UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

January 11, 1996
Holiday Inn Crowne Plaza
4255 South Paradise Road
Las Vegas, Nevada

BOARD MEMBERS PRESENT:

Dr. John E. Cantlon, Chairman
Dr. Clarence R. Allen, Session Chair, Afternoon Session
Dr. Garry D. Brewer
Dr. Edward J. Cording
Dr. Donald Langmuir
Dr. John J. McKetta
Dr. Jared L. Cohon, Session Chair, Morning Session
Dr. John W. Arendt
Dr. Jeffrey J. Wong

CONSULTANTS:

Dr. Patrick A. Domenico
Dr. Ellis D. Verink
Richard Parizek

SPECIAL GUESTS:

Dr. Ju Wang, Vice Director,
Beijing Research Institute of Geology
Chinese Nuclear Corporation
Michael Folger, Managing Director
UK NIREX, Ltd., Great Britain
Sir Richard Morris, Chairman,
UK NIREX, Ltd., Great Britain
Mark Hammond, British Embassy, Washington, D.C.

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I N D E X

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P R O C E E D I N G S

DR. COHON: One more time. Good morning. My name is Jared Cohon. I'm a member of the Nuclear Waste Technical Review Board and Chair of this morning's session on expert judgement. And let me just get something out of the way right away. There will be no jokes about experts this morning, since you're almost all from out of town.

The Board has been interested in expert judgment since its inception and has expressed that interest and raised some concerns about the use of expert judgment in many of its reports.

We recognize that many critical issues cannot be solved by data collection alone. Inherent uncertainties associated with the geologic system and predicting performance for many thousands of years require the substantial input of expert judgment.

Expert judgment can be defined as an inference or an evaluation based on an assessment of data assumptions, criteria and models by one or more experts in their field. More often than not, expert judgment is applied informally and in a non-explicit manner.

The Board's focus, however, has been on the use of

explicit formally developed expert judgment by the DOE and its contractors in programmatic studies. Examples of this include the Calico Hills Risk Benefit Analysis and performance assessment, such as in TSPA-93.

The Board's concerns about the DOE's use of expert judgment have centered on methodology, the need to include experts outside of the DOE and its contractors and the need to resolve possible conflicts with the NRC in the use of expert judgment prior to the beginning of the licensing process.

The last issue has also been a great interest to the NRC's advisory committee on nuclear waste. In the Board's Fourth Report, we recommended that the DOE convene a workshop on the use of expert judgment. This workshop was held in November, 1992, and it was summarized by the workshop steering committee in 1993.

In the Board's 10th Report, we urged the DOE to utilize the insights gained from the workshop and to prepare and to implement a plan to increase the quality and effectiveness of its use of expert judgment in the high level waste program. This plan should include 1) guidelines for the use of expert judgment in both programmatic studies and performance assessment; 2) increased involvement of management in planning and monitoring the use of experter

judgment. We felt that the absence of management involvement had led to several problems in the past. 3) The plan should include increased use of outside expert judgment; and 4) development of an experience base of using expert judgment and interactions with the NRC.

We requested that the DOE present this plan to the Board, and we'll be hearing about it, this plan, in the first presentation of the day from Tom Bjerstedt of the DOE.

Following the DOE's presentation, Aaron DeWispelare of the Center for Nuclear Waste Regulatory Analysis will be substituting for Michael Lee, who appeared on earlier agendas, to discuss the NRC's views on expert judgment as formulated in his staff position paper.

We will round out part of the session before the break with a presentation by Steve Frishman, who will provide us with the views of the State of Nevada on this topic.

After the break, we will hear about some actual applications of the use of formal and informally elicited expert judgment. Kevin Coppersmith of Geomatrix Consultants will first brief us about the just completed DOE-sponsored elicitation of expert judgment on volcanic hazard at Yucca Mountain. The Board has felt that this would be one area where a well-structured probabilistic analysis using external expert judgment would be very useful in evaluating the

importance of some of the very contentious issues that have been raised. We are very interested in hearing whether this study has been useful.

Supplementing this talk, we have asked Alex McBirney, a volcanologist from the University of Oregon who served on the expert panel, to provide us with his perspective on the study in the process of expert opinion development.

Following the discussion of volcanic hazard, we will hear again from Aaron DeWispelare, who will discuss and NRC-sponsored elicitation of expert judgment on future climate at Yucca Mountain. Although this study was completed two years ago, it generated considerable interest and addresses a very important topic.

Finally, in the final session, final talk of our session on expert judgment, Bob Andrews of the M & O will bring us back to the current reality of ongoing performance assessment. He will discuss with us the use of expert judgment in TSPA-95, its accomplishments and problems.

With that, let's get going with Tom Bjerstedt.

DR. BJERSTEDT: Good morning. I seem to be coming across fairly well. My name is Tom Bjerstedt. I'm a member of the--assistant manager for suitability and licensing in the project office, and I'm here to present our talk on

expert judgment.

Topics I'll be covering are the purpose, goal and scope of DOE's expert judgment position statement, which we sent to the Nuclear Regulatory Commission on June 1st of last year. In it, we state a series of principals, and a synonym of principals are ingredients, and also our implementation guidelines, which we could also use as a synonym as our process requirements for when we do expert judgment applications.

I'll also talk about the status of past recommendations on expert judgment, and there is some backup material, which I won't explicitly go over in view graph form, but that may come up in questions and answers or as we go along.

The purpose of our position statement is to provide a set of ingredients and process requirements for formal applications of expert judgment and peer review--that would be elicitations or peer review conducted by Yucca Mountain for site characterization. It is a followup to the recommendations DOE evolved from the 1992 expert judgment workshop. It was meant specifically to attempt to resolve one of NRC's site characterization analysis comments on our 1988 SCP and to engage in a dialogue prior to NRC's intention to put guidance out about their position on how expert

judgment might be used in a high level waste program; also, to address and partially fulfill recommendations made by the Board in their 10th Report.

The goal of our position is to preserve DOE's flexibility in how we apply formal expert judgment; also, to commit DOE to some basic operating guidelines for the application and documentation of expert judgment.

I might point out that our quality assurance requirements and description under which we work does identify peer review, and there are passages in the QARD, and we also have an implementing procedure. But really, our QARD is silent as to requirements, specifications for elicitations. And so it was felt that this position would be a useful bridge and to try to at least lay out DOE's intentions for our ingredients when we do these and also our process requirements for what results and how they're conducted.

The scope establishes thresholds when formal applications of expert judgment might be appropriate, the principles or ingredients expected, the process requirements or guidelines for how they're conducted and expectations for the documentation that results from when we do apply them.

Insofar as the first sub-bullet, there are some thresholds identified in my back-up material on Slide No. 28,

which itemizes some of those thresholds. And I might point out that if you look at that, you would find that virtually every one of them is applicable to the Probabilistic Volcanic Hazard Assessment.

Just as a bit of background material, we have engaged in sometimes formal and sometimes less formal applications of expert judgment as we've proceeded with site characterization. On the less formal end of the spectrum, we could look at the development of our site characterization plan in 1988, and also towards the less formal end, I would put our technical assessment and the design reviews we do for our exploratory studies facility design reviews. Peer review would be somewhere in the middle, and towards the more formal end, I would place rather structured elicitations and some of the problematic elicitations that we had done in the early 1990s as examples of more formal applications of expert judgment.

I'd like to go through the general principles and the guidelines and then go off and talk a little bit about the recommendations that DOE has sought to address by putting this position statement out.

For elicitations or peer review, we'll have a predetermined structure for how the elicitation or review proceeds. It's either proceduralized under our QARD in the

sense of peer review or a planning document for elicitation. We expect each application to be systematic, open to scrutiny, easily understood and subject to the appropriate management controls under our QA program.

The bases for expert judgments, including data, assumptions and uncertainties, we expect to be explicitly considered and rendered transparent.

Principles, again, is the last slide for principles are ingredients. Responsible managers can be involved in the planning and monitoring each application to ensure that the results are useful. The Board has pointed out in some of its prior recommendations and comments on expert judgment that perhaps management had not been involved to the extent it needed to be to ensure that the results that evolved were useful for decisions that had to be made. And the documentation will be adequate to provide objective evidence that these guidelines has been satisfied and also that the controlling management plan has been faithfully executed. Again, in the sense of a peer review, that is an auditable process. We've had several peer reviews, and the documentation resulting from it undergoes a quality assurance audit or can be surveillance or audit.

Elicitations are a little different. We put out a management plan that specifies what we're going to do and how

we're going to do it, and the resulting documentation needs to be faithful to what we said we were going to do.

Insofar as our guidelines, we are committing, in effect, in this document to a set of operating procedures or process requirements for how we conduct elicitations in peer reviews. These sub-bullets I'll talk about a little bit more as I go along.

Commit to a planning document or procedure that lets people know what it is we're intending to do in the scope of the application, selection of experts, general selection criteria, independence, qualifications and balance and documentation. And I'll have a slide on each of these as I go along here.

Each peer review or elicitation, either by QARD requirement or by management commitment, will have development of a planning document or procedure that defines applications and appropriate controls. That would include a description of the issues to be evaluated, the spectrum and size of membership methods, processes to be used and a schedule for reporting results, and the considerations or criteria that should be addressed and documented.

For selection of experts, the number of experts involved would be commensurate with the complexity of the issues under consideration, importance of the results to

programmatic objectives, the number of disciplines involved-- there may be a lot of cross discipline application--the degree to which uncertainties exist, and the extent to which differing viewpoints are strongly held within the technical community.

So far as general selection criteria, we seek to include a diverse technical and institutional points of views and seek to include qualified independent experts that are outside of the DOE.

We will not have anonymous members. We will not seek to exclude technically qualified people only on the basis of having been funded by DOE for unrelated work or having the opportunity to have reviewed DOE-sponsored work. And we will also not seek to deliberately span the representation of stakeholder groups. Those are all considerations, but we won't deliberately identify membership partitioned out to interests of stakeholder groups.

For independence, a formal elicitation may include qualified experts that are associated with the project so that their knowledge specific to the issues under consideration can be dealt with and that they can benefit through that information and knowledge; also, external to the project to ensure that the range of diverse technical viewpoints are represented.

And a peer review will include qualified experts, and I mention again that that is a proceduralized process that basically identifies the people needing to be--conduct a peer review independent of the work that was conducted.

Qualifications and balance: Technical experts involved will have the qualifications that are recognized and verifiable and appropriate to the issues under consideration, have the expertise and qualifications that span the issues involved in the evaluation, including divergent technical viewpoints. And the potential for technical and organizational partiality will be minimized.

And insofar as documentation, our expectation is that what results is sufficiently thorough and complete to enable external parties to reconstruct the rationale for the results that were obtained.

