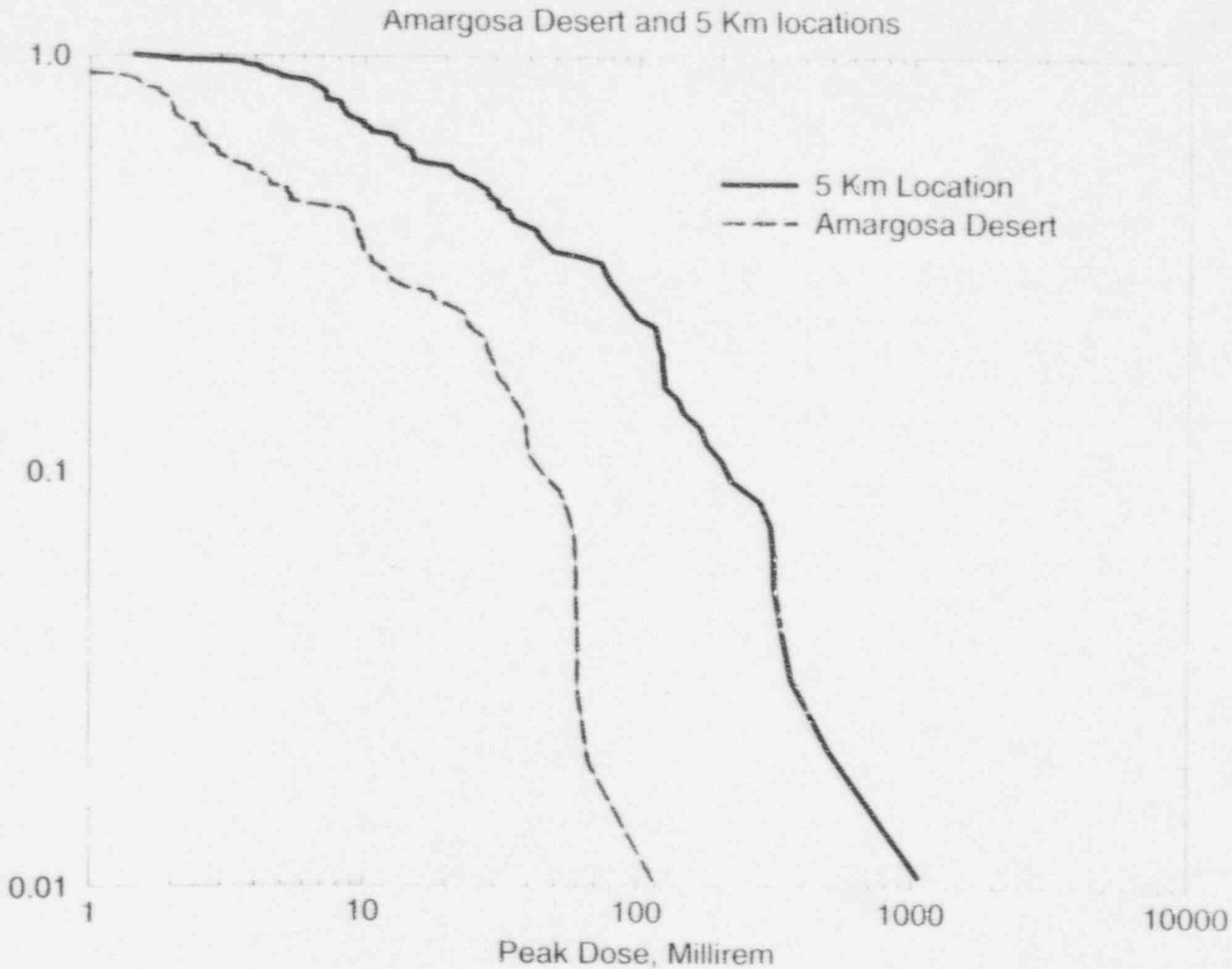
- Nominal base case analyzed

  - probability near unity
  - peak dose (up to 1 million years)
  - compliance at 5 km (drinking water pathway) and 30 km (all pathways)
  - average member of critical group approach for individual dose

- Extrusive volcanism analyzed

  - undisturbed performance, extrusive volcanism, and human intrusion (analyzed separately)
  - compliance at 20-30 km (all pathways)
  - average member of critical group approach for individual dose
  - consideration of magmatic interaction with waste and deposition of ash

