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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

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BRIEFING ON

STATUS OF ESTIMATING PERFORMANCE OF IGNEOUS ACTIVITY

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THURSDAY

FEBRUARY 7, 2002

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ROCKVILLE, MARYLAND

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The Commission met in at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, at 1:30 p.m., B. John Garrick, Chairman, presiding.

COMMISSIONERS PRESENT:

B. JOHN GARRICK, Acting Chairman

MILTON N. LEVENSON, Member

ACNW STAFF PRESENT:

HOWARD J. LARSON, Special Assistant, ACRS/ACNW

RICHARD K. MAJOR

LYNN DEERING

ANDREW C. CAMPBELL

LATIF HAMDAN

SHER BAHADUR, Associate Director, ACRS/ACNW

JOHN T. LARKINS, Executive Director, ACRS/ACNW

AMARJIT SINGH, ACRS Staff

ALSO PRESENT:

BILL HINZE, ACNW Consultant

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

(1:30 p.m.)

CHAIRMAN GARRICK: Come to order. As we announced this morning, my name is John Garrick, accompanied by Milt Levenson, another member of the committee, and Bill Hinze, a former member of the committee and consultant. We are short a couple of members of the committee -- George Hornberger, the Chairman, for compelling reasons, and Ray Wymer, Vice Chairman, for other reasons -- so we're going to have to do the best we can. This topic we're going to start off with, the member who has the lead on it happens to be Hornberger but, in his absence today, we're going to lean heavily on former member Bill Hinze and his distinguished knowledge about this topic.

So, with that, I think we will go ahead and start. Tim, are you going to start it?

MR. McCARTIN: And end it hopefully.

(Laughter and simultaneous discussion.)

MR. LARSON: If I were introducing this topic, I'd say, "This is the cat, the cat that has had nine lives, or more".

MR. McCARTIN: Thank you, Dr. Garrick.

(Slide)

Today I'll be talking about the status of estimating performance for igneous activity, and you're right in that I'm starting it. I will give the entire presentation, and let me just say the reason for this is, what we are hoping to do today is give a perspective not so much in terms of the detailed geologic igneous processes involved, but more how we're representing it in the performance assessment and what it means in terms of its implications in terms of the dose.

We'll be talking about the differences between ourselves and DOE, et cetera, but today what we're hoping to do is talk more about how things are abstracted in the performance assessment and not really the detailed geologic processes, why I'm here rather than someone like Brit Hill from the Center of John Trapp from the staff. And John Trapp is notable by his absence. He is recovering from some recent surgery, which is why he's not here. I'm sure he'd rather be here than recovering from surgery, that's for sure.

(Simultaneous discussion.)

MR. McCARTIN: In terms of how I'm going to present things, it's not so much in order, but the topics that I'll address, and what I'm hoping to do is give some small insight on the uncertainties in estimating volcanic disruption of the repository, talk to differences between the NRC and DOE in our approaches for representing this in the performance assessment, talk to a path forward and, at the very end -- and I'll do those three topics in the context of the "big ticket" items for estimating the performance of the repository.

At the end, I'd like to talk briefly about the treatment of uncertainty in NRC's TPA code, and I'm going to draw parallels to what we do in igneous and in the waste package area. It's the beginning of an effort to scope out how we're dealing with uncertainty in the entire code, and we hope to certainly pull out are there discrepancies. We want to see, as Marty Virgilio talked about earlier today, we want to see a consistency in the way we're dealing with uncertainties, get a better handle on it -- it might get to some of the treatment of conservatism, et cetera -- but that's at the very end, and it's a small nugget. We probably will be coming back to it maybe later in some subsequent meeting when we have it fleshed out for the entire code.

(Slide)

First, as just a general overview -- and this pretty much gets to what I'll be talking about -- what are the areas of uncertainty? Certainly, we have estimating the probability, and then after you get, if it will occur, the consequences, and there we have -- I'll talk to five particular topic areas -- the interaction of magma in the repository, magma in the waste package, magma in the waste form, the redistribution of volcanic ash after the event has occurred, you have an ash deposit, how may it redistribute in time, and then, ultimately, for the exposure scenario, the inhalation is the dominant pathway. And so I'll be talking to each one of these with respect to those -- the uncertainties, the approaches, and the path forward.

CHAIRMAN GARRICK: Are there areas that we should have our attention called to that have gone through some change in the last few months?

MR. McCARTIN: I'll try to address that. I think there have been some changes in the DOE program. I certainly will be talking to things that we're in the process of examining and I think in the next six months to a year, we -- I won't guarantee that we have changes, but we'll have hopefully at least increased confidence in the approach we have if it doesn't change. But I will try to talk to that as

I go through.

Right now, in terms of resolutions, there are agreements in place for all these that we think the DOE will address the issues. However, one thing I think the staff certainly recognized that we aren't going to reduce all the uncertainties in any area of the TPA code, and uncertainty is something we need to live with, and we think that we want to see a treatment of uncertainty commensurate with its importance to risk, and also a recognition that information in the performance confirmation period will continue to be collected and also will shed some light on the uncertainty.

CHAIRMAN GARRICK: The issue is not to eliminate the uncertainty, the issue is to quantify them.

MR. McCARTIN: Yes, exactly. And what you will see today is, really what I am trying to show is that what we are trying to do is get a better understanding of the uncertainties and possibly use that to refine the agreements and help in our getting ready to review a DOE license application.

(Slide)

With that, let me go to the first area, which is the probability of an igneous disruption.

Basically, in terms of the issue, estimating the probability of disruption is related to how many volcanoes are currently there from the past, and looking at subsurface geologic features. Those are two parts of, we believe, in estimating the probability.

In terms of the NRC approach, we're using the currently identified features from the past, and that is approximately 13 events over the past 11 million years, and using subsurface geologic features to better constrain what might be the probability of disruption at the Yucca Mountain Repository.

Nevada is using a smaller subset of that. They are using primarily volcanoes that have occurred over the last million years comprising two to three events. They use that in the defined particular zones, some narrow zones that tend to focus on the Repository.

MR. LARSON: Is that Cole (phonetic) and Smith, or is it Smith and Kenno (phonetic), or who is it?

MR. McCARTIN: With that, I'll refer to Brit.

MR. HILL: This is Brit Hill, from the CNWRA. That's the series of models from Smith, et.

al., 1990, also a series from the Cole and Owen Smith.

MR. LARSON: Does this take into account the recent work of Smith and his student Kenno?

MR. HILL: No, it does not.

MR. McCARTIN: Notably, the zones are not constrained by subsurface geologic features. DOE's approach, they are constrained -- their probability probably primarily on volcanoes over the last five million years, that's approximately seven events. They just define some broad zones, some of which miss the Repository.

They also are not using geologic features to constrain the probability.

MR. LARSON: Is that really right? Is that really right? It seems to me that DOE's approach does use geology in the sense that they are limiting -- most of the source-zones are limited to crater flat, and also they use topography, which is certainly, in this area, part of the geological regime.

MR. HILL: Again, this is Brit Hill, from the Center. When we say the zones are not bounded by geologic features that localize volcanoes, we've addressed the technical basis on why alluvium or small topographic changes do not localize volcanoes, and are restricted solely to the crater causation. And zones that are defined based on features that do not demonstrably localize volcanoes are the ones that are in many of the source-zones that bypass the Repository.

I would also point out, in the PVHA report, when the experts talk about how they define the zones, they clearly do not make a linkage to exclusive geologic features.

(Slide)

Now, with that, what does it mean quantitatively in terms of where the various groups are at, and the horizontal lines on the left are depicting the range of probabilities that were seen estimated by the three groups. You can see, State of Nevada-sponsored work is somewhere in the 10^{-6} to 10^{-8}, NRC is around 10^{-7} to 10^{-8}, and the Department of energy is somewhere -- a little bit of both, 10^{-8} down to 10^{-10}. The Department's mean value is approximately $1.6x10^{-8}$, which has it in that area of probability space where it needs to be considered in terms of the Yucca Mountain Repository, our probability cutoff being approximately 10^{-8}.

CHAIRMAN GARRICK: Now, are they all using the same time interval? I thought I heard you say that Nevada was using a different time interval.

MR. McCARTIN: Well, in terms of what are the relevant events that they're using to determine the probability, you're right. The easiest way to put it, there's not an off-the-shelf thought to here's how you determine --

CHAIRMAN GARRICK: So, these are not all against the same database.

MR. McCARTIN: Correct. Well, people are using different parts that they believe are more relevant, and it is a matter of opinion. The Department -- and I'll let Brit add anything -- but the Department feels the most relevant events are those in the last 15 million years, and they have a basis for not including --

CHAIRMAN GARRICK: But this is such a straightforward kind of question that we're asking here, namely, the likelihood of the event. And it would seem that you would not want to sweep any knowledge under the rug that would shed any light on the frequency of that event, of the recurrence of it.

MR. HILL: Brit Hill, from the Center. First, a minor correction. The Department is heavily focused on 5 million year and younger, not 15 million year and younger.

MR. McCARTIN: Oh, I thought you said 15.

MR. HILL: I wouldn't characterize it as "sweeping it under the rug" or anything like that, except a difference of opinion by what is the most relevant in determining recurrence in the next million to 10,000 years. Whether you are using a tighter range of information because of your understanding of process, how these were formed by the same process or not, or believe that only the last million years is the most important for the next 10,000 years --

CHAIRMAN GARRICK: Of course, the discriminator here would preclude the need for asking the question would be if we saw the probability of distribution, we'd be able to see what the shorter time interval meant in terms of the uncertainties versus the longer time.

MR. HILL: Right, in trying to gauge a model that's based on recurrence rates of 1 to 2 events per million years, and you think about space and time, and then look at -- you're trying to focus on a 10,000-year interval which is so much shorter than anybody's recurrence rate. I can give you a sense of why, for example, our position is that really an order of magnitude construction of probability is about all that's warranted because both the time interval, sparsity of data, and the long recurrence doesn't warrant a large significant figure.