Now I'd like to talk a little bit about the status of expert judgment recommendations that arise from three points: NRC's site characterization analysis done in 1989, the internal recommendations we made to ourselves back in 1992 after the Albuquerque workshop, and also the Board's May 1994 report.

I mentioned earlier that we wrote a position statement to specifically address Open Item No. 3 on expert judgment. The NRC wanted to have statement criteria for the

formal use of expert judgment, and their goal and interest in expert judgment is to ensure that its usage and how it's used doesn't foreclose the opportunity to gather reasonably available information.

And inputs that we accessed when we were preparing our document are the Center's report, background report on the use and elicitation of expert judgment, the NUREG contractor report, elicitation and use of expert judgment in PA and for repositories, and also the peer review NUREG that NRC put out in 1988.

We have received, by the way, confirmation that the NRC has sent us back a letter saying in July of 1995 that acknowledged that they received the information, and they also attached some transcripts from the ACNW's workshop and the staff's briefing to the commissioners, and the ACNW workshop that followed on June 21st and June 22nd of last summer. And they basically said that we're not prepared to really address whether you've resolved the open item, but here are some relevant pieces of information on what our current thinking is about what we're doing by way of guidance and how the NRC follows about expert judgment.

The November 1992 workshop was a rather extensive and thorough investigation. Potential applications had wide representation and a variety of viewpoints from different

organizations, and we feel we have had followup to the recommendations that take into account the fact that the program has shifted from more programmatic applications in the early 1990s to more focused technical applications in recent efforts, and that also that program management has changed, and that the OCRWM program has entered an era of limitations with our fiscal 1996 appropriation.

I'll go through the series of recommendations that were made and make a couple of statements about each one.

One of the recommendations was to evaluate decision-analysis approaches alternate to those that DOE has used. We feel we've been responsive in that we've planned two EPRI-type elicitations for geologic hazard evaluation. The Probabilistic Volcanic Hazard Assessment is the first one that's complete, and you'll hear a lot more about that today with two speakers, and also a probabilistic seismic hazard analysis, which was on the Planning Board for awhile and has actually began in the last fiscal year. But as I understand now, it's been interrupted.

The second recommendation, to develop a flexible plan for future use of expert judgment. We feel that the plan that we have put out is that plan.

And initiating training in quality decision making and the formal use of expert judgment. The Probabilistic

Volcanic Hazard involves training modules for the experts that are elicited for things such as debiasing and recognizing bias and how to elicit a technical opinion that can be used in the aggregation process.

Participating in a test case involving expert judgment in a regulatory environment, we didn't participate directly--we didn't participate as participants, and we were not observers of the climatic elicitation in 1993. We had a number of things that were underway in the project office. We had a major reorganization. We had a transition in the manager for the climate program. And up until quite recently, the climate program hasn't been given an awful lot of attention. And a search through the records, I don't recall--I haven't been able to retrieve evidence that we were actually invited to participate or observe. And I can be corrected on that if that's not true.

Holding a meeting with stakeholders for insights into alternative views, we feel we've been responsive to that in that how stakeholders and the public could be involved in our technical site suitability evaluation process, was specifically elicited in the workshops that we had in 1994 to develop the process.

Investigating the use of expert judgment by other government agencies, we have investigated other expert

activities, such as the ongoing Senior Seismic Hazards Advisory Committee, or SSHAC, studies while we've planned Probabilistic Volcanic Hazard Assessment, and the seismic application as well.

In the Board's recommendations from their 10th Report, one of them was to establish guidelines for formal use of expert judgment in programmatic studies and performance assessments. Our position statement deals mainly with programmatic issues and technical/management issues. Insofar as direct use and performance assessment, model uncertainties and scenario analysis, we haven't really fully explored potential applications in this program for formalized expert judgment application. There's been no clear benefit for doing so in the total system performance assessments that we've been doing.

Bob Andrews will talk a little bit later, and he will be able to describe how expert judgment is factored into the TSPAs. But as for a very formalized process to roll into a TSPA, we really haven't felt the need or seen the potential benefit of doing that yet.

And also, one of the recommendations to increase involvement of management and planning and monitoring formal use, we've been DOE's managers both from the regulatory side of the house and the technical side of the house has been

involved in all stages of the Probabilistic Volcanic Assessment, and we have been engaged from a management and a technical perspective in that elicitation.

To increase the use of outside experts, the Probabilistic Volcanic Hazard Assessment we feel has acted fully on this recommendation because there's a very wide representation, and many of them, most of them, are outside of the program.

Develop an experience base that includes the use of expert judgment in both internal studies and those involving interaction. The structure of the Probabilistic Volcanic Hazard Assessment, as you'll hear later, provided for outside observation, provided time for comments and questions from those that were observing, and also provided the opportunity for interested parties to present technical information that was relevant to the technical issues under consideration. So we feel that we've come a long way to try to involve other groups and to expand the representation of viewpoints that might be there.

And so in conclusion, I would say that our principles and guidelines document establishes requirements and minimum acceptance criteria for formal applications and peer reviews when we do do them.

And I can field any questions that the Board may

have.

DR. COHON: Thank you, Dr. Bjerstedt. Thank you. Questions? Garry Brewer?

DR. BREWER: This is Brewer from the Board.

As I think the only Board member who was at the meeting in Mexico when a great deal of the expert judgment activity was set out, I would like to commend the DOE for having listened and from all appearances actually have taken a lot of the recommendations that came out of that workshop and putting them to pretty good use. I look forward to hearing the rest of the presentations today.

DR. BJERSTEDT: Thank you.

DR. COHON: Other questions? John Cantlon?

DR. CANTLON: Yes, we're going to get into the specific topic later today, but since you've indicated that these principles and guidelines were in place for eliciting both expert judgment and peer review, could you comment on whether or not these principles and ideas might have improved the nature of the product that came out of the academy peer review? Is there some aspect of how that was prepared for that these guidelines might have rendered a somewhat different outcome?

DR. BJERSTEDT: I would say that we prepared these guidelines to help us not only commit ourselves to some

minimum requirements and expectations for when we do them, but they would be things that we charter ourselves to have done. With respect to the academy's review, that was an independently done--a review that was done by another organization according to their structures, procedures and traditions that we really had very minimal ability to influence.

DR. CANTLON: Yes, I agree with that, but early in your guidelines here, you're talking about what the agency, what DOE itself does to prepare for it, one of which is to get your data and so on into a high quality position and so on. And it seems to me that the guidelines really laid out some things that might have improved the quality of what came out. But we'll pursue this later.

DR. COHON: Don Langmuir?

DR. LANGMUIR: Langmuir, Board. Obviously, your offices are with DOE. I'm wondering to what extent the M & O management has been brought into this process and to what extent they are part of the evolution of this program and using expert judgment and will be implementing it for the DOE?

DR. BJERSTEDT: Well, certainly with respect to peer review, that is a proceduralized process that we have quality assurance audits on so that there's not a lot of--you know,

if we do them, we have to follow these requirements. And insofar as elicitation, this is a policy statement that essentially lays out what we expect to have as minimum acceptance criteria for the product that the M & O would deliver to us. And, in fact, many of these guidelines are line item inserted into the acceptance criteria for the Probabilistic Volcanic Assessment, for example.

Did I answer your question?

DR. LANGMUIR: Not quite. Let me expand a little bit on it. As I think about the program, there are all levels of activities within the program where sciences and engineering are applied at the labs and the GS and so on, and these are subcontractors to the M & O. Are we talking about here only dealing with larger issues within the program when we're talking about expert judgment; in other words, the larger programmatic products of the program? To what extent does this move down as a concept to more specific tasks within the program at the lab level, for example?

DR. BJERSTEDT: Well, since we have a streamline management structure and our M & O is managing these entities, if we lay out acceptance criteria for a DOE deliverable, then we would expect to have adhered to in the final product or else there would be some dialogue that goes back and forth.

I'm not sure I answered your question. I'm not sure if I understood it.

DR. LANGMUIR: Well, I think it will come out during the day. Perhaps I'll have a chance to find clarification later on.

DR. COHON: Pat Domenico?

DR. DOMENICO: There are experts, and, you know, there are experts. And if we take the Volcanic Hazard program, there are a few folks who know something about it because they've been studying it for several years. How is that information conveyed to the panel of experts that you may select that are not necessarily as familiar with it as let's say the number one principal investigator in that area, and would that principal investigator be included on your panel?

DR. BJERSTEDT: Well, in the structure of the Probabilistic Hazard Assessment for Volcanism, we did have the principal investigator there to be able lay out what it was that--the program that they worked to and to convey that information. It depends on the structure. For that particular one, there was a lot of internal communication amongst the experts. They had meetings and workshops that had presentations not only by workshop members, but by external parties to acquaint the panel members with all of the information that was out there, and then to discuss what

the merits were and in it. And I think you'll hear a lot more about this in the two subsequent talks.

DR. COHON: This is Jerry Cohon, Board. I have, well, two or three questions.

First, the guidelines as I've seen them and as you've presented them seem to be silent on the specific issues related to how one actually goes about getting opinions from experts and then how one goes about reconciling differences of opinion. These are formal methods that exist. Does DOE recommend particular techniques, or is that up to the people actually applying expert judgment?

DR. BJERSTEDT: For an elicitation, in specific, there's always a lower tier of detail that's laid out in a planning document, and the range of options that were exercised with Probabilistic Volcanic Hazard Assessment were specified in there. There may have been a range of options, or it may have been laying out a specific method under which to proceed. But a lower tier planning document would really be the place to find the details in how the elicitation was expected to unfold, involvement by other parties, et cetera.

DR. COHON: So DOE management is not providing any more guidance than we've seen here; that is, it's up to the--at whatever level this actually happens, it's up to that level to decide what specific techniques are used?

DR. BJERSTEDT: That's correct. One of the goals of this position statement is to preserve a degree of flexibility. The NRC has some concerns about how it's conducted, and they're considering, or I think they're going forward, I'm not sure, with a staff technical position that would lay out some guidance with respect to how it's done. You know, we would look at our guidelines again in that event, and, you know, it's a reb (phonetic) zero, so we have opportunity for expansion or growth or consideration of other conditions.

However, right now we wanted to put reb zero on the street as to just a layout from a programmatic perspective or project perspective because this really does apply to site characterization, and we wanted to lay out some minimum expectations and some process requirements for how we would-- DOE, as the organization chartering these things to be done, would expect them to be done.

DR. COHON: The written guidelines that I've seen say quite a bit about the importance of making a distinction between those issues in which you want the opinions of experts and those issues characterized by great value content where their value judgments have to be made, where you do not want expert opinion, reserving the flexibility of management to bring in other issues. You didn't say anything about that

today, and I wonder if you would like to expand on that?