CCDF's for Peak Dose, Millirem



NRC Evaluation of NAS

Uncertainty analysis (100 realizations) of annual individual dose

Individual Dose Comparison

(analysis done to evaluate implementation of NAS Recommendations)

CALCULATION	MEDIAN	MEAN	95th PERCENTILE
TSPA 95 (5 km, drinking water)	0.4 mrem		10 mrem
NRC NAS Nominal (5 km, drinking water) (range: 1.5 - 1,060)	23 mrem	76 mrem	104 mrem
NRC NAS Nominal (30 km, all pathway) (range: 0.2 - 118)	4 mrem	14 mrem	18 mrem

∞

U.S. DEPARTMENT OF ENERGY
OFFICE OF CIVILIAN RADIOACTIVE WASTE MANAGEMENT

ADVISORY COMMITTEE ON NUCLEAR WASTE

SUBJECT: **Planned Sensitivity Analyses of the Effects, Consequences and Risks of Volcanic Hazards at Yucca Mountain in TSPA-VA**

PRESENTER: **Dr. Abe VanLuik**

PRESENTER'S TITLE AND ORGANIZATION: **Technical Synthesis Team Leader**
U. S. Department of Energy
Yucca Mountain Site Characterization Project Office
Las Vegas, Nevada

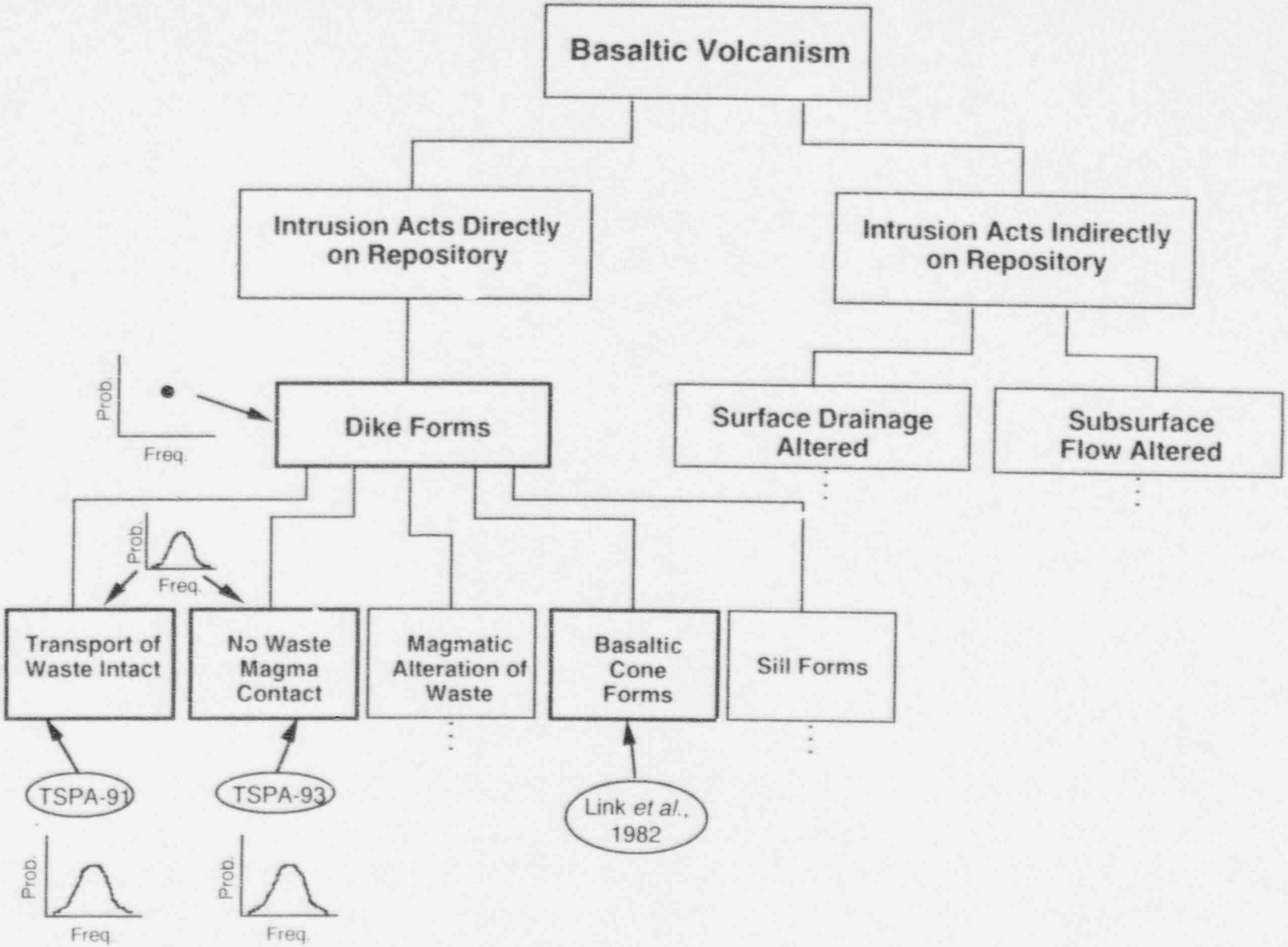
TELEPHONE NUMBER: **(702) 794-1424**

April 24, 1997

Outline

- Summary of volcanic scenarios used in prior TSPA analyses
- Summary of volcanic scenarios to be considered in TSPA-VA
  - incorporation of alternative interpretations of probability of occurrence
  - incorporation of alternative models of direct and indirect effects and consequences

Scenarios - Summary of Prior Work

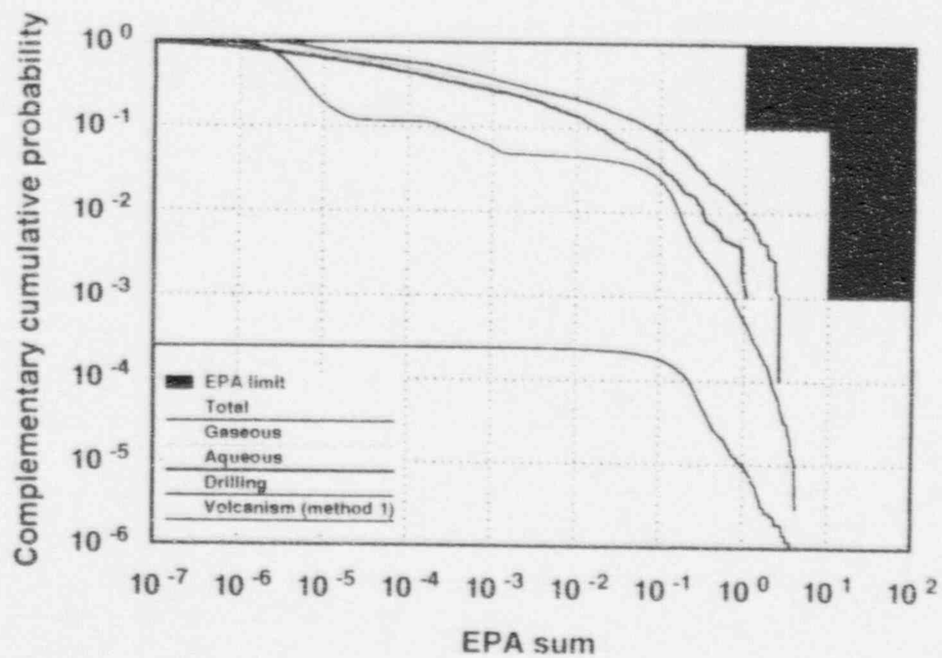


Previous Inclusion of Volcanic Scenarios in TSPA

- TSPA-1991
  - direct entrainment of waste to surface
  - amount of entrained waste a function of volume of dike, extent of wall rock erosion, area of repository intersected by dike
  - random dike orientation, length, amounts of wall-rock erosion
- TSPA-1993
  - indirect effects of intrusive event accelerates waste package and waste form degradation