CHAIRMAN GARRICK: Well, the only thing is that if it comes down to -- if it's a 10^{-7} value that's important, and a 10^{-8}, or whatever the line is, is not important, then it becomes more important to turn up the microscope a little bit on the different assumptions that are made about the input information for the distributions.

MR. HILL: Just one final point. It's not really the input assumptions that are driving this, it's the alternative conceptual models. And whether we want to take a single tendency from a series of alternative conceptual models or not is not at all clear at this stage.

CHAIRMAN GARRICK: Well, I didn't mean to spend so much time on it, but I just wanted --

MR. LARSON: There really has been very little -- very few attempts at this kind of prediction until this problem came up, and the predictions were either on a very short-term basis -- an ad hoc basis of days, hours, months -- and the longer-term like we're talking about has just not been approached. So, we don't have standardized procedures -- I think that's what Tim said, that we don't have standardized procedures. And so while we're doing this job, we're trying to find those procedures.

CHAIRMAN GARRICK: The only thing I can think that comes close to this is the frequency of occurrence of very strong motion earthquakes.

MR. LARSON: Absolutely. There's a lot of analogy there. In terms of short-term prediction versus long-term prediction, these kinds of things, this is the same problem, except that you have more of an historical record with seismicity which gives you a better chance of doing a decent job. And, also, we understand that earthquakes are associated with faults. Frankly, we have a really difficult time pinning down what's really happening in the mantle that's generating volcanic magmas.

Cognizant of your preamble, Tim, about not getting into too much detail, but also recognizing that you are talking about uncertainty, it strikes me, when I see this diagram, I can recall during the PVHA very lucid discussions of the NRC approach were made by Brit and by Chuck Connor of the Center, and John Trapp. I don't know whether I attended those or not, it's been too long ago, but if I understand correctly the results here and your previous slide, the ten experts in volcanology that were selected by the DOE to run -- to be involved in this expert opinion, chose not to accept the approach that is being used by NRC today.

Can you understand the uncertainties that develop as a result of this? Can we understand why they failed to accept this? Does this cast any light upon the uncertainties that we see not within NRC's work, but between NRC and DOE?

MR. McCARTIN: Well, in terms of between NRC and DOE, I think it's -- in terms of -- I wasn't involved in the PVHA either, so I don't want to talk to that. I'll ask Brit to comment, if he'd like. But, generally, this diagram here is showing the variation of probability spatial based on the NRC approach, and you can see the Repository loosely is in this 10^{-7}-10^{-8} range, and I think, obviously, these aren't very -- you see variations in these isopleths, and I think you're seeing the effect of structure, which I think is useful in terms of new information that could come in with the aeromagnetic data, but you're seeing -- and I think Brit's right, in order of magnitude, somewhere between 10^{-7} to 10^{-8}. From my standpoint, I look at the DOE current number, $1.6x10^{-8}$, they are in that same range -- I don't know if one could say we're in that big a disagreement at this time. And as I said before and you indicated, there is no textbook on how to do this, and there is opinion, there is what-features-matter, et cetera, but I don't know if Brit has anymore of what happened at PVHA.

MR. LARSON: I just wanted to make the point that -- and I do respect the NRC's approach and people that are doing it very much, I respect them very much, but I'm also struck by the fact that the expert elicitation seems to disregard the NRC approach, and I wonder where this is going to end up when you come to some kind of judicatio on it.

MR. McCARTIN: One thing that at least I, from my perspective and more PA rather than -- certainly, more PA than igneous -- but I think, as my next slide will allude to, we're expecting to get new aeromagnetic data that has been alluded, that there is possibly 13 more identified events in the area.

When I look at what I've heard and my limited knowledge --

CHAIRMAN GARRICK: Thirteen more, did you say?

MR. McCARTIN: Yes, potentially -- potentially. It's still being evaluated, but it is possible. Now, remember, you've got to look at the time period, too.

MR. LARSON: Yes, location and the timing is everything.

MR. McCARTIN: Is key, yes. But I think when that information comes in, if I had to guess, because of the fact that we use structure, subsurface geology, to help constrain these numbers, I don't believe the identification of those events will have as big of an effect on the NRC estimate as it will on potentially the State or DOE, and that's purely from my very novice opinion, and does the NRC have a more stable estimate because we have this other constraint there that allows for -- this additional information, depending on where it is, there are some bases. And I don't know if Brit has --

MR. LARSON: But there was more than just geological structure, I think it also had to do with the length of the igneous dikes that were going to be associated with it, and perhaps also the number, is that correct? There's a difference between the NRC approach and the PVHA approach.

MR. HILL: Again, this is Brit Hill, from the Center. There are a number of questions that have been raised. Going back to why the Center approach wasn't used, I think it's just important to clarify that the models that you see here that have geologic structure incorporated into the clustering algorithms, those models were not developed by the PVHA, they were subsequent to it. So, this really represents our preferred approach was not available at the time the DOE conducted PVHA in 1995, so there's been a difference in the available models, first of all.

Second, to clarify on the aeromagnetic anomaly -- of course, that is fairly new information -- the U.S. Geological Survey just released until it could file a report, an interpretation that says that these anomalies that are shown on the figure in the labeled letters are -- can be interpreted as varying basalt. It's not just the location of those features, but the age of those features that's going to affect all models, not just the State and DOE's, but it may affect ours as well. All assumed occurrence rate is fairly uniform and doesn't cluster in time. Until we have a better handle on the age, it's very difficult to say what the effect could or could not be. But taking a recurrence rate of seven volcanoes over 5 million years to 20 volcanoes for 5 million years could have a significant effect on any probability model.

MR. LARSON: Can I ask another question?

CHAIRMAN GARRICK: Sure.

MR. LARSON: Going to this aeromag survey -- and certainly it very much bears on the uncertainty -- has the NRC considered the number of possible basaltic features that might have been uncovered if that survey had been flown with the specifications appropriate for a survey designed for detecting these? This aeromagnetic survey was conducted to map hydrologic features. It was flown in a direction in which you cannot adequately map dipolar features that are small -- and I've written to you about that in a report -- and, also, if this is so important to determining the igneous concerns, why isn't this magnetic survey extended into other regions, to the north and to the east? There is a 300,000 year old volcanic feature to the northwest, off of -- called --

MR. HILL: Thirsty Mesa.

MR. LARSON: -- Thirsty Mesa, thank you. This survey doesn't go up into that area. If this is so important, why aren't you pushing to get appropriate data to do the job?

MR. McCARTIN: Once again, Brit will have to answer that. However, what we're trying to do here is give you a sense of the differences between ourselves and DOE, and we certainly can go into that detail, but Brit is going to have to answer that question.

MR. HILL: Well, we had looked at available information at the time, and thought that the Cain and Bracken (phonetic) survey, the existing aeromagnetic data, we had evaluated that and, as you are aware, conducted a series of ground magnetic investigations to look for potentially varying characteristics, and we felt that with the available information we didn't have a significant concern. Now we're seeing new information from this aeromagnetic survey. And I agree, it was not flown designed to look for these features, but it was a target that provided the Department a chance to mine the existing data and come up with new characterization information. Now we have the report, and it's raising some very potentially significant concerns about how many features, varied igneous features -- it doesn't mean intrusive, necessarily, it could be buried volcanoes -- really are within an area that could affect our understanding of the probability.

MR. LARSON: Does that open up then other areas for further investigation based upon what you're seeing here?

MR. HILL: We're going to have to have discussion with the Department of Energy fairly soon, once we've had a chance to evaluate the open final report that came out last week, but this is the preliminary interpretation, which is also part of the technical basis impact weather report where the preliminary interpretations were that there were additional features, but they would not affect Department of Energy probability models.

MR. LARSON: Well, if you find all these to the south and west of the Repository, I just wonder what's up there to the north and west, because we do have one that certainly falls within the time span of anyone's evaluation of the volcanic processes in the area.

MR. HILL: We're very concerned about noisy terrain both adjacent to or east of the Repository as containing present but undetected igneous features.

CHAIRMAN GARRICK: Tim, this is something -- do these numbers within these contours, say, the 10^{-7} to 10^{-8}, do they reflect your uncertainty about the likelihood, and is that all informational as opposed to modeling?

MR. McCARTIN: Well, in this area, certainly these are related to models that the Center has used, and there's variation within those models.

In terms of whether modeling uncertainty or uncertainty of another kind, I think Brit has to talk to variation there.

MR. HILL: A very good answer is that the uncertainty in the parameters, and also alternative conceptual models. It does not capture the model uncertainty. We have not evaluated quantitatively the uncertainty in each probability model and factored that in as an uncertainty.

CHAIRMAN GARRICK: Right, because this suggests a pretty high level of certainty.

MR. HILL: We did not overinterpret each line being a solid line that's fixed to the ground.

CHAIRMAN GARRICK: That's the only reason I asked the question, don't want to do that.

MR. LEVENSON: Tim, I have a question. Just because somebody wrote the words down forces me to ask the question. In the Center review of DOE's elicitation of expert opinion was criticized for what ten people said greater balance is needed on the panel to encompass a wider range of viewpoints, and also potential conflicts of interest. Is the same criticism potentially applied to inhouse version? You'd probably have less than ten people.

MR. McCARTIN: Well, we certainly had less than ten people. That's -- I'm not sure how to answer that question.

MR. LEVENSON: I wouldn't have raised the issue, but you people raised it.

MR. McCARTIN: Right. I mean, there's a difference in looking at DOE, what they need to do to provide this information. We certainly are working to develop our own understanding here -- and I'll get to you in a second, Brit, I know he wants to add something here.

We have gone for peer review and outside review of what we've done. We certainly have not done a separate expert elicitation.

MR. HILL: We have not held our work out as an expert elicitation. We put it as expert judgment, and we put it in peer review literature, but we have not tried to portray this as a consensus.

MR. LARSON: Well, I don't want to come to anyone's defense here. God help me, I don't want to do that. But I sat in on some of the reviews of -- for the peer reviews of the Center's work, and although there were problems, there was general consensus that this was really headed in the right direction.