DR. BJERSTEDT: Let me try to repeat the question as I thought I understand it. Is there a means by which you can tag information that is discussed as either knowledge-based or opinion or--

DR. COHON: Yeah, but my question goes beyond that. Your written guidelines basically say it's important for management to reserve flexibility to bring in non-technical issues in their decision making. Therefore, we want to make sure expert judgment techniques are only applied to those matters which are largely technical and do not venture into the non-technical.

DR. BJERSTEDT: Absolutely. I think from DOE perspective, we can see a lot of value in the way that these have evolved through the last five years. In the early 1990s, we were planning a site characterization program. We had an SCP. We had some fairly expansive programmatic elicitation for how we could configure an ESF and what the best options for underground tunnelling might be that would optimize various considerations. Those were large programmatic applications that dealt with a planning basis, which has a lot more degrees of freedom, a lot more variables that could be considered, and that's why they were probably rather expensive.

As we've moved into an implementation mode for site characterization, we're starting to try to see where we're going to begin to think about demonstrating compliance, and so the degrees of freedom in the potential applications are far more targeted, they're more focused, and they are less expensive.

And once we get into that realm, once we've stepped away from some of the programmatic applications that we did back in the integrated test evaluation, the test prioritization task, ESF studies, Calico Hills, all of these were elicitations trying to help us plan a program. Now we're trying to take that data that we've evolved, the data that may be existing from outside the program and to see where we are from a performance perspective, or in the case for seismic, to actually sit down and try to come to some agreement on design inputs.

And so that evolution has taken place, and we see a lot of benefit to trying to focus it on the technical issues.

DR. COHON: Don Langmuir?

DR. LANGMUIR: I think one of my concerns all along with expert judgment in the program has been my suspicion that given budget problems, which inevitably have been coming along and getting worse, dealing with goals that are fixed and means which are limited, would be forced, then, to using

expert judgment as a substitute for data and empirical information.

In your backup materials on Page 28 is circumstances where formal use of expert judgment is appropriate, and one of the bullets is data are ambiguous, non-reproducible, or I would emphasize or not reasonably obtainable. That might be read to me not obtainable because of budget constraints rather than simply because it's difficult scientifically or engineering wise to obtain it.

And I can appreciate the dilemma, the quandary that the program is in right now with a limited budget.

What's your view of the possibility--in fact, I would expect the program is going to be forced to argue that it's not obtainable because the money is not there in some cases, and so to conclude that the site is suitable because of this or that set of arguments, we're going to have to bring in the experts sooner than we'd like.

That's a generic question, but I guess I'd be interested in your thoughts.

DR. BJERSTEDT: I think, speaking again generically as your question was posed, an era of limitations does present constraint, and it does represent a fixed variable for what you might want to consider to undergo, an elicitation, for example, or a peer review. In a program like this, it's as

valid as a--that, as a specific consideration, is as valid as a specific charter to say, well, we've gotten--there are certain things we just simply cannot go after, irreducible certainties, so we have to try to make a decision with what we've got. That's one consideration.

Another one, and so the financial situation is definitely a variable, and it's going to be as valid as a selection criteria as the desire to take action or make conclusions with uncertainty.

DR. LANGMUIR: But you stand the risk that the experts will conclude that the uncertainties are so large that they can't conclude anything.

DR. BJERSTEDT: That's correct.

DR. COHON: John Cantlon for the last question.

DR. CANTLON: All right. And I would simply extend you're acknowledging the cost for the cost of deriving the data is a prohibitive thing. You would move toward expert judgment. Would not the same thing hold where the time to generate the data also are prohibited?

DR. BJERSTEDT: Yes.

DR. COHON: Thank you, Mr. Bjerstedt.

As indicated earlier, Michael Lee could not be with us today, but we're fortunate that Aaron DeWispelare from the Center for Nuclear Waste Regulatory Analysis is. He will be

a substitute for Mr. Lee, as well as giving his own presentation later.

Dr. DeWispelare?

DR. DEWISPELARE: Well, good morning. It's a pleasure for me to be here today. I extend a greeting for Mike Lee and an apology that he is still snowbound in the East and was not able to make it out here.

The proposed technical position that I'll be briefing this morning has been briefed to the NRC Commissioners and to the ACNW this past summer. It is currently undergoing a final internal staff review and is planned, either this position or something close to it, to be published for public comment at the end of this month.

I'd like to cover in this presentation a little talk about the need for the guidance, the scope of the guidance, the role of NRC's for expert judgment in the decision-making process, the proposed positions and a sample protocol for conduct of expert elicitation, and then closing with a current schedule.

The NRC believes that there is a need for guidance in this area. Obviously, the characterization, the problem that we heard earlier with the large uncertainties, makes it all likelihood that expert judgment will be used to support the licensing application.

In the past, the NRC has had specific concerns with the DOE's use, and this is documented in the record.

As was mentioned, both the ACNW and this Board had made comments about the need for both guidelines from the DOE and guidance from the NRC associated with this program.

And then finally, in the DOE guidelines that we just heard that were published this last June, there was a planned technical exchange between the DOE and the NRC to talk about the specifics of both of those, and, unfortunately, that was postponed indefinitely.

The scope of the guidance in this position, there will be a set of conditions which may warrant formal elicitation that will be included in this position, and a suggested protocol, which would have a set of elements which would be the minimum set, if you will, that would allow for a defensible process as well as for a process which allows credibility to be very high and for quality to be judged in the utility of the result.

The position, though, does not prescribe specific applications for expert judgment, and there is no intent to discourage less formal uses of expert judgment.

The caveat here, though, is that all judgments, as they will be used to support the license, will need to be documented to a level so that their utility and source can be

understood.

The NRC uses expert judgment as input to its decisions, has in the past. Basically, they are decisions based on fact, and when required, complimented with opinion and judgment. In a program like this, judgments are made routinely in a technical perspective all throughout the program. And NRC's perspective on this is that the judgments may compliment, but not substitute for reasonably obtainable data and analyses. And this feeds the basis for the reasonable assurance requirement that is in 10 CFR Part 60.

Prior to licensing, of course, DOE has a wide latitude on using expert judgment. The NRC's concern during this period is that if they see things going on that might hinder the quality of the license application as it pertains to the use of expert judgment, then they feel appropriate to comment. Once the review of the license application starts, though, the technical staff needs to have that basis of fact, complimented where appropriate with understandable, source-derivable expert judgment to produce their safety evaluation report. If they do not have that, they can request additional information at that time.

And now the proposed current staff position. The NRC will continue to accept judgment as support for license application, but not as a substitute for objective analyses

and data. Judgments may be both informal and formal. The key here is that they must be documented as to source and utility.

Areas appropriate for considering formal elicitation; when data is not available or obtainable, when the uncertainties are large and significant in terms of repository performance, when there are many prospectives and approaches that exist in terms of understanding the discipline and understanding the data, and when the literature contains what are called bounding assumptions, particularly if those are characterized as conservative and there needs to be an examination of those.

Next, the position is that a consistent, defensible process is used in the formal elicitation, and this, of course, builds directly in the usefulness of the data that's derived and the credibility of that data.

And finally, if after elicitation is conducted and judgments are derived, if new information becomes available, or data, then they would expect those results to be updated.

These are a set of steps, if you will, or parts of a suggested protocol that would contain a minimum set of elements which would produce a defensible and high quality elicitation, which would allow an observer to judge the quality and the product that comes out of it. I will spend

just a little bit of time talking about a few of these.

Starting at the top, a clear delineation of objectives really leads the elicitation process down the right path in terms of what is the essence of what's going to be garnered from this exercise, who are appropriate experts and so forth.

Criteria for the selection of the experts; clearly credentials of the expert are very significant, education, experience. But in addition to that, the diversity of opinion that exists in a discipline must be represented in the set of experts. And also, there needs to be a willingness of the individual experts to attach their judgments, if you will, to their name, no anonymous presentations. I'll say a little bit more about this in my next presentation.

During the free elicitation period, there needs to be appropriate decomposition of the objectives and issues so that definitional questions can be resolved. These are very complex kinds of questions that are asked, and the experts come from, even though they may be in the same discipline, come from different backgrounds, and they can misunderstand what appear to be a common set of terms. And if you're going to have comparable results from among the experts, that needs to be resolved early in the process.

Even though the experts are very familiar with the discipline at hand, many may be also very familiar with the problem at hand. It is quite likely, though, that they do not all have access to the same source data and range of source data. Likewise, they probably all do not have access to the different perspectives and the publications on the different perspectives associated with a particular problem. And so an effort should be made to gather that information and distribute that to the experts and make those sources of data available early in the process.

Again, even though the experts are experts in their field and their discipline, they more than likely have not taken part in an exercise like this, a formal elicitation. As a result, there needs to be a training session or sessions conducted to make sure that they understand what is going to be expected of them, what subjective probabilities for an encoding thereof is all about, and also sensitizing them to sources of potential bias when one goes about garnering certain type of information, particularly distribution.

When it comes to the elicitation themselves, the experts should be elicited separately, supported by the elicitation team of generalists and normative experts so that there can be uniformity of questioning preserved, as well as consistency checks applied in a thorough manner, and then

documentation for the individual elicitations should be done as completely as possible because soon after the elicitations, the documentation needs to be provided back to the experts and indicate what the elicitation team understood the experts to say; here's what the variables were, here are the distributions. And more importantly than that, or as importantly as that, is what are the bases, what is the rationale for these answers because that really adds to the quality and the utility of the data.

If during the feedback process the experts have disagreement with any of the things that are presented to them, that should be modified, of course, and rationals for those changes documented.

It is possible that the individual judgments are not all that needs to be used, that, in fact, many analyses and models require an aggregation of the judgments. When this is done, whatever technique is used, the impact of the individual judgments must be traceable from the aggregate. If there are disparate views, then those should be documented and the basis for those disparate views.

And really, the critical part of this process is the documentation, the what, why, when and whom. This is really the fundamental basis for an observer, judging the quality of the effort and utility of the output. So a

defensible process needs to as completely and as thoroughly as possible document the sources and what the experts said. If there were conversions or translations so that the parameters or distributions can fit models, those need to be documented as well.

And again, that is the final step, if you will, in that set of steps, which is a suggested protocol, which does give the minimum acceptable set of elements that provide a defensible elicitation.

And now let me cover the current schedule. As I mentioned, this position was briefed to the NRC Commissioners and the ACNW staff. It is undergoing final review, internal review now, and is expected to be published for public comment the end of this month.

Finally, that following that comment period, there will be a revision, if necessary, briefing to the ACNW again in late spring, the April time frame for instance, then finally, a final publication.

That concludes my presentation.

DR. COHON: Thank you.

Questions from the Board? Garry Brewer?

DR. BREWER: This is Brewer from the Board. We've heard the NRC's view, and we've heard the DOE's view just in general terms in terms of process, procedures and so on.