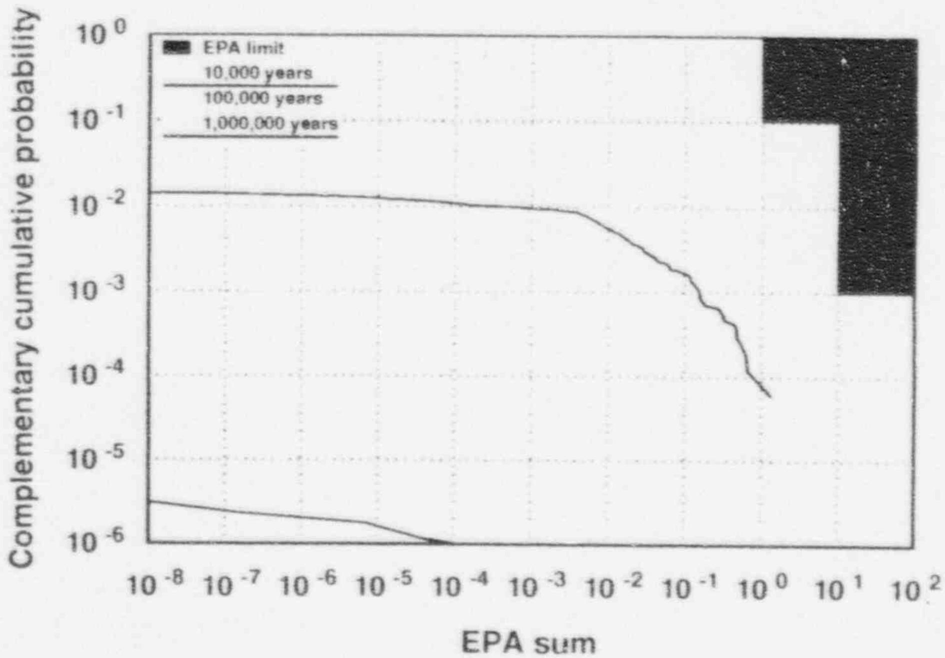
Summary of TSPA-1991 Volcanism Results

- Probability of occurrence of igneous event = 2.4×10^{-4} over 10,000 years
- Release models felt to be conservative
- Maximum release (expressed as old EPA sum) ≈ 1



Summary of TSPA-1993 Volcanism Results

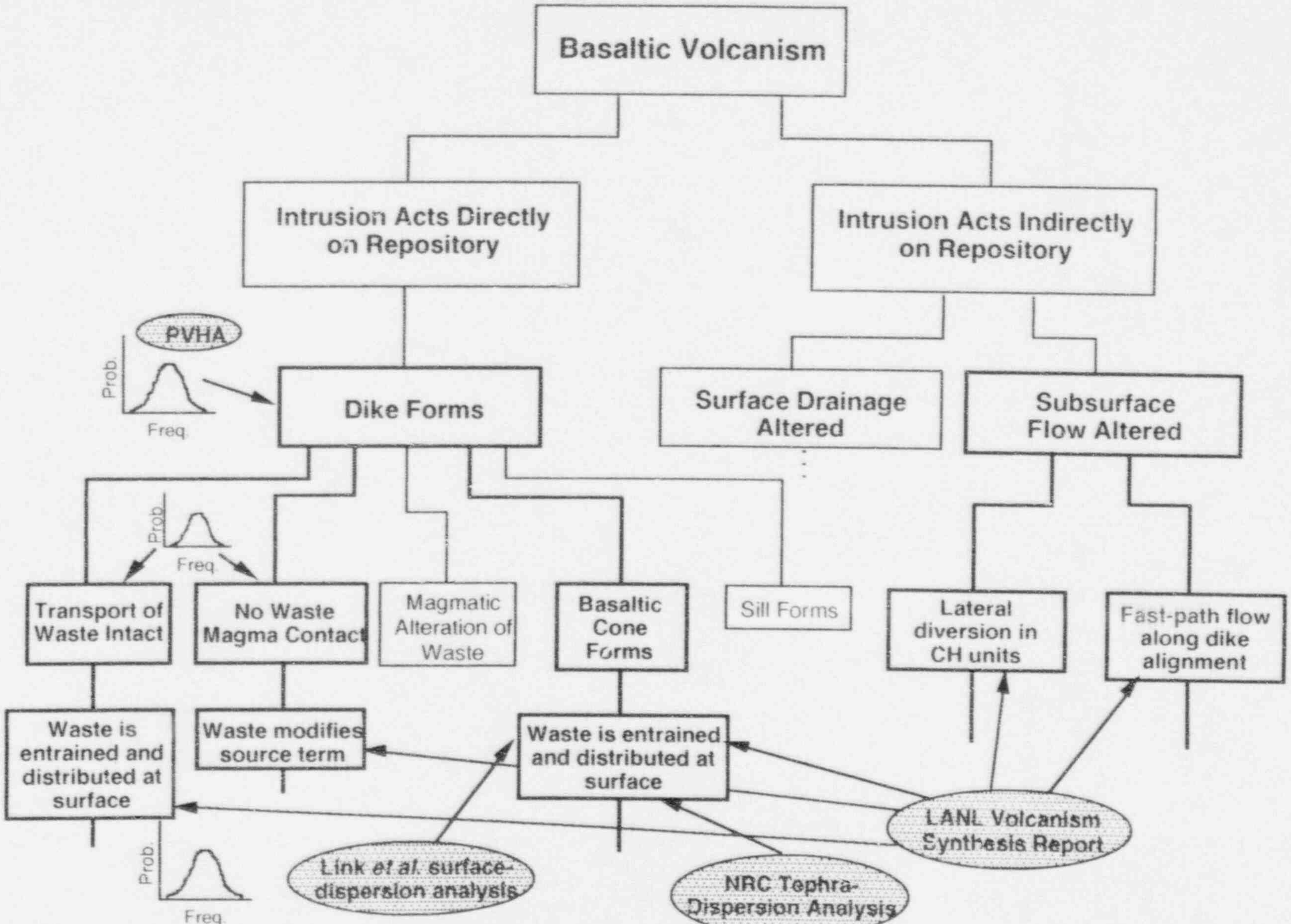
- Both probabilities and consequences are low
    - $P[1|10^4 \text{ yrs}] = 2.4\times10^{-4}$
    - $P[1|10^6 \text{ yrs}] = 2.3\times10^{-2}$
- Maximum release at 10,000 years ≈ 10^{-4}