In addition, when many of their

publications came up for review, I happened to be editor of the Journal and they fell in my lap, and I don't think I've ever given anything as tight a review as the papers that came in from the Center. Instead of having two, three reviewers, they had five or six, including members of the DOE staff. So, I think that this has been looked at by the public and been pretty well received.

CHAIRMAN GARRICK: Is that articulated in your database and in your supporting information, what Bill just described?

MR. HILL: I'm not sure what you mean by supporting information.

CHAIRMAN GARRICK: Well, that's pretty valuable evidence, it seems to me, what he just described.

MR. HILL: It's not part of the record.

MR. McCARTIN: In general, though, our approach has been to make sure DOE -- what information do they have to bring forward to support so that we can review their license application?

(Slide)

Continuing -- hopefully I'll be able to pick up the pace.

CHAIRMAN GARRICK: That's not your

problem.

MR. McCARTIN: In terms of probability, we've pretty much discussed this quite a bit. As I said, the new aeromag data is coming in. From a risk standpoint, what we see now is there's an approximately an order of magnitude difference in the probability between ourselves and DOE.

MR. LARSON: DOE keeps talking about their probability assessment as being very robust, and I think perhaps if we asked you, you'd say the same thing.

What's going to happen with -- let's say that just a few of these anomalies are volcanics that fall within a range of distance and time that are interesting. What's this going to do to 10^{-7}, 10^{-8}? What are we worried about? Are we so -- is this whole approach so fragile that finding another volcano or two is going to send this catapulting to an even higher probability?

MR. McCARTIN: Well, as indicated here, if you ask for a best-guess at the moment, we're looking at a 2 to 5 times increase in probability.

MR. LARSON: On the basis of 13 more volcanoes in their position, in their place, as you have them? So you analyzed them?

MR. McCARTIN: Well, what we're trying to do is give you where we're at today and where we think things may end up. And to say -- I mean, I don't think we want to be held firmly to anything here, but trying to give you an informed estimate that's where we think it might end up.

Is that a big deal? I think what I'd like to do is look at all the uncertainties we have and look at it from that standpoint.

MR. LARSON: But, Tim, this is really the critical uncertainty. The DOE oftentimes approaches this, and many people approach this, from the standpoint that you're multiplying zero times any number is still going to be zero, and such a low probability -- the probability is the backbreaker on this.

MR. McCARTIN: Well, in one sense. I don't believe that's so much the case currently, and for this reason, that if it was below 10^{-8} and now we're looking at a decision to whether it's considered or not considered and it's screened up, that's not the case. Right now, DOE is, like I said, approximately $1.6x10^{-8}$, so it's being considered.

And at least from my standpoint, not looking at whether it's probability or some other factor, if the estimate -- clearly, if the probability increases by a factor of 5, it does increase the overall risk by a factor of 5, but there are many other aspects of the calculation that have similar effects, either raising it or lowering it. And so this is one of those factors, but if it were not in a position of whether it's considered or not considered -- and that's where I think the critical aspect was. Whether it's 10^{-8} or 10^{-7}, I don't think is as critical as it's going to be considered or not going be considered, at least from my perspective.

CHAIRMAN GARRICK: Tim, we're sorry to press you the way we are.

MR. McCARTIN: No, no. It makes it more interesting for me.

CHAIRMAN GARRICK: Let me ask this question. Supposing that the TSPA/LA comes out with a much more robust analysis that shortens the time to the peak dose dramatically, as well as the magnitude, and we already see scenarios where the peak dose time varies from a few thousand years to a million years, and it is swung dramatically by certain parameters and certain performance characteristics, such as actinide solubility and -- so, if it turns out that in the license application or in the analysis that's going on now that's a follow-on from the SSPA, that this dose drops down and the time at the peak dose shortens dramatically, does that impact the significance of the 10^{-7}?

MR. McCARTIN: In terms of -- you're saying if the base case dose shortens and --

CHAIRMAN GARRICK: Yeah. It's one thing to talk about recurrence of the order of 10^{-6}, 10^{-7}, when you have a situation where the worst part of the problem is of that same order in terms of time, but it's another thing when that time dramatically shortens as well as the magnitude of the dose. And I'm just asking if the performance calculations change dramatically, does that change the significance of the recurrence interval for igneous activity?

MR. McCARTIN: Well, I guess I still have to ask, with respect to the base case, you're saying that the base case scenario occurs much earlier --

CHAIRMAN GARRICK: Yeah.

MR. McCARTIN: -- so that there's not --

CHAIRMAN GARRICK: I'm saying if they come forward with --

MR. McCARTIN: -- would they add together rather than be separated in time?

CHAIRMAN GARRICK: -- yeah, one in ten

million year event, how significant is that when now you're talking about something of the order of 10^{3rd} years and a much lower peak dose? It seems to me that you've got to reassess the significance of --

MR. McCARTIN: I think that the information that DOE will give us, we'll be able to see what the base case results -- what the disruptive ones are. The Commission will have to weigh that in terms of what does that mean, if these two are additive and now they are slightly above the standard.

CHAIRMAN GARRICK: Now, picking up on something that Bill said earlier alluding to the fact that the DOE has come forward -- feels that their recurrence interval calculation, their probability calculation is very robust, and yet they have kind of caved in to the NRC number. Does that make sense? Have they been able to -- have you been able to demonstrate to them that your number is comparably robust and that's why they chose to do this?

MR. McCARTIN: Well, in this particular area, there is an agreement that the DOE can come forward with a license application with their probability number.

CHAIRMAN GARRICK: Oh, I see.

MR. McCARTIN: They have agreed to also,

as the sensitivity analysis, to include analysis with the 10^{-7} value, to provide that additional information, but it's sort of a two-prong kind of thing.

CHAIRMAN GARRICK: Okay.

MR. HILL: Just perhaps one very brief point, the reason that we have that agreement is because at the staff level we had fundamental disagreement with the robustness of DOE analysis.

CHAIRMAN GARRICK: That's what I was curious about.

MR. HILL: In August of 2000, we had a technical exchange that seemed like we were really in a fairly impractical position on several of the issues, and determined that if we got the analysis as part of the TSPA/SR and TSPA/LA we agreed would have, in our view, an adequate basis at 10^{-7} at that time. That would provide us with enough information to reach a licensing decision, and we would not need to move the issue forward, but it's more that this was to address specific technical concerns at that time.

CHAIRMAN GARRICK: See, I have a --

MR. HILL: It was not a matter of we were just insisting on this number for some unspecified reason.

CHAIRMAN GARRICK: Yeah. See, I have an ulterior motive here. I'm trying to extract from this whole process how much of it is compliance-driven and how much of it is risk-driven.

MR. McCARTIN: In terms of the path forward, there's really two parts of it. The second -- that last part we've talked about enough, the 10^{-7} value that DOE will use, but the first part -- I think both ourselves and DOE will be analyzing the aeromagnetic data and updating estimates, and I think including uncertainty is a key part of that, that I think one of the things we'll be looking at, does it make sense to have a single value, or should we be sampling from a distribution --

MR. LARSON: Does that mean that DOE will go beyond the report that was published this year, of the USGS? Is Los Alamos continuing their work on this?

MR. McCARTIN: Well, right now DOE, I think, is updating a lot of their plans and what they are going to accomplish, and when. And I don't know if I want to try to speculate what DOE is going to do. I assume they will analyze this information, but in what time frame and how, I don't know. We would expect to analyze -- to speak more for ourselves, and

I put 2002 there -- we're going to analyze this information this year. I would expect DOE to do the same but, like I said, they are updating their plans.

(Slide)

I finally got off probability.

CHAIRMAN GARRICK: That was the easy one.

MR. McCARTIN: Uh-oh, I'm in trouble. In terms of getting in more now to the consequences in terms of the Repository magma interactions, the issue is one of when magma rises up and there is a drift, there's obviously a path there that it can take down the drift, and the question is one of what's going to happen in terms of an alternative pathway. Rather than continuing straight up to the surface, the magma goes down the drift and breaks out at some more distant part, thereby intercepting more waste packages. And so it has to get to how many waste packages are intercepted in the event.

Alternatively, the NRC approach, we still have a single vertical conduit, 1 to 10 waste packages are intercepted, it depends on the diameter of the conduit, on average 5 waste packages. However, there are these numerical experiments and calculations being done to look at what happens when the magma hits the drift, and there are evaluations for that at the current time we are looking at it is on average. It could be as high as 100 waste packages, depending on some assumptions in terms of how far down the draft things go. But if we represented today this alternative, you'd be looking at approximately 100 waste packages versus the 5.

DOE has a very similar approach right now of looking at a single conduit up through the Repository. They sampled somewhere between 3 to 30 waste packages, a median value of around 10, as part of this vertical conduit. They have agreed to look at this alternative approach. We haven't seen any of their analysis related to this, but that's part of the --

MR. LARSON: Is the alternative approach only for dikes, or is it for a pipe?

MR. McCARTIN: Well, if the dike comes up, intersects the drift, the Repository drifts, and then the question, where does the conduit form? And it's the dike actually that's intercepting the drift, and could intercept multiple drifts, which is how we get the possibility for 100 waste packages, and it is on average 2 drifts are intercepted and where conduits form.

MR. LARSON: Maybe the committee knows

this already but, if not, just what is NRC doing in this area at this time, have you people learned that? Do you know what --

MR. McCARTIN: Well, I thought in August Brit Hill gave a presentation to the committee in terms of the -- both there are numerical calculations going on with laboratory experiments, analog experiments, to try to corroborate the numerical calculations. I don't know if Brit wants to add to that at all, but --

MR. HILL: We also went over this last month.

CHAIRMAN GARRICK: The one thing that has always been difficult for me to understand, and not being an earth scientist, it's not difficult to understand why I don't understand, but it's one thing to get these recurrence intervals on the basis of the kind of data we've been talking about, but what do you use to establish your insights on pathways and depths and magma flow and what have you? Where do you get that information? How do you get that information?