Could you characterize differences, conflicts, inconsistencies between the two? The point I'm trying to get at is the NRC has a certain view on the world of experts. How does this fit or not fit with DOE's view, which we've just heard?

DR. DEWISPELARE: The NRC is currently reviewing the guidelines, and so I'm not at liberty to really discuss those because that has not been completed yet. But in the past, the NRC has gone on the record in saying that a couple of the areas that they've had concerns with is the procedure in understanding the various elements and processes that take place in the elicitation itself, and then what specific areas that the DOE plans on using expert elicitation on. And so those are a couple that are documented right now.

I don't have specifics on differences between the staff technical position and the guidelines, though, to give you today.

DR. BREWER: Okay. This is Brewer again. I wonder if I could ask Dr. Bjerstedt to try to respond to the same question. It's I think at the crux of what we're hearing here this morning.

DR. BJERSTEDT: I don't detect a great deal of disparity between the upper level guidance, which would be by way of process requirements that we would expect to have as part of

these exercises with NRC's vision of how you may actually conduct them. However, they'd like a little bit more detail, and our approach is to lay out the upper level expectations and to make sure that we would be able to address when their guidance comes out, to look at it and ensure that the manner in which a specific exercise was to be undertaken, that disparate of those requirements are met, if not the letter--if the letter may not be appropriate for that specific application. But we would be able to document a variation of the variance if we didn't do exactly what their guidance may state to be their preference.

DR. BREWER: This is Brewer again. Let me say as far as we've heard this morning, there are no major inconsistencies between the two of you, it's probably in the details, and we shouldn't be surprised?

DR. BJERSTEDT: I see a member of our staff waving his hand in the back that would like to say something, and I'll let him say.

MR. SULLIVAN: Tim Sullivan, DOE. We have--we, at DOE that is, has had the opportunity to review our early drafts of the NRC staff technical position. Now, undoubtedly, we will have some comments during the comment period, but we find nothing, at least in early versions of that document, that's inconsistent with the application in Probabilistic

Volcanic Hazard Assessment.

And secondly, the NRC staff has participated as observers in all of the workshops that were conducted as part of PVHA. To this point, they have been supportive of the process, and perhaps Kevin will elaborate on that a little bit in his presentation. They did provide some comments to us and reiterate their focus on the documentation that will ultimately result from that process. And we are taking those comments into consideration as we prepare the final reports.

DR. BREWER: Thanks to all three of you.

DR. COHON: Clarence Allen?

DR. ALLEN: I just have a comment, not necessarily directed to you. But it seems to me that the attributes that we ask of experts, you have not listed one of the most important; certainly technical expertise, but almost as important is the absence of the intellectual arrogance. That is the--and I mean this very seriously. The willingness of people to modify their positions, to listen to others during the elicitation process, which is its whole purpose. And I don't know how to quantify this, but I would certainly place it very highly. I think all of us can think of people who are tremendous experts that should not serve on an elicitation team.

And let me just give one example here, familiar to

the geologists I think. Some years ago, there was a great debate about the origin of the Scablands of eastern Washington where one particular professor from Chicago who proposed that great sudden floods had caused these impressive features. Had an expert group been set up to evaluate his opinion, he would have gone down in flames, I'm sure, by all the experts or at least a great majority of the experts in this country.

One of the interesting things was after it was pretty well clear that he was right and the others were wrong, one of those experts, one of the most eminent geologists in this country, visited the site for the first time. And he stood there, and apparently the words he were to have said, "How could I have been so wrong for so long? Why didn't I have the intellectual flexibility to listen to others to try to review the evidence?"

And I would only emphasize that I think that is an equally important attribute, the ability to be flexible, as expertise, technical expertise itself.

DR. COHON: Pat Domenico?

DR. DOMENICO: I'm looking at your fifth slide, and it gives the role of expert judgment as input to the decisions. You have the first bullet that says, "Decision based on fact plus opinion."

How do you feel about decisions based on opinions in the absence of fact? I think I can recall at least one project in this whole program that comes very close to that. So how do you feel about that?

DR. DEWISPELARE: Let me defer to the NRC representative here from Las Vegas.

Bill, do you have any comment on that, the NRC's perspective?

DR. COHON: Could you repeat your name and affiliation again?

MR. BELKE: Bill Belke, B-E-L-K-E. I'm the NRC on-site rep.

And I am totally unfamiliar with it. I haven't been involved in it, so I would have to defer that and take your comment, and I'll get back to my management. That's an easy copout, but--

DR. COHON: No, that's a good answer. You're saying you have no facts, so, therefore, you have no opinion.

MR. BELKE: Right.

DR. COHON: Don Langmuir?

DR. LANGMUIR: Generic questions again here, but I think it's a real possibility. What I perceive is that the DOE is going to propose a number of things which will be looked at using expert judgment, which was part of the overall package

submitted to the NRC for licensing ultimately. And the NRC will say we need more data on each of these than the DOE can afford to get, either in terms of time or funding support. And the DOE will also say, well, look, you shouldn't be looking at these individual packages by themselves. It's total system performance. It's the total system uncertainty that really decides suitability. And maybe you should be more broad-minded and allow more uncertainty on these expert judgments, parts of the program or some of them, because even with those expert judgments where there are larger uncertainties that you might wish to accept because of a lack of data, total system performance is acceptable, or the confidence that you might have in suitability is high regardless. So, please, don't pin us down on the pieces and prevent the license when the overall system, even with the uncertainties you're not happy with individually, can satisfy suitability.

I wonder if there's any flexibility at all in the NRC to accept that approach?

DR. DEWISPELARE: I feel ill-prepared to answer that, but I will take that to the performance assessment folks at the NRC.

DR. COHON: Leon Reiter?

DR. REITER: Garry was asking about the conflicts

between NRC and DOE on judgment. Let me ask you about another kind of conflict that we've heard about, and that is that we had heard, and apparently there is a presentation to the ACNW to which some of the lawyers expressed reservation to the position. Can you enlighten us, or maybe somebody in the audience can enlighten us on that aspect?

DR. DEWISPELARE: I was expecting this question, I guess, and the response I would have is that the comments are on the record that were made there, and that the staff is internally reviewing the position at this time. And that's all I--

DR. REITER: Well, could you tell us the nature of the comments, the general gist of the comments? You or somebody else? I mean, you were there, I guess. What were they so concerned about?

DR. DEWISPELARE: I was not there for that entire presentation. I don't know if anyone else in the audience was. So I was only there for part of it, so I don't know the answer to the range of those concerns.

DR. COHON: Dan Metlay?

DR. METLAY: This can be addressed either to the DOE representative or to the NRC.

Both of you in your presentations have sort of laid out a long, and as I can sense, a rather sensible list of dos

and don'ts of how you should go about this kind of a process. The underlying implication is that if you don't follow these dos and don'ts, you're likely to produce a product that's not going to be very useful.

But there's another underlying inference or suggestion, and that is if you do all these dos and don'ts, you will produce a product that will, with some degree of confidence, reflect what the real world is. What's the basis for believing that?

DR. DEWISPELARE: Let me maybe take the first part, and you can jump in, Tom, if you want.

In the first part of the question, I think I would use the analogue of a quality assurance program for any data collection process. If you have a quality assurance program and you can document anything you need to know about where the data came from, how it was processed and so forth, and what accuracy of which your measurements were and all that kind of thing, then you're able to step back, and with some confidence, decide whether that data is useful to you or not. Or if you do use it, you're able, then, to quantify or in some way characterize the answer relative to some uncertainties or confidences that you have.

And so I think that's where the NRC is coming from. If you don't have that fundamental understanding of the

process of what the experts use for a basis and what their results were, then you may not have very much of a comment on really understanding the uncertainties that they were faced with, and, therefore the utility of that. But that says nothing about the fact if you follow all that, just like in a good quality assurance for data, when you get done, you say, well, I've got the wrong data, or I processed it wrong, or I asked the wrong question, something like that. So the two don't necessarily--

DR. METLAY: Yeah, I guess that was the point of the question, that that is clearly--it's reasonable to accept the first inference; that is, if you don't do all these things, you probably do have problems. But then the next question, which is the critical one, and I think several members of the Board hinted at this, is, in fact, the amount of experimental work is cut back because of budgetary reasons or because of time, how then do you grasp the results even of a very, very well done expert elicitation process and make some judgment as to how confident you are that they actually represent what the real world is?

DR. DEWISPELARE: I think you're in a position to try to assess that, if that second part is true; that is, if you understand where it came from, then you can start to say how valuable was this piece? How much confidence do I have in

this? If you don't have that, you can't get to that point.

I mean, you're right, there are going to be some tough decisions and some judgments involved, and is there enough here, was it done in enough detail kind of thing. But you basically cannot get to that point if you don't understand where the data came from and where the judgments came from and how the two intermix.

DR. BJERSTEDT: Dr. Bjerstedt. I would just add that if you do it here to the dos and don'ts as you say, that each organization is trying to flush out here, that you increase your likelihood of being able to submit a cogent body of structured intellectual thought into a licensing hearing that can be evaluated on its own merits. And each agency has to wrestle--as an expert agency, has to wrestle with the question of whether it's enough data or whether it's good data, factual or otherwise. But it is what it is, and when it goes up, it will be what it is, and you're increasing the likelihood of a potential licensing board as rendering an opinion, an evaluation that it's a good piece of work.

DR. COHON: This is Jerry Cohon, Board. I'd like to ask one last question. It's a specific one, and it's actually seeking clarification.

In the process that NRC follows itself and recommends, the experts are handled separately for the

purpose of getting their judgments. Is there any point where they come together and actually operate as a panel and interact?

DR. DEWISPELARE: Now, we'll be talking about the climate elicitation that we did. We brought them together on a number of occasions for dissemination of information, for training and so forth, to allow them to take advantage of each other's experiences and so forth. The only thing that was done separately was the elicitation itself.

DR. COHON: And specifically, does NRC recommend that you not bring them together to reconcile differences?

DR. DEWISPELARE: No, it does not.

DR. COHON: And you avoid doing that?

DR. DEWISPELARE: No, no, that's the case.

DR. COHON: Okay. There's--

DR. DEWISPELARE: Yes, the triple negative.

DR. COHON: You do bring them together?

DR. DEWISPELARE: Yeah, you can bring them together.

DR. COHON: Okay.

DR. DEWISPELARE: Certainly, if there are differences, disparate views, the documentation of those views are significant. And if you need to bring the group together to reveal those or clarify those, by all means.

DR. COHON: Reveal, clarify, reconcile?

DR. DEWISPELARE: And reconcile, if possible.