Applicability of Recent Interpretations

- PVHA
  - provides credible estimates of probability of occurrence (and uncertainty) for intersection of dike with repository
- LANL Volcanism Synthesis Report
  - data on erupted lithics, intrusion geometry, geochemical alteration, vapor convection
- NRC/CNWRA
  - model of direct effects and tephra dispersion from basaltic eruptions

Proposed Scenarios for TSPA-VA



Alternative Models for TSPA-VA: Probability of Disruptive Events

- Use elicited values from PVHA
- Employ importance sampling to assure events are sampled and weighted appropriately
- Use fixed ("best-estimate") direct effects consequence model [direct effects considered to have about 10,000 times greater consequence than indirect effects]
- Consequence analyses use peak dose to average member of critical group located at 5 km and 30 km at times of up to 10,000 and up to 100,000 years
- Evaluate sensitivity to alternative PDFs honoring the elicited frequency of intersections (sensitivity evaluated with alternative CCDFs of peak dose)

Alternative Models for TSPA-VA: Probability of Disruptive Events

- Alternative PDFs to be studied in sensitivity analyses:
  - fix values at 50th and 95th percentile
  - sample from raw pdf (or raw pdf transformed to log normal that honors 50th and 95th percentiles)
  - sample from uniform distribution of elicited median values
  - sample from log-uniform distribution encompassing the 0.1th and 99.9th percentiles
  - discard the one outlier at the low end of the distribution and recompile the pdf
- Document results in TSPA-VA

Alternative Models for TSPA-VA: Direct Effects and Consequences

- Review appropriateness of conceptual assumptions in the CNWRA's model (compare to models used in TSPA-1991 and recent information from Volcanism Synthesis Report)
- Assuming CNWRA model is representative, define reasonable ranges for key input parameters
  - use DOE experts and subset of PVHA external experts
  - CNWRA "bounding" parameters should be represented within the range of parameter values
  - examples: magnitudes of ejected material, depth and percent of wall rock entrainment, dike length and width, erupted material characteristics
- Conduct conditional simulations of consequences (assuming probability of occurrence equals 1) over the range of parameters
- Document Results in TSPA-VA

Alternative Models for TSPA-VA: Indirect Effects and Consequences

- Review previous models bounding indirect effects
- Revise previous models or develop new models, including bounding the effects of
  - enhanced degradation of waste packages
  - enahnced degradation of cladding and waste form
  - revised solubility of radionuclides
  - modified transport characteristics along likely flow paths
  - revised saturated zone flow field
- Conduct conditional consequence modeling of indirect effects using bounding models of indirect effects
- Reconfirm that the consequences of indirect effects are significantly less than those due to direct effects
- Document assumptions and results in TSPA-VA