MR. HILL: There's a lot of sources of information. At first, we are strongly based on an analog approach where we looked at similar volcanoes that have been recently active. Second, on the flow pathways, we're taking a stronger numerical and analog experimentation approach because we can't go down 300 meters and look at these sorts of conditions. And it's unprecedented that ascending magma has intersected a horizontal void of any extent at these kind of depths. We hear a lot of tubes, but they are very, very shallow, and so you don't have the same sort of depressurization and flow phenomenon that you would expect down at 300 meters. That's in part why we've been doing a lot of the experimentation and are talking about this year continuing with fluid dynamic flow experiments to better look at this kind of a process at the appropriate scale for fluid containing a lot of gas. But there really is a lot of information out there on how normal volcanoes of similar composition and volume and character, how the volcanoes erupt in nature. We can glean an awful lot of the physics and fluid dynamics just by very straightforward observation and simplified modeling. And, remember, we're talking about a pressurized fluid intersecting an atmospheric void.

CHAIRMAN GARRICK: Right. And I guess the issue of backfill comes into this in a significant way.

MR. HILL: Certainly.

MR. McCARTIN: From a risk standpoint, obviously we are looking at an approximate order of magnitude, when you look at 1 to 10 waste packages, of up to 100 on average.

In terms of what's going to go on to move the path forward, DOE -- we're expecting them to analyze this scenario and come to some conclusions.

From NRC's standpoint, as Brit indicated, we are continuing with the numerical analyses in some of the experiments. In September of this year, we expect to have some additional information on verification of the model and the experiments, and as you indicated, backfill, and that's exactly what this last tic is, looking at the consideration of Repository design in terms of how this impacts the number of packages that could be affected. Backfill certainly has a big impact. Right now, the design is not to backfill the drifts, but the access tunnels, et cetera, would be backfilled, and that has some implications.

MR. LARSON: Are you having any peer review of the work that you are currently doing, verifying the numerical models and the analogs, the laboratory analogs?

MR. McCARTIN: A couple of them. Well, I

guess it was about two years ago we had -- well, some might call it a peer review, we certainly brought in a number of different experts in different areas to review what we were doing with the TPA code, and got review comments, et cetera -- maybe an expert review rather than a peer review. And certainly the Center, as a group, tries to publish in a number of journals. NRC staff did the same thing, and so we continue to publish. I don't know if there are any explicit plans for any specific review of a particular topic.

MR. HILL: The publication is to elaborate very briefly. We've been having trouble getting the two reports that we talked about earlier last year, getting those reports accepted for review because the topic is deemed too esoteric to appear in Geological Journal. So, we have yet to receive --

CHAIRMAN GARRICK: I knew these scientists were stuffy.

MR. HILL: But we're continuing to resubmit to different journals that have a little more dynamic approach. Of course, this has been presented at international meetings like the American Geological -- or American Geophysical meeting.

MR. LEVENSON: Tim, I have a question in the context of trying to move toward risk-informed.

There's a statement in the report here that "Staff concludes that the character of past YMR igneous activity represents the most conservative bounds on future YMR activity". That most conservative, 1 order of magnitude, 3 orders of magnitude?

MR. McCARTIN: I guess I'm not -- what report is that?

MR. LEVENSON: That's the Center's report on --

CHAIRMAN GARRICK: It was in our briefing book.

MR. LEVENSON: -- our briefing book. I do read the junk you send us. It's Technical Basis for Resolution of Igneous Activity.

MR. McCARTIN: I'd be happy to take that page down and -- I can't --

MR. LEVENSON: I'm really asking a generic question. I get nervous whenever people say, "Well, that's very conservative, so it's okay". I mean, is it a factor of 2, is it a factor of 100?

MR. McCARTIN: Well, partly, what I'm trying to do in going through this is give sort of where things could end up, and I will talk to that, but the approach we're trying to do and part of what this is as a result of, we're looking at the information that we have, what we're using to estimate the consequences, and how we might improve things to get a more realistic approach.

We certainly are not trying in any area of the TPA code to take the most conservative approach. I'm not -- those words trouble me, I guess, because it's not clear on the context --

MR. LEVENSON: It isn't clear from here whether it's the NRC staff that did that analysis or what, you can't tell who did that. This is the staff commenting on it.

MR. SINGH: I'm not sure if that's been issued to -- sounds like it's still being reviewed.

MR. LEVENSON: I'm sorry, go ahead.

(Slide)

MR. McCARTIN: In terms of the magma-waste package interactions, clearly, when a hot magma interacts with a waste package, we're talking about a -- certainly, in the conduit, a fairly violent interaction. Physically, chemically, thermally, these conditions are quite extreme.

So, the question is how does the package act in this environment? In terms of the NRC, for the extrusive amount, the package is in the conduit. We're assuming the package offers no protection from the spent fuel from the magma. And so it's as if there is no package.

MR. HILL: Just to answer the previous question about the character of Yucca Mountain volcanoes conservatively, that's specifically to address that we do not believe that magma water interactions would create a different class of volcano, and that there is no other class of volcano of this composition that would give a more dispersive eruption, which is the process that was discussed in that section. So, the past character would bound the dispersivity and fragmentation capability, and that any other -- trying to say that they're less dispersive would be a less conservative approach.

MR. McCARTIN: In terms of the intrusive amount, we have on average in our code approximately 40 waste packages failing, and just assume, once again, if magma contacts those waste packages, it will fail the container. Part of it is due to the temperatures. The magma is approximately 1100 degrees, et cetera, and the drifts are unbackfilled. This really does not account, at this time, for this alternative flow path. It could be more packages if we looked at that alternative flow path. We have not worried about varying that. We believe that's a very small contribution to the overall risk, and you'll see that actually is a slight difference between ourselves and DOE.

In terms of the DOE approach --

MR. LARSON: Excuse me, Tim. Is that then all taken -- enters into the biosphere through groundwater?

MR. McCARTIN: It has that possibility, yes.

MR. LARSON: But only --

MR. McCARTIN: Only -- yes, it is intrusive, yes, absolutely. For the DOE approach, they have a similar approach for the extrusive. Any waste packages in the conduit don't have any effect of limiting the entrainment of spent fuel in the magma.

In terms of the intrusive, they have a slightly different approach. They have two zones. Zone 1 where the package offers no protection, also similar to ours, and in that zone right now they have approximately 200 waste packages. And then there is a Zone 2 where they have what's called some "end-cap" failures, some moderate failures of the waste package. And they have, on average, 2,000 waste packages there, a significant amount more. And that is a big difference between ourselves and DOE. They have far more -- they get a larger intrusive release than we do. However, even if we increased our number of waste packages, we would not get, I don't believe, get to the numbers that DOE has, and that's something that you'll see in terms of the path.

CHAIRMAN GARRICK: Well, what they gain on their probability, they lose on their consequences.

MR. McCARTIN: That's one way to put it, for intrusive. That's the intrusive -- yes.

(Slide)

And that's part of the risk insight here. We understand that the intrusive is a very low fraction of the extrusive, and so we have not concentrated much effort on that particular part.

Conversely, DOE has the intrusive a much larger fraction. Until recently, the intrusive was the larger dose contributor. In the SR, intrusive was a larger dose than the extrusive. That has flipflopped, but it is still a very high percentage of the extrusive.

CHAIRMAN GARRICK: Now, have you seen the analyses that they performed where they assume these 2,000 waste packages?

MR. McCARTIN: Yes, that's the SSPA.

CHAIRMAN GARRICK: So you're able to pin

it down as to the fundamental difference between the NRC analysis and the DOE analysis?

MR. McCARTIN: Qualitatively, I believe so. I'm not convinced. I mean, that's one of the things, path forward, that this last -- this year, I promise you, we will understand those differences. I think they're -- at a very broad level, I believe there is approaches in terms of the release, in terms of their diffusional release, that when they go to this igneous scenario where they fail the waste package, I believe they give some very, very large diffusional releases, and that's part of it.

There might be some ways that they are using the probability to weight things that we don't quite understand how they are doing it. They have a slightly different approach to weight the consequences with a probability than we do, and there are some other aspects to the calculation. We are digging into it. We don't have the answer. This year, we will. I think part of it is we need to get a little smarter, look a little deeper into the DOE calculations, and ultimately we will end up with probably an Appendix 7 or some other type of meeting with DOE to go over, okay, here's how we understand your representation.

As you know, I've mentioned this before, diffusional -- one simple thing to point to is diffusional. Ourselves, and DOE, for the base case, have approximately the same releases for completely different reasons. We take no credit for cladding, but we have no diffusional releases. We did an estimate and saw that diffusional releases were such a small fraction of the invective releases that we don't have diffusional release.

DOE, on the other hand, takes a lot of credit for cladding, and has a diffusional release that dominates over their invective release, approximately an order of magnitude, and I think we --

CHAIRMAN GARRICK: Particularly for long-term effects.

MR. McCARTIN: Yes. And in the situation for volcanism in the intrusive, I believe for those 200 waste packages they take no credit for cladding and no credit for the waste package, and so they have some very, very large releases. I think that's at the heart of it, I'm not certain, but there's an aspect of taking in and understanding this better that we certainly are in the process of doing. I'm very confident this year we will have an answer to that. To me, from a performance assessment standpoint, that's the most fun this job brings, is trying to uncover a mystery. We have a mystery here we don't quite understand, and technically it will take a little bit of work, but I think that's the fun part.

CHAIRMAN GARRICK: Andy.

MR. CAMPBELL: Tim, are there any examples of magma interaction with features at all? The reason I ask is, if I understand correctly, basically, once a waste package "fails", for all intents and purposes, it is not there.

MR. McCARTIN: Right.

MR. CAMPBELL: And you haven't talked about waste form interactions, but essentially you assume a significant fraction of the waste form becomes a very small particle. Is there anything that can be gathered in terms of interactions with engineer features or wall rock interactions that can constrain these kinds of models?