DR. COHON: So Expert A may make a cogent argument, Expert B says, ah, now I understand--

DR. DEWISPELARE: That's right. I think, though, as Board member Allen there suggested, and as our experiences with the climate elicitation, these folks really take their bases seriously, and reconciliation is a real challenge. And we're not very successful at it, and I'll talk about that a little bit when we get to it. And that's a fundamental of reality when you get both these experts that are very specialized and so forth. And, obviously, if you can reconcile, great.

DR. ALLEN: Let me just add--Clarence Allen--that having been involved in several these, that's indeed the most important part of the process, is having voting, so to speak, independently, then to, in a formalized way, discuss the issues with your colleagues, go back and vote again; if necessary, discuss again. And some people maintain positions, some people change them. But that interaction with other experts, to me, is perhaps the most valuable part of the whole process.

DR. COHON: Thank you.

And thank you very much for substituting. You did a very good job, and we'll tell Mr. Lee that you only had

very easy questions to handle.

It's my pleasure now to welcome back to the Board meeting Steve Frishman, who will be conveying to us on behalf of the State of Nevada its views on expert judgment. Mr. Frishman is with the Nevada Nuclear Waste Projects Office.

MR. FRISHMAN: Once again, I'm not going to break tradition, and you have no handouts.

I've been thinking back to the 1992 workshop, and I want to borrow from something that I believe Ralph Keeney (phonetic) said there, and I know I've heard him say it other places, too, and that's what we're trying to do in the way of site characterization and maybe even licensing interaction, is we're trying to as best we can describe a state of nature. And what we have in the realm of expert judgment can be nothing more than describing a state of knowledge.

And I think there's a real difference, and I want to talk about that and maybe a few other things that are very closely related because in the approach to expert judgment that we're seeing developing, both by the Department and by NRC, what we're looking at is really, and not surprisingly, how a lesson to DOE, or DOE writing its own lesson, on how it's going to be able to acceptably present what it causes evidence in a licensing area.

And that's about all we're talking about here.

It's how well do you have to get it down to make it acceptable and believable in a licensing proceeding so that the state of knowledge is considered somehow to represent a state of nature.

Well, we've been going through this for a long time, and I think you've heard me speak on this. The ACNW has heard me speak on this. And I know that by now you're all well aware of how skeptical I really am of that whole process, and because of some of the details that we don't see in front of us, but have been well recognized as being necessary to understand by some of the questioning today.

Let's start out with a product, a 1990 product of expert judgment in this program. And I know we're all supposed to leave things behind us that are behind us, and that was done programmatically, and now we're talking about specific topics and so on.

Well, the Calico Hills Risk Benefit Analysis is one that I think we have behind us, but at the same time is worth looking at just very briefly to understand where expert judgment can lead you. I remember the day that Dobson came running out saying, we have finished the Calico Hills Risk Benefit Assessment, all done. We are at the answer, and the answer is that Calico Hills is such a good geologic barrier, we don't need to know any more about it.

Well, this was in one of the project's TPO meetings when Dobson came in and announced all this, and there were some funny looks around the room. And most of the people said, or appeared to be saying and thinking, well, can we really say this out loud? And, you know, we really don't have any data, but the expert judgment is that it is such a great barrier, we don't need to know any more about it. It's going to do the job.

Well, the skepticism or the apprehension about whether you can go public with such a thing got so great, that the whole thing was given back to the decision analysts to rework, and they came up with this concept of, well, maybe that's the right answer, but maybe we need to have some kind of other answer that says why we're going to look at the Calico Hills anyway. And they came up with this new term, "value of information." It isn't necessary, but we need for it value of information.

And I think you can see, here we are five-and-a-half years later, just yesterday someone asked at this table, are we going to look at the Calico Hills? We don't know.

So that's what expert judgment has done for this program so far, and I don't expect that the results are going to be any better or any more convincing when you see it applied in a much more workman like way, workman like

meaning, the procedures are down better, and you can prove you followed the procedures.

And I think the discussion related to one of the questions was correct, the Department is treating it, and NRC to a certain extent, for very different reasons, I think. NRC is, I think, very honestly is trying to tell DOE, this is what it takes to present evidence, and evidence that at least is admissible. But I think from the DOE standpoint, they're treating it very much like they treat QA, and that's if you've got the procedure right, you must, therefore, assume that the answer is right. And that's wrong, and it's going to stay wrong. You don't know that the answer is right.

And we see both from DOE's standpoint and from NRC's that, you know, of course, we're not going to use expert judgment as a substitute for data. But then we also hear a little bit of hedging out of one of the questions that we just heard right now about, well, what will you really do in deciding whether to use expert judgment when you get down to not only cost, but time also in making decisions? Are you going to use expert judgment, or are you going to make the investment?

Well, Dan Dreyfus gave us the answer yesterday, and this is from his prepared statement. "In my view, the cost can be significantly reduced if the focus of the presentation

and the licensing review is on the predicative performance of the repository and on the safety case made for a specific repository design rather than on a comprehensive discourse on site characterization."

That's the answer. That's what you were talking about, Don, in your question. That's what the Department is planning to do. And now if we look at ways that we see this being implemented already, let's go first to the basics. Basics as of today are the waste isolation. And the waste isolation strategy, if you look down that, has notably excluded issues relative to site performance that have anything to do with seismicity or volcanism, and that's because expert judgment has already led the Department to believe that they have proven their hypothesis that neither one of these is a problem, otherwise they would be on that list because they have been known to be a problem all along.

And we're to the point now where through just even the application of the concept of expert judgment, we don't have to talk about them anymore when we're making, or when the Department is trying to make a waste isolation case. I think that's important to see, and I think what we're going to see is more and more things sort of dropping off the list because they have the authority of expert judgment having been applied to them.

Now, that list is an important one because if you read at least the document, the waste isolation strategy document that is available, that was handed out at your meeting, and look at that, and also look at the TSPA-95, what you'll see are some things that are now becoming sort of expert judgment because they're sort of building themselves into the assumptions that are beginning to answer some of those hypotheses already that you see in the waste isolation strategy.

And what it's leading to is a set of assumptions that are going to remain there for any determination that's made in 1998 that we know there are contradictory assumptions that change the answer tremendously. And this has to do with the thermal loading issue, and the thermal loading issue going directly to the question of whether you can keep moisture away from the container for a very, very long time.

And if you put the two documents together and sort of kick them back and forth, what you find is that there is really a very large controversy over the thinking on whether that can happen or not, and it has to do with the assumptions, and a whole set of assumptions that applied one way lead you to think, well, you might get a few thousand years out of a container, another set of assumptions that lead you to think, well, you're going to get tens of

thousands of years out of a container.

And this is sort of being built into the waste isolation strategy concept, and in 1998, there will be no data to tell you which is a better idea. What you will have are persuasive arguments that go more towards the long time because there's advantage in that for the Department. But you also have in the TSPA the acknowledgement that you change those assumptions and it doesn't work that way, or it doesn't work nearly as well that way.

And now we have no time, we have no money, but a decision is going to be made, and it's going to be called an expert decision one way or another. It may go through the evidentiary process that has been laid out here. In fact, I doubt it will because I don't think you can get a panel of people together who would give you what DOE says is part of the circumstances where expert judgment, and NRC notably does not say is that it doesn't fit into the box if there is an opportunity for consensus building. NRC doesn't ask for consensus building. NRC says specifically, point out what the big differences are and document those differences. This is not a consensus-building process. This is a documentation process.

So we're seeing expert judgment from the perspective of DOE invading the program in one sense and a

proposed very, very formalized way to get rid of issues, the two big missing issues that are already gone, even though the expert judgment is not finished. We already know how it's coming out. So those issues are gone. They don't count in waste isolation concerns anymore.

But at the same time, we see a growing, and I know we're going to see even more growing use of expert judgment at the not so rigorous level that drive assumptions into the analyses, and you have to dig back to even find them and figure out where they came from. And the thermal loading issue is going to be the big one, and at this point, we're going to see a decision in 1998, if the program gets to that point, that is underpinned by expert judgment that most likely does not meet even one of NRC's criteria, but DOE presents and puts forward as essentially a consensus understanding that everyone is supposed to then accept.

This is where the danger lies, and I'm speaking to I think the Board in trying to get you to understand that there are two big questions about expert judgment that are really, you know, in the forefront. One of them is the issue of whether--or the extent and circumstances under which it should be used, and the second is if it's going to be used, then what are the procedures to keep it honest; honest only to the extent that the results can, in fact, be presented as

evidence because if the Department is off on a track where they're doing work that can't be presented on evidence, then, yeah, they are wasting the money and the time.

But if the Department is on the track of trying to make something look authoritative when, in fact, it's not, then I think you, the Board, need to find ways to tell them that they're wasting your time even telling you what they're doing.

So we're no farther ahead really than we were in 1992 in our thinking about expert judgment. The only thing that's moved forward is that there's more paper laying out a process, but we also have circumstances that are overtaking all of this that are--and, in fact, outrunning this very type procedure that's being laid down. And I think the great example of that and the one that is probably going to become the one that worries us most is the expert assumptions that are behind how DOE goes about dealing with those five hypotheses in the waste isolation strategy.

And I'm only saying this today because that's what the circumstances are today. In 1992, we were talking about other circumstances, but the problem and the issue is still the same. And I think we're going to have to come to grips with it at some point. I, for one, don't give it an awful lot of thought because I think it's premature. I think at

this point most of the anticipation of using expert judgment is sort of wishful thinking, and it's wishful thinking because you can get people together when you can't get data together because it's cheaper and easier. And the Department, I believe, firmly subscribes to the concept that it is consensus-building. Well, the consensus ain't going to be there, especially without the data, and that's just going to be a fact of life.

So I think continued sort of emphasis on when and where you decide to use expert judgment is we're going to see it growing in the face of lack of money, but that doesn't mean that we have to be any more accommodating to it than we were when we thought that there was going to be all the money in the world to find real answers rather than trying to describe a state of knowledge rather than a state of nature.

As you know, I could go on much more on this and give you more sort of horrifying views of how it all works, but I don't think I really need to. I think Dan--if it had snowed just a little bit harder, Dan wouldn't have been here, and he wouldn't have been able to make my point before I did.

So any questions?

DR. COHON: Thank you, Mr. Frishman. This is Jerry Cohon from the Board. I'd like to ask you a question for clarification.

With regard to seismicity and volcanism, towards the end there, you posed your two questions or conditions, which I find very helpful. And I wondered, do you object to the use of expert--in the case of those two issues, seismicity and volcanism, is your problem with them question one or question two, or both? That is, do you think DOE--it's inappropriate to use expert judgment in those two cases or that expert judgment is being used not appropriately?

MR. FRISHMAN: I'm not satisfied that we are at a level of information where the decision to use expert judgment was a correct one or an appropriate one.

DR. COHON: So you think maybe it fails on test one?

MR. FRISHMAN: I'm not sure that it passed test one because I think there's a possibility that there are more and better data that might have made us more comfortable with some of the concepts before ever having to convene expert judgment to put them to bed.