Summary

- TSPA-VA will utilize recent results from PVHA, Volcanism Synthesis and NRC/CNWRA analyses/interpretations
- Sensitivity analyses of probability of occurrence, direct/indirect effects and consequences will be conducted and documented in TSPA-VA
- If **either** consequences or risks are significant, then volcanic scenarios will be included in TSPA-VA reference case
- If **both** consequences and risks are insignificant, then volcanic scenarios will not be included in TSPA-VA reference case

DOE/NRC AGREEMENTS FROM TECHNICAL EXCHANGE WITH COMMENTS

Presented:ACNW APRIL 22, 1997

By:
John Trapp, NRC
(301) 415-8063
AND
"Tim" Sullivan, DOE
(702) 794-5589

AGREEMENTS FROM TECHNICAL EXCHANGE

1. DOE and NRC agree that the rate of volcanism is relatively constant for the last 5 million years and can be assumed to remain relatively constant for the period of performance.

2. DOE and NRC agree that based on current information, silicic volcanism need not be evaluated.

3. DOE agrees to consider evaluating new data such as:

- **the size and volume of Little Cone**
- **the number of events at Anomaly A**

through hazard sensitivity studies.

AGREEMENTS FROM TECHNICAL EXCHANGE

4. NRC believes that an annual probability of 10-7/yr is a reasonably conservative upper bound for extrusive events. There are differing views on the lower bound. DOE considers that the PVHA provides a defensible basis for characterizing the probability of disruption (includes both intrusive and extrusive magmatic events). The probability distribution function (PDF) has an upper bound frequency of 10-7, a lower bound of 10-10, and a mean of 1.5 x 10-8 per year. DOE agrees to explain how the PDF for the probability of disruption will be used in performance assessment, including sensitivity studies, recognizing NRCs comments.

AGREEMENTS FROM TECHNICAL EXCHANGE

5. DOE and NRC agree that:

A. Volcanism is of regulatory interest and its probability and consequences will be considered.

B. If determined to be significant with respect to repository performance, the effects of volcanism will be included in the total system performance assessment.

6. The treatment of consequences outlined by DOE that includes extrusive magmatic events (cone and dike formation) and intrusive magmatic events (sill and dike formation) with both direct and indirect effects is generally appropriate at the level of detail provided.

AGREEMENTS FROM TECHNICAL EXCHANGE

7. DOE and NRC agree that there is uncertainty in consequence analysis for magmatic waste package/waste form interactions and will be evaluated.

8. DOE agrees to provide the NRC with a letter describing the DOE basis for subissue resolution, as specified in 3 and 4, for consideration in the development of NRC's Issue Resolution Report.

United States
Nuclear Regulatory Commission

NRC PLANNED ACTIVITIES

Presented: ACNW APRIL 22, 1997

By:
John Trapp, Senior Geologist
Engineering and Geoscience Branch
Division of Waste Management
Office of Nuclear Material Safety and Safeguards
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IGNEOUS ACTIVITY KTI SUBISSUES

WORK ORGANIZED ALONG THREE SUBISSUES

PROBABILITY

CONSEQUENCE

DATA QUALITY

PROBABILITY SUBISSUE

IRSR PROBABILITY OF BASALTIC IGNEOUS ACTIVITY

EARLY FY98

TECHNICAL WORK

- **-INVESTIGATE SIGNIFICANCE OF BURIED GEOPHYSICAL ANOMALIES**
- **-PROBABILITIES OF INDIRECT/SECONDARY EFFECTS AND PROCESSES**
- **-EXAMINE CONTROLS ON CONDUIT LOCALIZATION**
- **-EVALUATE MODELS WITH DATA FROM OTHER VOLCANIC FIELDS**

PROBABILITY SUBISSUE (CON'T)

PLANNED PEER-REVIEWED PUBLICATIONS

-INTEGRATED VOLCANISM-STRUCTURE PROBABILITY MODELS — FY97

-VOLCANO CLUSTER DEVELOPMENT, SAN FRANCISCO VOLCANIC FIELD — FY97

-PETROGENESIS OF YMR BASALTIC MAGMA SYSTEM — FY98

IRSR PROBABILITY OF SILICIC IGNEOUS ACTIVITY TECHNICAL WORK COMPLECTED — TBD FY98?