MR. McCARTIN: Certainly, there's wall rock interaction. On here, we are going to try to look at the literature for analogous kinds of situations, and it's primarily the Center is going to be helping us, try to see are there things that we can draw some parallels. It's not easy, and I don't know if Brit has any other ideas, but we're going to try to look at that in a data-poor situation and try to find some additional information -- I know Dick Codell is trying to look at are there some reactor accidents, like TMI, where maybe there's some information more in the fuel area, can you glean some information in an area where you have very little, but we are going to try to pull in information where possible.

MR. LEVENSON: There is data from Chernobyl, both the U.S. models and the Russian models predicted that the molten core would certainly go through the floor down, and it didn't penetrate the floor anywhere. The molten fuel ran for long distances on the top of floors, and then poured down through existing holes, and all of the models were incorrect in their projections, both U.S. models and Russian models.

MR. HILL: To directly answer your question on engineering analogs, the answer is no. There is no experience of having a basaltic volcano or any volcano erupt directly through an engineered facility. And, remember, we're talking about putting a waste package into an erupting volcano, not on top of a lava flow. So, there is a long-range of physical, thermodynamic process that's involved in this, and exactly modeling what's going to happen as we go from highly reducing environment at temperatures of about 1100° C. under very high dynamic flow, the best I can tell you is that this starts off as a 1 meter wide dike and reams out the wall rock on the order of 10s of meters in diameter, through overpressure-underpressure relationships, so there is sufficient force, sufficient work to essentially at times melt or disaggregate solid rock.

And just a clarification on some of the Chernobyl information, I believe the temperature for the fuel to become molten is about 2000° Centigrade, there's some very high temperature beyond what we have been seeing in igneous events, or well below the solubleness for incorporation in basalt, but we're dealing with, again, a process where at the point of initial incorporation where a highly reducing environment of producing these are very low, on order of 10^{-9} atmospheres, but as we go up into the erupting cloud, we're going -- mixing with the atmosphere very rapidly, at temperatures on the order of several hundred degrees Centigrade. And one analogy we have drawn from the Chernobyl accident is a very rapid oxidation and formation of secondary oxide phase and embrittlement of fuel particles during these complex, rapid geochemical mixing events. We are continuing to work with people like Dick Codell and other folks that have a much better background in waste forms to try to glean some things from these reactor accidents to look at more mechanically how would the fuel aid during an event, but we're dealing with very rapid changes of very high dynamic load on all of the waste packages waste forms.

CHAIRMAN GARRICK: Just in that connection, what do you assume about the deposition of the radionuclide inventory in the magma, is it just instantly available?

MR. McCARTIN: There's an incorporation ratio that picks it up.

MR. HILL: And we're looking at a process of conduit-widening right now where the waste package is heated up for ambient conditions up to about magmatic temperatures, and the internal contents are heated as well, so we're going to have cladding degradation, oxidation --

CHAIRMAN GARRICK: So there is a process.

MR. HILL: It's process-driven, but it's not mechanistic because there are so many uncertainties about how would this feature behave. We faced early on the dilemma of, well, guess, it's not instinct, would it be 100 percent efficient? Probably not, except if I look at holes in the ground from volcanoes, they're 100 percent efficient in lightening. But how would I get a technical basis to say it's 80 percent or 70 percent efficient? How would I defend that?

CHAIRMAN GARRICK: So it's just one of the many abstractions that are involved.

MR. HILL: It's a complex abstraction, that is a simplification, but it is based on the unusual mass and mechanical loads that are inherent in this system, analogous for a reactor loci than any sort of storage type accident.

CHAIRMAN GARRICK: Thank you.

(Slide)

MR. McCARTIN: Going to the fuel, along the similar lines, but certainly once again the chemical/thermal aspects of the magma are going to be a harsh environment for the fuel. Right now, NRC has a fairly simplified approach -- it's an incorporation ration in that particles -- in simple form, particles that are larger than the spent fuel particles can incorporate that smaller particle.

DOE has adopted a similar approach.

MR. LARSON: Does that take into account the density of the -- the high density of the fuel?

MR. McCARTIN: Yeah.

(Slide)

In terms of the risk insight, there's a lot of uncertainty here, as Brit was describing, exactly how is this fairly violent eruption, as its pushing through the conduit, interacts with the fuel. Certainly, you can do some further refinement, but our gut feeling is that the refinement would not result in a significant change. Right now, 100 percent of the fuel is incorporated. Is it 80 percent? Is it 50 percent? You might have to do a lot of work to get a little more resolution on that, and we're not convinced you could get it down very low, but that's a gut feeling. Certainly, our path forward this year, we're looking at refining our source-term model in that sense. Once again, as was mentioned, we're going to try to evaluate the relevant reactor accidents possibly. Maybe there's some information there that will help us.

And the bottom line is, DOE does need to develop a technical basis for this. This is one area where I know they have adopted our model. Merely adopting the NRC model is not a technical basis. Just because it's our model doesn't mean it's necessarily supportable. The DOE needs to do some work to at least convince themselves that it is a reasonable model, not just because it's the NRC's.

MR. LARSON: Are you evaluating more complex repositories that simply are a tube? Are you assuming any distortion of the tube? Any rock falls, and backfill?

MR. McCARTIN: In terms of the alternative pathways?

MR. LARSON: Right.

MR. McCARTIN: Yes. That was part of the repository design, you look at backfill. But we also are looking at, over time, you'll have rockfall and accumulation of material in the drifts.

MR. LARSON: If I understand the process here, what you would end up with is the canisters that are being opened up being pushed to some portion of the drift. Are you evaluating the effect of that, of a concentration of the fuel on the thermal aspects of the surrounding rock? You know, if we're going after a low temperature repository, we'd push all of the magma -- or all of the fuel into one end, we've changed that process.

MR. McCARTIN: Yes. The initial calculations were assuming empty drifts, and subsequent ones are looking to refine things -- and Brit can talk to the later work.

MR. HILL: Right. The initial scoping report back in 1999 talked about the possibility of the initial shock being sufficient to move waste packages. In the Woods, et. al. paper, we've refined that approach with a little more added, and do not believe that you have either enough friction or velocity to move a waste package.

MR. LARSON: So, a lot of simplifying assumptions.

MR. HILL: We're not seeing things like moving down the drift, the waste package remains intact even under the range of flow conditions in the Woods, et. al. report. So that was an initial model that turned out to be --

MR. LARSON: So there will be no concentration of the fuel in any portion of the drifts?

MR. HILL: Not during the initial stages of flow because you cannot move an intact cold waste package under the conditions that we currently realize as reasonably bounding the expected upper range of flow during that initial impact.

CHAIRMAN GARRICK: You can't have it both ways.

MR. HILL: Another thing is, we haven't

looked segregated flow during sustained eruption. After the waste package is disaggregated, it's possible you could get a concentration zone between a low velocity and high velocity horizontal flow, but I don't think that's likely given the turbulence inherent in the system. However, we haven't analyzed it, but plan to analyze it in the coming year.

MR. LARSON: Thank you.

(Slide)

MR. McCARTIN: Well, we finally got out of the repository now. We have an ash deposit on the ground, and the question is one of with this ash deposit at the RMEI location, how is it going to evolve with time? There are really processes that could remove material. There's processes that can bring it in from, say, flooding from the repository, 100-year flood comes in and washes some of the ash from near the mountain down to the RMEI location.

Right now, NRC has a simplified approach. I have "conservative" with a question mark. I think the gut reaction is that this is a conservative approach. However, having criticized DOE for doing the same thing, what is your basis for saying it's conservative? And I think we have to do more work to understand that. It may very well be.

Right now, we're having the ash plume blow in the direction of the critical --

MR. LEVENSON: Excuse me a minute, Tim. Before you get to the ash moving, what was the approach in deciding what the composition of the ash was, the ratio of plution or fission products or what have you, to ash. Is this concentration of mass involved here?

MR. McCARTIN: Well, there's the volume of the mass of the --

MR. LEVENSON: You've got a limited number of containers that have failed, and a huge amount of ash.

MR. McCARTIN: We're assuming a uniform mixing. At the RMEI location, the material that's put down is uniform within the ash.

MR. LEVENSON: And the difference in density of a factor of 10 doesn't give you any segregation at all.

MR. McCARTIN: Right now, we're not accounting for those kinds of things.

MR. HILL: Please recall that it's being incorporated into molten material, not adhering on a solid.

MR. CODELL: This is Dick Codell, NRC. We are working on an alternative model of the fuel ash incorporation where we do take density into account, and it may supersede the incorporation ratio model. In this case, the fact that the fuel is much denser than the ash could lead to more small dense particles, which may behave differently once they are thrust out into the atmosphere, although we don't really have an adequate model for dealing with that atmospheric transport.

MR. HILL: We did do a very quick scope that even if we fail 10 waste packages, there still -- with a small volume tetra eruption, you're dealing with .01 weight percent of high level waste in the total amount of the eruption. So this is really a trace component in the total mass core volume of the eruption.

MR. LARSON: But it would seem reasonable that you could get some stratification in terms of the distribution of the fuel contained tetra. It just seems logical that there would be a stratification with the density --

MR. LEVENSON: Especially at the low flow rates. At the relatively low flow rates, there's not turbulent mixing as this goes down the drift.

MR. CODELL: Well, the true is, what's

likely to happen, though, is that the biggest ash particles will take the biggest fuel particles with them, simply because there's more of the bigger ash particles. There's more mass in the bigger ash particles, so it's not going to change density of the mixture that much. My best guess is it's a relatively small effect.

MR. LARSON: But calculations are being made on this?

MR. CODELL: Yes.

MR. LEVENSON: Having spent a number of years trying to mix things to get uniformity, and knowing how difficult it is, I have trouble assuming this kind of a thing. I mean, the flow down the tubes is not fast enough to push the containers, and yet you're assuming complete mixing of everything which is coming long after the containers have failed. It doesn't sound like a good assumption to me.