And second, I guess the concern that I have on how they did it goes to whether the experts were selected from a broad enough pool of points of view. And that's one of the most difficult parts, as Clarence was talking about. You know, how do you make sure you have the right people who are going to do the job and do it in a way that doesn't raise questions? And I'm not speaking to the credibility of any of

the experts. I'm talking about those who selected the pool and tried to look at the pool and say is this going to do what we really need done? And where that comes out is when you get into a situation of aggregating answers because you can end up very easily with a range of views that are so tight that it makes essentially a point.

But in the alternative, you can end up with a range of views that is very wide, and you have to aggregate to some point where what you get as an answer, none of them would have given.

And that's just one of the traps in the procedural part of it and one that maybe that is unavoidable, but if you work real hard at it and do it explicitly, maybe you can avoid, you know, the worst of the trap.

DR. COHON: Other questions from Board members or staff?

Thank you very much, Mr. Frishman.

MR. FRISHMAN: We'll be doing this again in a year or two.

DR. COHON: We'll take a break now, and we'll reconvene at 10:30.

(Whereupon, a break was taken.)

DR. COHON: We turn our attention now to applications of expert judgment, and we begin with a presentation on the results of Probalistic Volcanic Hazards Analysis from Kevin

Coppersmith of Geomatrix.

DR. COPPERSMITH: Thank you.

The question you never want to ask is am I wired?

We're going to talk about the Probabilistic Volcanic Hazards Analysis. That's likely the last time I'll call it that. From now on, I'll probably call it PVHA, as it's become known. This is a study that was sponsored by DOE, and the goals of my presentation here are not to give you detailed information on the assessments that were made, although I will touch on that, but to keep with the spirit of the topic of the session, which is the use of expert judgment.

So I'll be focusing here on procedural aspects, the process that was followed. In some cases, I can get into the level of detail that anyone would like me to in terms of how say particular interactions with the experts occurred and other details like that.

But I'll stick with process and throw in a couple of maps to keep Clarence and others awake.

The purpose of the project is to develop an assessment of the probability of disruption of the potential Yucca Mountain repository. Importantly, the probability estimate needs to incorporate uncertainty. And, in fact, the assessment of uncertainty--I think, in fact, all the

discussions that we're having here today regarding expert judgment I think are a subset of the larger--the total issue, which is one of uncertainty. When we deal with the playoff between data and expert judgment, it's simply a subject of uncertainty. If we have very few data, we may and should display a very large uncertainty. If we have a lot of data, we should, if we're lucky, have a narrower uncertainty.

We use experts to process data in all cases, even if it's a single-age date or dozens, as you'll see in the volcanic for Yucca Mountain, hundreds of age dates on a particular rock.

Disruption is defined as the probability--I'm sorry, disruption is defined as a physical intersection of magma with the repository volumes. We're dealing with the probability of or frequency, annual frequency, of an intersection. Probability for these purposes is a frequency because we're in very low numbers, $10^{-7}$, $10^{-8}$ annual frequencies. Well, to be displaying those as annual probabilities or annual frequencies, we asked the experts to consider the probability over a 10,000-year time period. After we had begun the study, the concept of possible or longer time periods in the future was introduced. We only discussed that informally. These assessments were not set up with a criterion of a forward view of say the next million

years. They are focused over shorter time periods of 10,000 years.

The uncertainties are incorporated using multiple experts. So we're able, then, to get the diversity of views of cross experts, as well as the uncertainties within a particular expert's own judgments.

Both modeling and parameter uncertainties are important. We see the application of this PVHA product in assessments of risks, consequences, as well as the performance assessment.

Now, in describing PVHA, there's a couple of key components. This looks very much like Probabilistic Seismic Hazard Analysis, for those that are familiar with that. There's two aspects, the frequency of occurrence of volcanic activity and where those volcanoes would occur or subsurface dikes would occur in terms of their spatial location.

In the Yucca Mountain region, the number of volcanic events is low. We're looking at about 10 volcanic events on that order over the last say five million years. So generally, we're dealing with the phenomena that recurs on the average about every half million years.

Because of these low numbers, the data in the future location of rates of volcanism are uncertain. If we had a case where we had hundreds of volcanic centers, like we

do for some fields in the Basin Range, we'd have a better handle in the future of spatial and temporal distribution, but we'd probably also have a different set of problems.

To a variable extent, as we saw in the assessments by the experts, the methods that were used and the parameter of values come from site specific Yucca Mountain data or from analog regions around the world. One advantage of this particular panel is they have a lot of worldwide experience, were able to use that analog information for the Yucca Mountain assessment.

The structure of the hazard model and the uncertainty treatment, the use of things like logic trees and proability density functions follows that very commonly used in the seismic hazard field.

Just my one comment on the use of expert judgment, it follows on my earlier statements, reflects my own personal bias more than anything else here, is that in my mind, the use of expert judgment is used in any complex technical problem. In fact, even in simple problems, expert judgment often comes into play. However, that judgment is often implicit, undocumented, and the goal here in this study is to make the expert judgment process explicit, what was the basis or reasoning for your assessment, and to document that process.

Again, I think here the larger goal in this study and similar studies is to quantify uncertainty, and that uncertainty comes from a lack of data or a variety of models that can't explain the available data.

Before I get into the expert panel itself, let me just through some of the interactions that occurred as a way to give just an overall view of the study. It's impossible to go into all of the detail here, but I'd like to give you a feel for the types of interactions.

I think the recurring theme and the methodology followed here is one of interaction among the experts, and we talked a little bit about that previously.

Four workshops were held and two field trips during the course of the study. You can see it spanned about a one-year period overall, and let me step through some of the topics here.

The first workshop was designed to identify key issues and to associate those key issues for the analysis with the data requirements, the types of data that experts would need.

Early in the process we want to know from the experts what data sets they would like to have. They, also, many of them, as we'll see, are not site specific. They're not Yucca Mountain related people, and they don't know how

much data exists. They don't know the formats. This was an opportunity to discuss those data sets with them to make requests for data, and we then spent a lot of time getting those into formats and getting them into their hands for the subsequent analysis.

The first field trip was held at Crater Flat, and I should point out here that in all of these interactions, workshops, field trips, not only did we have the participation of the expert panel, but we brought in other experts who were not on the panel, others who could have been on the panel, others who potentially or in a conflict position, members, for example, from the Center, from UNLV and so on, who themselves were acknowledged experts in these areas, and they contributed very heavily throughout the course of the project.

For example, the Crater Flat field trip was co-led by not only the DOE-related individuals, people from Los Alamos, but also UNLV. Gene Smith, Gene Yogodzinski and that group led much of the field trip in northern Crater Flat.

The second workshop was an opportunity to talk about alternative hazard methods and models; what are the procedures that can be used to carry out a PVHA. Essentially, the tools in the toolbox. This is a case where many of the participants and presenters at the workshops had

themselves published methods and procedures, both for characterizing the spatial distribution of volcanoes or the temporal distribution. And we had an opportunity to hear those assessments so that the experts would know what tools were available to them.

At the same time, we encouraged them to develop their own methodologies, and what we saw is that many of the experts, in fact, came up with some new methods for dealing with the future spatial distribution of volcanism.

Field Trip No. 3 was to Sleeping Butte and Lathrop Wells. This was an opportunity. We had heavy involvement here by the USGS and some of the work they were doing in the Sleeping Butte area. Lathrop Wells is another example where a lot of work, H dating mapping had gone on not only be DOE-sponsored scientists, but by other groups, in particular the USGS.

This has been a very contentious area, and we were warned early on that, hey, get ready, these people are going to beat each other up, and watch out especially when you get them in the field where they have implements where they can actually hit each other.

We found that this was a group that occasionally a snide remark was here or there. That was usually by us in the methodology team trying to get people back into the

buses. But in general, people acted at a very high professional level. In fact, we demanded that. They were reminded of guidelines periodically throughout the course of the study, but, in fact, they didn't need to be reminded. This was a group that could interact on a professional level and honestly disagree.

Workshop No. 3 was an opportunity to do two things. You should include this. This was elicitation training was inadvertently left off. It was very important. We devoted the better part of the day to elicitation training. We had a normative expert, Bruce Judd, who has done these things before coming, go through the process of training, what they're going to go through in an elicitation, how it will encode subjective probabilities, what some of the biases might be. And, of course, these guys, this what I call the Stanford Mafia, who are the normative experts, who make twice what we do, did a wonderful job of carrying this out. And it's very important because as we said before, these are technical experts, and they're not--in many cases, had not been through an elicitation process before.

At the same time, Workshop No. 3 was the time to really get down to the brass tacks and to talk about alternative interpretations at Yucca Mountain, to actually put up together side-by-side someone who says that the age of

Lathrop Wells is 100,000 years and someone else who says that it's 5,000 years; to have alternative interpretations of the numbers of events in Crater Flat, for example, or the potential for alternative models, conceptual models, tectonic models and so on.

This was a chance for all members, and in many cases the Center, for example, had done a lot of work there. And this was an opportunity for them to present some of their spatial models, spatial smoothing and so on.

The elicitations occurred after that. Those occurred in a series of two-day meetings for each expert, and I'll talk a little bit more about that in a minute.

Workshop No. 4, we then went back and did calculations based on that first round with the experts. We also documented the elicitation in the course of that and gave that to the expert for their revisions and further documentation.

Prior to Workshop No. 4, we provided that written documentation to all of the experts so that they would have an opportunity to see what their colleagues had said, to see the technical basis for the assessments that have been made, and they could go into that workshop with the knowledge of not only what they said, but what others said.

Workshop No. 4 was a chance for them to display and

present and defend their interpretations. It was an interactive type of process. We also tried to focus it on those elements that were most important to the first round results.

Overall in the study, we're now in the process that we've finished a final loop following this workshop. They've made the final revisions, and we are in the process of developing a report due from us in draft form at the end of February.

Let me go now to the members of the expert panel, and because of some interest in the selection criteria, I thought I would just touch on this a little bit more than I have in written material.

This is a group of 10 experts, a highly-esteemed group with a lot of experience and high levels of capability. The way the panel was selected was to first seek nominations from a dozen or so acknowledged experts in the field. We developed a large list, a pool of 60 or 70 individuals who potentially met the selection criteria, and then narrowed them down to a group of 10 that we felt represented a balanced diverse group.

Let me quickly just mention some of the guidelines for selection because it has been brought up. First is an earth scientist with widely-recognized competence and

academic training and tangible evidence for that competence and journals and refereed reports.

Second, the understanding of the general problem area from experience in collecting data in Southern Great Basin or similar extension environments. Experience at Yucca Mountain was not necessarily required. In fact, most of the experts had no previous experience at Yucca Mountain.