CONSEQUENCES SUBISSUE

IRSR CONSEQUENCES OF BASALTIC IGNEOUS ACTIVITY

EARLY FY99

TECHNICAL WORK

- **-EVALUATE SECONDARY/INDIRECT EFFECTS ON PERFORMANCE**
- **-DEVELOP ADDITIONAL BASIS FOR SUBSURFACE AREA OF DISRUPTION**
- **-MODIFY AND TEST TEPHRA DISPERSAL MODELS**
- **-MODEL EFFECTS OF REPOSITORY ON ASCENDING BASALTIC MAGMA**
- **-MODEL WASTE PACKAGE/WASTE FORM BEHAVIOR**

CONSEQUENCE SUBISSUE (CON'T)

PLANNED PEER-REVIEWED PUBLICATIONS

- **-COOLING AND DEGASSING OF SHALLOW BASALTIC DIKES** **FY97**
- **-TEPHRA DISPERSION AND RISK ANALYSIS, CERRO NEGRO** **FY97**
- **-DEVELOPMENT AND EVOLUTION OF THE 1975 TOLBACHIK ERUPTION** **FY98**

DATA QUALITY SUBISSUE

REVIEW RELEVANT ASPECTS, DOE VOLCANISM SYNTHESIS REPORT

INVESTIGATE SIGNIFICANCE OF BURIED GEOPHYSICAL ANOMALIES

CROSSCUTTING ACTIVITIES

-SENSITIVITY/IMPORTANCE STUDIES - VOLCANIC SYSTEM AND TOTAL SYSTEM
-REVIEW RELEVANT SECTIONS OF DOE TSPA-VA PLAN
-REVIEW RELEVANT SECTIONS OF DOE TSPA-VA
-EVALUATE DOSE SENSITIVITY TO CRITICAL GROUP LOCATIONS AND OTHER PATHWAYS
-EVALUATE DOSE CONVERSION AND PATHWAYS
-INTEGRATE - WASTE PACKAGE/WASTE FORM, STRUCTURE, PA

REPRIORITIZATION
TO FOLLOW SENSITIVITY/IMPORTANCE STUDIES AND BUDGETING

YUCCA MOUNTAIN PROJECT

Studies

Igneous Activity Program

Path Forward

Presented to:
91st ACNW Meeting

Presented by:
Tim Sullivan
Viability Assessment Team Leader
Yucca Mountain Site Characterization Office

April 22, 1997

U.S. Department of Energy
Office of Civilian Radioactive
Waste Management

2TEPVHA.PPT

PATH FORWARD
HAZARD ANALYSES

- The results of the PVHA are intended to provide a sound defensible basis for licensing
- DOE has evaluated new CNWRA data and these data do not significantly impact the results of the PVHA
- The results of PVHA are robust; new information is unlikely to change the disruption probability documented in the PVHA

PATH FORWARD
HAZARD ANALYSES

- No further site characterization data collection planned; new data are unlikely to change results
- Disruption probability estimates from PVHA will be used for the reference case and sensitivity studies as described earlier
- DOE will address TE agreements (# 3, 4, and 8) for evaluation of new CNWRA data and description of the use of hazard results in TSPA-VA in an upcoming letter
- Next step is hazard probability subissue closure

PATH FORWARD CONSEQUENCE ANALYSES

- Consequence analyses for direct effects will be developed by DOE for TSPA-VA
- For indirect effects DOE will review previous bounding effects considering new information and reevaluate the earlier conclusion that indirect effects are several orders of magnitude less than direct effects
- Document assumptions and results in TSPA-VA
- This approach is consistent with TE agreements (# 4,6, and 8) for consequence evaluation
- Next step is consequence subissue closure

PATH FORWARD
TSPA-VA

- Igneous activity scenarios wil be evaluated for TSPA-VA
- If either consequences or risk are significant, DOE will include volcanic scenarios in TSPA-VA reference case
- If both consequences and risks are insignificant, DOE will not include volcanic scenarios in TSPA-VA reference case and will document the rationale for this approach in TSPA-VA