MR. HILL: Just to follow up with that, we're considering that we're dealing with a very complex gas-fluid mixture that's taking a very irregular geometry. Even though we talk about horizontal flow, it's coming up from depth vertically as about 50 percent fragmented -- 50 percent gas volume, 50 percent magma -- both horizontally and then breaks and goes vertically again along something that starts off as a 5 meter in diameter tube, but we have every reason to believe that as the wall rock is stressed, the over- and underpressure will be plucked and scoured the way any other volcanic conduit is.

So, while we may develop a quasi-angular flow regime, there's going to be backpressure within the system and there's going to be angular collapse if it has any reasonable analogy to a volcanic conduit. Both will produce an incredible amount of churning and turbulence within the deposit. So, it may not be uniformly turbulent with continuous incorporation, but for the duration of the eruption, something on the order of three weeks, we'd be having repeated overturn and convection within the system. It would be hard to say that you would keep segregation throughout the duration of an eruption.

CHAIRMAN GARRICK: Does the scouring and the other phenomena that's taking place have a significant effect on the density?

MR. HILL: There may be very transient effects due to gas resorption during slight overpressuring, but that would quickly be -- it would only be in the change in the bulk.

MR. LEVENSON: Because the wall is almost the same density as the magma. It's the fuel that's a factor of 10.

MR. McCARTIN: In addition to having the wind blow south to the RMEI location, the question is how long does that deposit persist at the RMEI location? And right now we have a half-life of approximately 1,000 years for the persistence of that deposit. That's primarily based on the fact that in looking at analogous deposits, that they seem to persist in an area for around 10,000 years.

CHAIRMAN GARRICK: What do the wind rows look like at Yucca Mountain?

MR. McCARTIN: It's quite varied, and it's approximately, I'll say, on the order of 30 percent of the time due south. Now, the other part of this is that we don't account for any movement in to the dislocation from other parts, to the flooding, et cetera. There's no remobilization. We are merely subtracting. It is a simple approach. Like I said, we're actually going to try and do a fair amount of work this year to get a better sense of the reasonableness of this approach.

DOE has partly a similar approach. They are having the wind blow due south also, however, in terms of how long does the deposit persist, I estimate a half-life of around 50 years, so it doesn't persist nearly as long. And I'm trying to understand a little better -- they do not use the half-life concept, they have a different kind of approach, but between the erosion rates they use how thick the ash deposits are, I'm estimating that -- I think I'm close, but I may be off on that -- but you can see significantly different half-life than the NRC. At the NRC, we're looking for a much longer lived.

CHAIRMAN GARRICK: As I recall, you used the same erosion rate for longer periods of time as you did for initial periods, did you not?

MR. McCARTIN: Yes. I mean, it's a constant.

(Slide)

In terms of what this might mean, I think in terms of the redistribution, I think increasing the removal rate, if we went to a shorter half-life for the deposit, I think there could be possibly an order of magnitude difference. I'm not convinced of that, but it could be between the DOE and the NRC approach.

There are a lot of other effects that need to be considered in that. In terms of path forward, I think next year we're going to try to evaluate, if we take into account the local wind effects. As you were saying -- Brit was indicating this could be last three weeks, a month. How does this deposition look like with a varied wind pattern? I think it would be useful to look at that. Knowing, of course, what you get in one area, you take away. Then if you spread it out more, you have to be a little more careful about the redistribution over time. And that's why we want to understand, I think, better -- the risk significance this year is, how much does this redistribution really matter, and do some, obviously, sets giving analyses with the TPA code and see exactly what some of those assumptions mean. There's a lot of -- one of the key things you want to be very careful with in any analysis, and especially the TPA code where there is a host of interconnecting things, getting much better on one aspect like, say, the wind, and neglecting these other things, the confounding sensitivities that you might create by getting very sophisticated in one area and not doing it in another area, and that's a very complex problem that's part of this analysis. Certainly, I can get better in one area, but in terms of the overall analysis, what have you now done.

CHAIRMAN GARRICK: Sometimes this is referred to as the "lamppost" syndrome.

MR. McCARTIN: Absolutely, yes.

MR. LARSON: At what depth do you no longer consider the radioactivity? You get an accumulation. What is the maximum depth of consideration?

MR. McCARTIN: Well, we only look at the top 3 millimeters in terms of what would be possible to be in the mass load, in the particulates. Now, we keep track of the entire blanket in terms of -- or deposit for how long it persists there, but in terms of what's available for an inhalable dose, it is, I believe, is the top 3 millimeters. It's some small amount.

MR. LARSON: Taking into account the porosity of the tetra amount?

MR. McCARTIN: Yes. Now, DOE has a slightly different approach. They assume all the radionuclides are in the top centimeter. We do not do such a thing. That's where we start comparing that very carefully. We're looking at the -- it's an average -- it's uniformly mixed through the entire deposit. DOE takes the radionuclides, as far as I understand it, and puts them all in the top centimeter. So, there are differences. There's a lot of subtle differences. I tried to get on the ones that -- for now, we'll be doing some analyses in the future.

Here's another area where we think we can improve the understanding of erosion processes and things. Certainly, some surface water hydrologists at the NRC -- Ted Johnson has been out to Yucca Mountain to help us better understand how things might change with major floods, et cetera. And so we're hoping to do some more work along that line. And certainly with respect to redistribution, there certainly are analogs for movement of even ash deposits. I know Brit and some of this Center colleagues have been to Sierra Negro where there's an ash deposit, et cetera. So there is some information that hopefully we can continue to --

MR. LEVENSON: Tim, before you leave that, remobilization, in this same infamous report, it says "The high level waste contaminated tetra fall deposit will be modified by wind and water for many years after the eruption can be transported away into the critical group by wind and water following most future eruptions". Are you analyzing multiple eruptions?

MR. McCARTIN: No. Once again, I'd have to read the whole page. I recognize the one sentence. We certainly only analyze a single event. I don't know -- Brit, do you know?

MR. HILL: I was the author of the report, I think I can clarify that. You consider a variable wind rows for any future eruption most of the time, unless you had a wind directed blowing towards the northwest sector at an extremely high wind speed, something on order of 10s of meters per second, you would have an appreciable amount of tetra falling on east-facing slopes that drain into the 40-mile wash drainage system. So, for most eruption scenarios, you would have, even if the plume is directed away from, at that time, the critical group location, you would still have tetra that would fall on slopes that would feed into 40-mile wash and the potential grade leading down to the critical group location. It's very difficult to have an eruption for a probability at the proposed repository site, and not have material eventually end up in 40-mile wash, even if the plume is 180 degrees from it.

MR. LEVENSON: Well, that I understand, but that's not what this says. This talks about future eruptions many years later.

(Simultaneous discussion.)

MR. HILL: I know we haven't appreciated --

MR. LEVENSON: Maybe it's just the words that are here. I'm just reading what's here.

MR. LARSON: Is DOE conducting similar studies?

MR. McCARTIN: Certainly, they have evaluated the redistribution and the erosion at the location of the RMEI. They right now are -- I don't know in terms of analogs off the top of my head, I know they are using USDA numbers for similar kind of areas to get a general erosion rate.

MR. LARSON: There was some talk about using Sunset Crater at one point.

(Simultaneous discussion.)

MR. McCARTIN: Could be, I'm not familiar. But it is an aspect of the calculation that certainly we think has some importance.

(Slide)

Finally, you end up getting a dose, and the inhalation scenario generally is related to how much dust or ash mass is in the air. Estimating the mass loading as uncertainties, there are assumptions about outdoor activities, et cetera.

For the NRC approach, once again, we have -- and I tried to put these in similar terms between ourselves and DOE for ease of comparison. We don't use an average over 10 years, but DOE's numbers were presented in that way, and so it's slightly easier.

As you can see, we have approximately on average about 1.5 to 2mg per cubic meter. How did we get that number? There are three components to it. The first one is a high disturbance, and that's looking at activity such as farming and plowing where a lot of dust is raised, possibly traffic on roads, et cetera, and there's a certain exposure time to that. We have approximately 1 percent of the time in this high disturbance type of activity.

Next, a lower value of mass loading for general outdoor activity, being outside, walking, other types of things. That exposure time is around 20 percent, and then sort of a background level that is at around -- add the two, it will be around 79 percent to get to the 100 percent.

Generally, the dose is dominated by the first two. Those two contributing about the same. You can see right now you have about an order of magnitude higher mass loading for this value, but the exposure time is about an order of magnitude less. So, between the two, the overall dose is really dominated by those two.

In terms of, well, how long does it stay dusty? What we do, we have a background mass loading that is significantly smaller, and we have a half-life of 10 years, and so this higher mass loading decays into the background with a half-life of about 70 years. So, at approximately 70 years, it's going to be pretty much at that background, but it gradually decays. And that is an assumption. Some of that is based on analog information at Sera Negro. That deposit has been there for a while. How long will this ash stay there in a fairly --

CHAIRMAN GARRICK: So what are some of these conditional dose rates?

MR. McCARTIN: It depends on when -- well, it's dependent on many things. Probably the two biggest variables to give you would be -- one would be the time that the dose occurs, primarily because of decay of some of the key -- the short lived nuclides that you don't see in the groundwater pathway, but if you have an event at, say, year 100, it's certainly more prevalent there rather than at 500 years.

And then how many waste packages do you assume are entrained -- and it's always dangerous to go off the top of your head, however, I will try to give you my best estimate. I believe that for our base case, it's on the order of 10-to-100 rems at 100 meters. So that would be the worst event, and that's using these assumptions and everything else.

MR. HILL: This is in the IRSR Rev 2, and Tim is correct, at about 100 years it's on order of an average of 100 rems. At 1,000 years, it's on average of order 10 rems, and by 10,000 years it's on average of 1 rem as the conditional dose in that given year. But it is in IRSR Rev 2.

CHAIRMAN GARRICK: Of course, if you were talking about a 100-year compliance period, you'd be talking about a substantially different probability. So, the weighted risk is still the way you have to look at it. But I was curious. Thank you. I wanted to know what those numbers were.

MR. McCARTIN: There's been a lot of evolution of this calculation, and even in the IRSR Rev 2, we have changed -- some of the mass loadings have reduced since then, and so there's a lot of thinking going on, but certainly we're in the right ballpark.