Third, and importantly, an availability and willingness to participate, to maintain a commitment to the project, to continue to give it high priority through the course of the study, an issue that does come up on other studies, but didn't come up on this one.

Fourth, personal attributes and strong communication, interpersonal skills, flexibility and impartiality, one of the criteria that Clarence mentioned, and the ability to voice their own interpretations and not necessarily those of their institutions. We're asking for their personal interpretations as experts.

And finally, we were seeking to provide a balanced panel in terms of diversity of opinions, areas of expertise and institutional organizational backgrounds.

So I would say that this panel could go through every one of those criteria. They're all of high stature, many of which have international reputations. As a group,

they represent a diversity of areas of expertise, from isotope geochemistry through field mapping to a couple of experts who, in fact, are experts in volcanic hazard itself and hazard methodologies, people like Dr. McBirney who has been involved in hazard methodology and hazard analyses at other locations.

Some have spent their professional career, like Bruce Crowe, at the Yucca Mountain area. Others had very little familiarity with the Yucca Mountain databases. That puts a heavy burden on homework, on data dissemination, on the workshops to get that information to the experts.

Just quickly on the methodology team. We were, obviously, in charge of developing the methodology, but also its implementation. Just a couple of names that you'll recognize of people of some prominence in the seismic hazard field, like Allin Cornell and Carl Stepp, who have been involved in large multi-expert studies in the past. Others who have been involved, who themselves are volcanologists, Steve Nelson and Dick Smith, who provide the technical experience here. And then those who are hazard analysts, it involved either that expertise or areas of expert elicitation, like Pete Morris and Bob Youngs.

A couple of just interesting components of the processes we went through that might be important here from a

procedural point of view. The experts--I went through the expert selection process using explicit criteria, and we feel as a whole represent a balanced group. The question always comes up, could you have gotten another set of 10, and I think that we could have, that this is not the only group. I think it would be difficult, but I think you could find another group that represent the same type of balance that we feel we had here.

They have a range of views, and they voice them throughout. These are people with prominence and are used to being heard, and they also are used to listening. But they voiced some very strong opinions through the course of this, and I think that, from our point of view, was exactly what we wanted.

Many of those who are not selected or who are unable to serve on the panel because of their institution were involved heavily in the course of the subsequent workshops. We had over 30 additional people involved in presenting ideas, making presentations at workshops or in leading field trips who were unable to serve on the panel. And I think that helped to give the experts an opportunity to hear some other views that might not be represented say as the primary author on the expert panel itself.

An important component of all of these, and people

that have worked on some of the big studies back East in seismic hazard know, was getting the data to the experts and giving them an ongoing process for retrieval of additional data as they need to get it. And we tried to set that up. When they hear about something or we would hear about additional data, this is obviously a study where data is being collected all the time, and we need to get it to them in an early fashion. This was where DOE and USGS and other groups were a tremendous help in carrying this out.

Interaction is a key component of the study. We encouraged it throughout the projects. We facilitated it. We would have dinners prior to workshops, between workshops. We would have interactive meetings to allow people to have a better understanding of technical issues. We tried in every case to have interaction, and this is contrary to some earlier studies.

I think what's been seen, and we saw it in our C guidance as well, interaction is the way science is normally done. You don't give a talk at the American Geophysical Union and then get in your car and drive home. You give that talk, and then you get surrounded by everyone who disagrees with you, and you battle it out. That's exactly what we do here, unless you have a really good idea, like characteristic earthquake. That's a different story.

Technical challenge and defense of interpretations is facilitated. It has to happen. And technical challenge can be kept at a high professional level without personal insults. So there's a set of ground rules that I presented at the beginning of each workshop that basically was designed to avoid personal confrontations. That never needed to be implemented. I never needed to step in, but I was prepared to, and I think it's part of a procedural guidance that there are cases where that needs to be moderated. Again, here people were able to keep at a high professional level throughout.

We did have additional participants who were not on the panel brought in from outside, who were a tremendous help and often were the lead in the discussions in field trips and so on. They also bring, obviously, help establish a broader diversity of views.

Elicitation training, which has become a stalwart, every expert needs to go through the process of this training to understand how his judgments can be represented.

Again, the training is not so much a process of telling them what they're going to say. It's telling them how to quantify their uncertainty. To me, that's what you're trying to do. They don't know the number of events in northern Crater Flat with certainty. It might range from one

event where all the cones in Northern Crater Flat are all related to each other, or five events where they're all separate, or maybe seven where there might be some buried and hidden and unknown. That uncertainty in that parameter, in that distribution is what you're eliciting from them.

And like other studies, for those that have been involved in this, once you get into it and do more, the experts feel more and more uncomfortable in expressing their uncertainties. In fact, it's a relief for most technical experts to be able to finally admit that they're uncertain; in fact, they can give it to you readily.

Elicitation interviews, we found in others, I think the NRC's, in their guidance, is saying that individual interviews are really the best way to go. A group type of setting leads to all sorts of problems of dominant personalities and so on. Individual interviews were conducted two days, two full days, and with some followup in some cases to carry them through.

Feedback is also an important thing. Again, in previous studies, all these things have been tried or not tried. It was not clear whether or not with feedback or seeing what others in your group had said was a good thing. We found that it was. It did not lead necessarily to a convergence of views.

This issue of consensus in some guidance called the SSHAC study, I'll maybe talk about it a little bit later, we deal with levels of consensus. And a perfect consensus would be agreement on all models and parameters. Another level of consensus would be agreement that the process that you followed was okay. Somewhere in between is there's agreement that my uncertainty is properly represented and I see it in the total uncertainty of the group. I don't necessarily agree with others in the group, but I see where I am.

Those are all levels of consensus, and we're not trying for same models and parameters. We're hoping we get agreement of the process was a good one. That's what we're trying for.

Aggregation is the process of integrating, pulling together diverse views. Our goal in this, and we made it explicit from the beginning, was to be able to apply equal weights to all 10 experts. To do that, you need the process to start at the beginning from expert selection through the dissemination of data, through equal interaction, opportunity to learn and to gather and to look at the data sets, such that at the end you're able to say--and the other thing is to keep them motivated and participating, so at the end you can say that we can apply equal weights in a defensible fashion.

Peter Morris, one of our members of the methodology

team is an expert in this area of aggregation and has also helped develop the guidance on aggregation for the SSHAC study.

Documentation, which we're in the middle of now, but occurs all throughout this process, from the strategic plan for the study to the expert elicitation or process that's followed, will be a key part--and we know that--will be a key part of the review of the project.

Let me just throw a little bit of science into the mix, as I see Clarence is beginning to nod off. Let me just deal with a couple of issues. By the way, I do know Clarence from past multiple expert studies, so I can make these sort of comments, I think.

In terms of the spatial models, it's interesting for those that have been involved in seismic hazard, particularly in the eastern United States where seismic zones are usually defined, that's a common process that's followed in Probabilistic Volcanic Hazard Analysis, too. These are homogeneous zones or zones within which there's a homogeneous spatial occurrence assumed to occur, or differences. And there are some models that some of the experts used that allow for different rates of occurrence within the zone.

I'll show a couple of those just for interest. These are different ways of partitioning out the future

spatial probability of occurrence of volcanism in the area. Just to get you oriented, this is the Yucca Mountain area here, the proposed repository footprint right there. This is Yucca Mountain--I'm sorry, Crater Flat, with the volcanism in northern Crater Flat, one million old centers here, 3.7 million old centers here, Lathrop Wells down here. These are the aeromagnetic anomalies in the Amargosa Valley, Buckboard Mesa, Thirsty Mountain, Sleeping Butte area up in here.

What is shown here, and this is from Bill Hackett, one of the experts on the panel, is the way that he would partition out his source zones relative to particular age criteria. For a time period of the post five million years, a zone would be identified that looks like this that would include basically the centers that are in post five million year period, and for a more recent time period, the area identified in red.

One thing that we saw was a tight linkage generally between the spatial distribution and the pattern of past volcanic centers, as well as an explicit consideration of the age of those centers. And that's a little bit different from those that are familiar with the seismic hazard analysis, where often there's more of a tectonic framework and not much difference in the distribution as a function of the age of the feature.

In the case of seismicity, earthquakes, if they've occurred, there's uncertainty in their location, but their occurrences is very recent. Many of these centers are eight, nine, ten million years old. So it affects the distribution.

Here's another example from R.V. Fisher, who considered, again, the age of these, did not consider the intervening region between Sleeping Butte and Crater Flat. His arguments were that in the last 10 million years, we have not seen evidence of basaltic volcanism in that intervening area, and we would not expect it, other than in a large regional background.

Now, I haven't shown this, but all the experts over this region, either in a region that would encompass something like this or in some other region, there is a background rate of occurrence of volcanism. There is a background rate everywhere in this particular tectonic region for volcanoes to occur. There's nowhere on any of these maps that have a zero probability of future volcanic occurrence.

You have other examples in your packet. Maybe I'll skip those. Maybe just show one to show a little bit of color.

This is Bruce Crowe's interpretation. The reason I'm going to show this, and this is common to all of the experts, is they're expressing the uncertainty in that future

spatial distribution, and they express that by alternative configurations, as you can see here. In most cases, these are actually alternative models. Each of them have their own particular rate, and they also have a weight, a relative credibility of each model.

Each one of these models in the text is described in terms of the basis for it, its tectonic basis or age basis. They each have parameters that will lead to a discussion or to an assessment of volcanic hazard at the site.

One thing that's important here is we also deal with a spatial dimension of particular features that might occur within these zones; dikes that might extend well outside of the zone, for example. Most experts looking at the Crater Flat area allowed for dikes to have dimensions that might be as long as 10, 20, maybe even 30 kilometers long in the subsurface that could intersect the repository. So many of the zones that are more active in these areas also led to a probability of intersection because that subsurface dike had sufficient dimensions to reach out and intersect the repository. That type of analysis has not been done before in previous volcanic hazard assessments for Yucca Mountain.

And maybe I'll, in the interest of time, just show the results here and then skip to the conclusions.

This is preliminary. It shows the first round results across the entire panel of experts. It's the annual frequency of intersection. The numbers here are $10^{-7}$, $10^{-8}$, $10^{-9}$ per year, frequency of intersection of a dike or magma with a repository volume. It's shown here as a probability density function.

And we can see that the total distribution across all experts spans over two orders of magnitude. For those who were involved in seismic hazard studies in say the eastern United States, it's not uncommon to do a vertical slice through say a particular ground motion level of .3g and to see two orders of magnitude uncertainty across that. That's the type of uncertainty that we're seeing across this expert panel.

Also shown on here are the distribution of the means of the individual experts. This would be the mean estimate for say expert--maybe that's you, Alex, I'm not sure. It's one of the experts. You can see the distribution of means across the experts panel is about order of magnitude itself. So that defines the difference, expert-to-expert difference in the mean estimates. Median estimates are comparable and, in fact, a little bit broader than the means.