MR. HILL: I don't think we've had order of magnitude changes, but again an order of --

CHAIRMAN GARRICK: Well, I think the way you've stated it in terms of mg/m^3 is the way to start this process.

MR. LEVENSON: Do you have an estimate for how much solid material is deposited in the lungs if you're breathing 4.5 to 9 mg/m^3? Do you die of the dirt in your lungs?

MR. McCARTIN: No, no, it's not that high. And be aware -- and this is another aspect that we're looking at with respect to -- I mean, it's -- and be aware that that's for 1 percent of the year at that high number, so --

MR. LEVENSON: No, no, I was looking at the 4.5 to 9, which is 20 percent.

MR. HILL: That's total suspended particulate, so an appreciable portion of that doesn't go into the lungs.

MR. McCARTIN: That's what I was trying to get to, is the -- one of the things we want to look at -- I know DOE accounts for a much lower value than we would for what gets ingested, but some of this is larger particles that get into the nose and ultimately get ingested.

My understanding, in talking to our health physicist, is that doesn't cause -- it still causes a fairly significant dose, not as low as the DOE is estimating. I'm not sure why those differences are there, that's another aspect that we want to look at.

I don't know who has the more reasonable approach, but we're looking at -- in my mind, I thought -- we looked at particles that were inhaled through the nose and ultimately ingested, not going into the deep lung, caused about a factor of 2 lower dose than what was inhaled into the lung. And so it isn't quite as important.

For DOE, I thought I remember reading something that was 3 orders of magnitude less, and I'm not quite sure why that difference is there, and that's something that I want to --

MR. LEVENSON: Well, that would be a big function of time because if you are a couple of thousand years down the road and most of what's left is plutonium, it pretty much goes through your gut, it's not absorbed at all, whereas in your lung you get the dose. So that ratio would be a big swing with time.

MR. McCARTIN: Yeah, and that's -- once again, it's another thing we're looking at, but in terms of deep ingestion into the lung, we're not looking at. It's not causing a significant health problem just because of the dust itself.

CHAIRMAN GARRICK: Well, thanks to us, we've extended this a little, so maybe we'd better try to wrap up this before we freeze to death.

Who controls the thermostat in this room? Is there a reason that door is open?

VOICE: Yes, sir.

MR. LARSON: To let the heat in.

CHAIRMAN GARRICK: Oh, to let the heat in. Oh, okay.

MR. BAHADUR: It is by design so that the meeting will be really short.

(Laughter.)

CHAIRMAN GARRICK: It didn't work. Okay, Tim.

MR. McCARTIN: I will try to go quickly. The DOE approach is -- we're not that different, but you can see we got to these numbers quite a big differently. DOE has an outdoor value that they have 32 to 45 percent of the time, and an indoor value for the remainder. However, they have a mass loading that applies for 10 years. After 10 years, they're at the background level, and so it drops off quite a bit quicker than ours.

(Slide)

In terms of the risk insight, if I look at the differences between our two approaches and be aware that there still is a fairly fluid situation, in my mind, because of the different approaches, also the part that DOE puts all the radionuclides in the top centimeter, there's a lot of little things there. But I think, I'll say around a factor of 5 difference in risk.

What are we going to do? we're going to start -- continue to look at analog systems for what is the right mass loading for different situations. As I mentioned, Brit, they've done some measurements at Sera Negro that can be used.

What's the evolution of the ash particle over time? As was indicated, once you get up in the air is a big part of this calculation, when this ash is deposited. How much of this and how does it change the particle size, that also is an important part. See, there's a lot of subtleties here that need to be considered.

And the last one that we're looking at is there has to be a correlation for how long the deposit persists and how dusty it is, and there certainly should be some -- if there's a lot of it in the air, the wind is blowing, it shouldn't last very long, and we'll be looking at that aspect of it for reasonableness, how long the deposit continues, and that's where the remobilization and the inhalation --

this is one where there's just a lot of interconnection between what assumptions are being made.

And the next two slides is really -- and I'll try to go through them very quickly, and I think I can --

CHAIRMAN GARRICK: They are very good, and they are quite self-explanatory.

(Slide)

MR. McCARTIN: Right. And what this -- this is sort of a preview for the future, and what we're trying to work on is a simple explanation of the approaches in the TPA code, where the uncertainties, how we got to where we are. I think we would like to do this for both our own code, but also for the DOEs, possibly a way to provide a very simple, quick way of explaining, understanding of the Repository. And rather than going through the igneous slide, which really is in some areas pretty much a summary of what you just heard, I will skip past that and go --

(Slide)

-- one of the things we're hoping to learn from this exercise is that, indeed, as Marty indicated, we're treating uncertainty in a similar fashion. That may not be the case everywhere, but I think it is important to understand how we're dealing with uncertainty. And this may be -- I said earlier, when we're a little further along with this for the entire TPA code, it may be useful to come back to the committee and go through this and get feedback. There's a lot of different areas of the TPA code.

For example, what really matters for waste package? Well, if you have a waste package that you think is going to last past 10,000 years, initial defective packages are quite important. the question is, the uncertainty in estimating that is the manufacturing process, the closure, weld defects, are things you have to consider. Right now, in the TPA code we have -- we're going from 0.01 percent to a 1 percent failure based primarily looking at some analog manufacturing information.

CHAIRMAN GARRICK: Boy, that's a big jump.

MR. McCARTIN: Yes. Well, on average, 0.01 percent.

In terms of the corrosion, as we indicated, what are the big uncertainties there, and certainly the hydro-chemical/thermal environment is the key there, and certainly we have the short-term measurements, and the effect of the welding, and the post-welding, et cetera, what's the going to do to the lifetime.

Our current approach is that the corrosion rate is based on the relative humidity, however, how do we account for more aggressive water chemistries? We have a chloride multiplication factor that increases the chloride content and thereby affects the waste package failure time. Right now, failures, as indicated here for uniform passive corrosion, it's on the order of 10,000 to 45,000 years.

Dripping on the waste package. Where will packages get wet? You have possibilities in the hydrologic environment for focusing flow. You also have diversion, capillary diversion of water. And so you've got a couple of processes there. In terms of the fraction of waste packages that get wet, we have a very broad range there. It's all of them or none of them. We sample between the two. There is a lot of uncertainty three, and you can see we've taken the full range.

However, within that, there is -- I'll maintain it's Dick Codell's finest moment at NRC. He may not think so, but in terms of how do you try to represent this dripping aspect, and he came up with some factors that account for how much water is dripping onto the waste package, and it's correlated.

Although we do sample between zero and 1, when you get close to 1 where all the packages are being dripped on, it's a little bit of water. Clearly, you have drips everywhere. You have a finite amount of infiltration. The amount of water than can drip on a package is small.

On the other end of the spectrum, when you get to very few waste packages, you have some focusing of flow, so you get a lot of potential for more water dripping on the containers because you have very few getting wet, but obviously where it's flowing there has to be more water to account for -- and I'll turn to Bill's infamous conservation of mass. We've got to account for the water. You can't have a lot of water on a lot of packages. It's a lot of water on a few packages or a little bit of water on a lot of packages, and that really is what that tries to account for.

Right now, how does that vary? We go from basically 1 percent of the infiltration rate when a lot of them are getting wet, to 3 times when very few packages get wet. So, you can see the range of the --

MR. LEVENSON: Three times as much water reaches the package and comes in?

MR. McCARTIN: No, 3 times the

infiltration rate.

MR. LEVENSON: It would be more than filtration rate.

MR. McCARTIN: Not in volume, it's focus.

MR. LEVENSON: Oh.

MR. McCARTIN: The infiltration rate is --

CHAIRMAN GARRICK: Funneled.

MR. McCARTIN: Right. In terms of the drip shield, right now in our code, we have no mechanistic model for its failure. We specify a failure of time, which is approximately 5,000 years.

CHAIRMAN GARRICK: That's quite a bit shorter than TSPA/SR at least.

MR. McCARTIN: Yeah, I think so.

CHAIRMAN GARRICK: It was about 20,000 years, as I recall.

MR. McCARTIN: But you can see, for us -- once again, getting back to what causes corrosion, it's the humidity. So, whether we have drips or not really doesn't affect the corrosion rate as much, but in terms of can spent fuel, if there is an initial defect, can water get in to mobilize the waste, the answer would be no. And as I indicated, we do not have a diffusive release, we only have an invective release, so you do have to have dripping water. We sort of felt without dripping water, you weren't going to get anything out of the waste package. DOE would have releases from drips.

CHAIRMAN GARRICK: So the drip shield serves your model.

MR. McCARTIN: Yes.

CHAIRMAN GARRICK: And essentially does not serve the DOE model.

MR. McCARTIN: No. I mean, if they had a defective canister underneath the drip shield, even if the drip shield was intact, the way I understand it, they would still have releases.

CHAIRMAN GARRICK: Well, they are based on diffusivity transport.

MR. McCARTIN: Yes. Spent fuel cladding, as I indicated, right now there is the unzipping of the clad. We think it's fairly uncertain what kind of zipping would occur over hundreds of years, and just thermally you're not going to see dripping for quite a while, so you're looking at this not really as put into the repository, but quite a few hundred years afterwards. We right now take no credit for --

CHAIRMAN GARRICK: And that's kind of evidence supported. There are thousands of assembly years of experience with no such phenomena observed.

MR. McCARTIN: And then the final one is diffusion of the radionuclides from the waste package. There clearly is a very complex chemistry inside the waste package, however, when we've done our analyses, when you have dripping into the waste package, the invection will dominate.

We have not included diffusion when -- you get a hole and all of a sudden things start diffusing out. we just don't include that, but we're aware of it. We may do some additional analyses along these lines because of the DOE model. At one time, be aware that we did have it in our TPA code. We removed it. It cost a lot of computer time and produced such a small release, we said why bother. We may add it back in, and we'll look at it, but --

CHAIRMAN GARRICK: This was the bathtub model.

MR. McCARTIN: No. I mean -- well, we saw the bathtub model, but a diffusional release out of the waste package at one time we had. We removed it. We were thinking about putting it back in due to the DOE model, but I think the important thing is we recognize the differences. DOE appropriately shows us what the diffusional release is, the invection release, so we can understand the differences, but it's an interesting aspect between the two.

With that, like I said, we hope to do that for all the codes, and this was really a very quick cut. We've just begun it. We hope to improve the detail and information on there.

(Slide)

With that, let me wrap up very quickly. What we're trying to do is improve our understanding in areas important to representing the consequences from magmatic events and --

MR. LARSON: Time, as a result of that improved understanding, have you come up with any FEPS that the DOE has disregarded that should not be disregarded? Is this being looked at?

MR. McCARTIN: Well, we certainly look at the FEPs. In a general sense, I'd say no. They certainly are looking at the alternative flow paths as another -- although, there's a part -- is that a different FEP, or is it part of igneous activity?

And I'm really not aware of any significant FEPS that we've seen. And how things will change, if I had to give my gut reaction as we go through all this work, I think it's possible one could see a 1 to 2 order of magnitude change. Time will tell. Like I said, there's a fair amount of work we hope to accomplish this year.

I'll make the offer -- none of my management are here, so I can speak freely. I think it would be fair, as this work improves, we can come back and report on this, and certainly I think Brit and John Trapp, as the aeromag data comes in, that clearly mobility is not my strength by any means in terms of how those might change. It might make sense when that is, as that's analyzed, to come back also.

CHAIRMAN GARRICK: A couple of closing questions. How many agreements are outstanding on this KTI?

MR. HILL: We had 22 to begin with. I believe 6 are completed -- 7 are completed -- close enough.

CHAIRMAN GARRICK: If all of this goes in the wrong direction, are we heading in a direction that could make this a possible problem?

MR. McCARTIN: Well, I guess my impression of the estimates are I find it very hard to believe that these things at all go in one direction. I think that is a very -- although there's no guarantees in life, but I think it would be very, very odd that they would all go in one direction or the other. And so my personal take is that I think we're probably closer to maybe it stays the same and maybe it drops an order of magnitude.

MR. LARSON: So your estimate there is not net, but individual estimates?

MR. McCARTIN: No, it's net. If I had to be pinned down, and that's what this is trying to do, I'd say 1 to 2 orders of magnitude, and I think that's the net change. Which direction is it? I think most likely it's the lower, my gut reaction at this time. There are things that could make it higher. You saw a combination of both. Probability could increase. Number of waste packages might increase. I think mass loading probably will be reduced. So, where you end up I don't quite know.

CHAIRMAN GARRICK: Andy, are they wanting anything from us on this, or is this informational?

MR. CAMPBELL: Well, this is primarily informational preparation for future ACW reports to the Commission, but if the committee feels that it would like to weigh in on this issue, this is probably the time to do that because this is a particular KTI that's been talked about.

MR. LEVENSON: Can I ask one more question out of ignorance, which is where most of mine come from. Why is Mount St. Helen's not referred to as anything that might be an analog, is it so different? Probably better measurements on that than most.

MR. McCARTIN: Well, in terms of -- Brit probably can answer this better than I, but I'll put my two cents in first and then go to Brit.

I do know DOE, in their reports, they certainly cite Mount St. Helen's as the basis for their ten-year -- after ten years reducing the mass loading to the background. And so they are looking at some of that information. There are aspects of the eruptiveness that are -- in terms of the mass loading, there are some limitations of Mount St. Helen's.

If you go near there in terms of -- there's a lot of education, different soil types than Yucca Mountain, et cetera, that you have to take into account when you look at if the ash can be easily incorporated into the soil there versus possibly at Yucca Mountain soils, and that's something that has to be considered. But I think we try to look at everything that's analogous, and I'll ask Brit if he has some other thoughts.

MR. HILL: Just a couple of very quick, off-the-top points. First, the grain size character of the Mount St. Helen ash, especially in the areas where the occupational studies were done for particle concentration. It's fundamentally very different from what we see from basaltic ash.

Second, it is highly dedicated, well established soil that receives a lot more rainfall than we see from the Yucca Mountain sorts of scenarios. So the granulometry is different, the depositional area is different. The volume away from about the initial deposit itself is fairly small and very thin deposits. On slopes and terrain it really was very stable to begin with, and also very permeable to begin with, that allow a lot of infiltration to occur.

So, while we can gain some general insights from the Mount St. Helen's information, trying to understand what's happening between zero and 20 kilometers away from the Yucca Mountain system has some fairly significant limitations.

MR. LEVENSON: We're assuming no climate changes for these 10,000 years, right?

MR. HILL: We're looking, though, at these peak risks from volcanic disruption occurring on the order of the first thousand years. So, while climate change would affect the 10,000 year view for the period that we're most concerned about, we're not too worried about climate change. There's at least no evidence to support significant climate change in the first thousand years.

MR. LEVENSON: And that affects the probability by a factor of 10.

MR. HILL: The probability is the same at any given year. There's really no -- with the homogenous fall recurring rate, there's no effective difference between the probability in year 100 and the probability in year 10,000.

MR. McCARTIN: When we do our calculations, we're looking at year one, the probability. We're not, say, taking the event, multiplying by 10,000 years, and getting a single probability to then say -- and I don't know, maybe Dr. -- this may not be as clear. We don't say take 10^{-7} times 10,000 so the probability of that event --

CHAIRMAN GARRICK: No, I understand.

MR. McCARTIN: -- okay -- at year one, it's 10^{-7}.

CHAIRMAN GARRICK: Right. Okay. Any other questions from any of the members, consultants, staff? Yes?

MR. HAMDAN: Tim, just one question. I'm not clear on the one aspect of this. Are we saying that every igneous activity will have an intrusive component and risk, and extrusive also?

MR. McCARTIN: Yes.

MR. HAMDAN: So, how are you deciding how much of the event is extrusive and how much is intrusive? Do you assign probabilities, or how do you go about doing that?

MR. McCARTIN: For us, right now, we merely -- we have -- we specified a number of waste packages that will be affected by intrusion. We have not worried that much about assigning a specific probability, we just -- we can allow it to occur at the same time we do the eruptive one, but it tends to be -- relative to the eruptive one, it's a very small risk value.

MR. HAMDAN: So, how about an intrusive event -- how about an igneous activity when that does not include eruption?

MR. McCARTIN: It could be a factor of 10 or higher in terms of probability.

MR. HAMDAN: Is that modeled --

MR. McCARTIN: We can. I mean, right now the intrusive event is for us a very small amount. DOE obviously has a larger one and, like I said, we'll be looking at those differences. It's quite possible we have it wrong. If it comes up in consequence, then maybe we need to look at it a little more carefully. But, right now, we see it dominated by the eruptive.

CHAIRMAN GARRICK: Andy?

MR. CAMPBELL: I was just going to ask if there was anything to be gained in terms of ash distributions from looking at similar type of basaltic cones, signatory cones, in the basin and range. You mentioned Sunset Crater. Is there anything to be gained from studying the volcanoes that are there at Yucca Mountain in Crater Flats? Are they so old that anything that you can interpolate to 10,000 years has long since gone?

MR. HILL: Right. The youngest volcano is 80,000 years old, and almost all the ash deposit is gone, there's just a fragment here and there. In terms of the basin and range, the Sunset Crater is about 1,000 years old. It's much larger volume. But, again, one of the two volcanoes of similar type that still has a preserved deposit, we're not using it as a direct analogy, we're trying to look at how tetra would behave, but not how the eruption would progress.

That's why we've been using the historical analogs, because we have nothing in the basin and range that's historical, and most of the deposits are before the highly eroded condition, so we look at much younger deposits on the order of 50 to erupting while we watch, so that we can better understand the dispersive processes.

But looking at places like in Mexico or Sunset Crater and then comparing those to the older volcanoes in the base and range, that's where we came up with the scaling for at 1,000 years there seems to be some deposit left, at 10,000 years it seems to be pretty well gone. So, the 1,000 year half-life that we're using is really a scale of 100 year, 1,000 year, 10,000 years. You see it at 1,000, we don't see it at 10-, and clearly it's there at 100. So, 10,000 year it's all gone, given a half-life of 1,000, that seems to be the best first pass.

MR. CAMPBELL: One other question for Tim, on these tables that you developed at the end of your presentation, is there any thought to developing a column to identify built-in conservatisms, and this is an issue the committee has been dealing with, and kind of raised it in the context of TSPA/SR of identifying conservatism. Has the staff thought about it, you guys thought about, you know, looking at how conservatisms are being treated and carried through the model? Is that part of all this?

MR. McCARTIN: We certainly will look into that. I'm always trying to think of a better word rather than "conservatism". I don't think there is one, unfortunately. The only reason I say that, we need to get to that, but part of it is the information you have, and some of it is you're making some assumptions and, as Dr. Garrick said, assumption base versus evidence, so often --

CHAIRMAN GARRICK: What might be better is a cryptic identification of the relevant evidence.

MR. McCARTIN: Yes. Absolutely.

CHAIRMAN GARRICK: Supporting evidence.

MR. McCARTIN: And that is one there that here's what the evidence tells us, and let someone draw their own view of whether it's conservative or not.

CHAIRMAN GARRICK: Any other questions from anybody?

(No response.)

Okay. I want to thank Brit and Tim for a very informative hour presentation that went a couple of hours, and we look forward to following up with all the path forwards that you identify, and we also want to thank our consultant for his significant contribution in this session, and we will now take a recess and when we come back -- this terminates all of our briefings and discussions of this type.

We will now go into our usual letter discussion and letter-writing session, for which we will not need reporting, and until then we will take a 15-minute break.

(Whereupon, at 3:40 p.m., the briefing session was concluded.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: ACNW Estimating Performance of Igneous Activity

Docket Number: (Not Applicable)

Location: 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.

Phyllis Young

Phyllis Young
Official Reporter
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