But this type of uncertainty, however broad, is comparable to the types of uncertainty that we see in seismic

hazard arena.

There were some important issues that were defined with sensitivity analysis. You have that in your packet. Let me just jump to the end, make a couple of points here.

The first is that we felt that we have a complex technical issue. I might add contentious. But it's certainly complex that we've tried to address using multiple experts. The process is designed to minimize bias and promote a diversity of views. We're not looking for what's called high order consensus, which is model parameter agreement. We're looking for low order, if you will, process agreement. The process was one that was reasonable.

If along the way sources of a disagreement can be resolved because of definitional differences or other things or clarification, that's fine, but we're not seeking that.

There are multiple facilitator workshops and field trips. We try to push interaction and communication and exchange of interpretations. We think we have the range of technical views well represented on the panel, plus presenters, field trip organizers and others who are able to participate in the process. I think that was important, and I think that this occurred more than any studies I've been involved in before where others outside of the process were brought in to offer their interpretations.

Elicitations were--we had individual interviews. I think it's the best way to go, followed by feedback, and that feedback also, not only in the workshop, but we gave the individual sensitivity analyses back to them so they could see the differences of various models that they had promoted, the different effects that they would have on the total calculation.

The result, then, now incorporates a range of views, and we now have individual within expert uncertainties, as well as expert-to-expert diversity. Those two components, by the way, for those that are interested in that because it is an interesting problem for some people, two-thirds of our uncertainty was within expert uncertainty, one-third came expert-to-expert, and that's comparable to some other expert studies where this has been looked at.

The report will be the documentation. That will include not only all the procedure, everything that was followed along the way, I think as Aaron said, the who, what, where, when, why, but also the individual expert elicitations will be summarized. Those will be documents that come from the experts that they're finalizing now, and those will appended to, included in the final.

Thank you.

DR. COHON: Thank you very much, and thank you for being

respectful of our time. It's too bad we didn't have more time. It's a very interesting study.

We've decided to postpone questions for Dr. Coppersmith until after the next presentation because we are fortunate to have with us today Professor Alexander McBirney, who was a participant in this study you just heard about, so we get to hear the perspective of one of the experts involved.

After Professor McBirney's talk, we'll then entertain questions for both of them.

Professor McBirney, welcome.

PROFESSOR MCBIRNEY: I'm, it turns out, the only representative of the participating experts, one of 10 who differed very widely in backgrounds and views, and I feel a responsibility here to try to present an objective picture of our reaction as a group.

I'm not going to address directly the technical outcome of the study, but rather the mechanism by which we arrived at it. And I will also try to speak to some of the questions that I've heard raised here this morning as to the manner in which it was conducted.

And before doing that, I'd like to stress that volcanologists are a very odd group of scientists in the sense that they deal with events which are very spectacular,

of great public emotional interest, and it is a field until which very recently, we had a very weak theory by which we could explain the phenomena that we witnessed.

Much of the interpretation of volcanism until quite recently has been based on empirical observations, and the views of individual volcanologists vary widely, depending on their background and their experience. If a person has worked in a Hawaii all his career and worked out the behavior of Kilauea after witnesses numerous eruption, he will have a very, very different view from a person who has been sitting on Mt. Ranier, which he has never seen erupt. He has had to go back into the record and try to deduce what happened there in the past and how he would anticipate an eruption, when it would occur, how it would occur and so forth.

As a result, volcanologists are known to be very quarrelsome, and I have been involved in past studies where very divergent views have arisen, and there's been no resolution of these. I found this quite embarrassing to me personally.

Now, this came out very clearly at the very beginning of our study. We had a group that Dr. Coppersmith has shown you of 10 individuals representing a wide range of experiences from different organizations and different interests. And very early on, it became apparent that there

were conflicts between individuals, in some cases personal, verging on petty rivalries. I won't go into that any further.

But I wrote a letter to Dr. Coppersmith after the first meeting and said that I found this very embarrassing. I did not want to see this break down into a battle similar to ones I had seen before, and I suggested that two members, who were particularly antagonistic, be dropped from the panel.

I received an answer back saying, no, quite to the contrary, this is precisely what he was striving for, that we would hear both sides out, and in the end it turned out that I had to concede that he was absolutely right.

We did not resolve all the differences, but we certainly came to a reasonable resolution of those differences, and in the end I would say it really did not affect the basic conclusion of the panel.

During the course of the workshops and field trips, we were provided with all manner of information. To my knowledge, nothing was withheld, or nothing was presented in a way that would cause us to give it more weight than anything else.

Of course, you can always wish that you had more information, particularly in something which is as inexact as

this question we're dealing with here.

Example: The magnetic anomalies in the southern end of the field are interpreted as bodies. It was a question of whether these were cinder cones that had been eroded and covered with alluvium, or whether they were intrusions that simply didn't reach the surface. In some cases, their identity was uncertain. We didn't know how old they were. And many of us thought it was absurd that we could not get these things drilled.

I remember expressing the view that they were spending more money bringing me to one of these meetings than it would cost to drill a hole out there and find out what this anomaly was caused by.

Why these were not drilled is another question which is beyond my role to judge, but in the end it turned out that this, in my view at least, was an asset. It seems strange to say that having a lack of information is actually a help, but in this case I think it was because we had an extreme range of interpretation of these five bodies. Some people considered them one event, others five separate events, and we had every possible interpretation. And when you put this all in the analysis, it turned out that it really didn't make much difference which way it went.

That may speak to the way we interpret it, but in

any case--I'll come back to this later--I think that the information was not as critical as we thought it was earlier on in the study.

Now, we were taken into the field and shown the local geology. Not many of us had ever been there before, so it was a new experience in most cases, and many of us approached the area with the background of having seen similar things elsewhere. And this previous experience came out very quickly, most dramatically in the interpretation of Lathrop Wells.

Now, like everyone, volcanologists have certain rules of thumb that you fall back on. You learn to live with them, and they provide you an easy answer when you get in a corner, and rarely are you called upon to justify these things by students because they think you're wise. But in this case, it was quite different. And I think it would be hard to find a better example than Lathrop Wells, which is a beautiful cinder cone, like several thousand that are scattered across the western United States.

Now, conventionally, I, and I think almost every volcanologist considers these monogenetic cones in the sense that they are the result of one eruption of short duration, which is never repeated at that spot. It's a one-shot affair, never comes back. And I could go into the

theoretical reasoning behind that interpretation. Anyway, in our experience, we never found any exception to that rule.

Now, in the case of Lathrop Wells, we had a cone which had been studied in meticulous detail, far more than any other cone of its kind. They had gone over it literally on their hands and feet and examined every outcrop, dated every possible eruptive product by every possible means. And a great deal of information was presented to indicate that this was not monogenetic, but eruption had returned to the site, not just once, but repeatedly.

Several people had a great deal of difficulty accepting this. I know the first time I went out there, I thought this was absolutely preposterous. But after going over to the thing and listening to the information, I, for one at least, was forced to admit the possibility that there had been multiple eruptions there. Not everybody came to that conclusion, but many of us did.

Again, in the final analysis, it turned out that although the entire spectrum of opinions was represented, it did not have a great effect on the final decision or probability estimate.

To my knowledge, no information was withheld. People were allowed to present even the most outrageous interpretations, and we listened to them patiently and

politely I hope. I think one of the remarkable things, most remarkable things in my view, was the way the Geomatrix people handled this whole process. To this day, I do not know what Kevin Coppersmith's view of this thing is. There are some aspects of this thing that he understands vastly better than I do, and never once has he revealed any opinion of his own. I'm sure he has one, but he has never once indicated to me that he thought one thing was more likely than another. Absolutely impartial and objective in his handling of our elicitation.

The other thing that was remarkable is that when we were asked our opinion about each step of the volcanological hazard assessment, we were required to back it up with evidence or references or examples. I've never been subjected to such a rigorous examination of this kind since my Ph.D. orals.

One of my fears, one of my greatest fears from the very beginning and even down to the end is that our geological input would be put into a statistical model which would become unrealistic and result in an interpretation which had lost contact with the geological reality.

We were given the chance to examine the process by which our numbers would be used. We tested these against known processes where we knew what the outcome was. We were

able in this way to sort out the different methods, and I felt in the end that we resolved that problem very well.

Finally, the outcome. Unlike other studies I've been involved in, there was a general convergence, as Kevin showed you, on the probabilities; not on all individual aspects of it, but on the overall summation. And I think that all the members concluded that the process was remarkably well handled. I cannot think of any way that I could have improved it.

I'm not saying that more data wouldn't change our interpretation somewhat, but I don't think it would change it materially.

Thank you.

DR. COHON: Thank you so much, Professor.

Questions for Dr. Coppersmith and Professor McBirney, or both, or either?

Don Langmuir?

DR. LANGMUIR: I have a generic question, which perhaps goes to Kevin Coppersmith rather than to Professor McBirney, but I'd be interested in both.

A lot of us, as consultants, have been experts in court cases, and my perception of how those experts in that environment perform and succeed is I suspect it is completely different than this for a good reason, and it's very positive

that it is. But in that instance, it's the persuasiveness in the appearance and verbal skill, the articulateness of the arguments that are made by the individual that decides which side of the arguments is accepted. It's not necessarily the scientific defensibility of the arguments.

I've seen this happen many times. A person who's not very persuasive, but he's right, will often lose in a court case, which is where this is all headed. This is a wonderfully academic approach that you've got here, and I commend you for it. I think it's marvelous that one of you doesn't know what the other one thinks because such a good job was done of conducting the exercise.

But I do wonder still whether there was anything in this that came through where clearly someone was more persuasive among the 10 experts, if it was 10, more articulate than someone else, and that there was a sense that because of that, their arguments came through and ultimately swayed more than others.

DR. COPPERSMITH: I think that, you know, this is part of the standard issue of group dynamics; dominant personalities, issues of people being able to communicate well, others who aren't verbal who tend to--but they internalize and be wonderful experts.

We had the full range. We had those who didn't

stop talking, people like me. We had some like George Walker, who will sit there, and you think he's dozing, and then come up and make a talk that is superb and incorporates all the comments made previously.

I think that overall wasn't a problem. I think that many of the experts knew each other and know the reputations and stature that they have. I think there's respect even for those that were less articulate verbally. And probably the best example is George Walker, who speaks so quietly that we always had to amplify him with a microphone. And, but when he spoke, people really listened.

Again, this is a common issue and one that we were aware of and tried to avoid that sort of group dynamic problem.

DR. COHON: This is Jerry Cohon from the Board. I think what this underscores and what Professor McBirney's comments also emphasize is the importance of the management of the process, that if you don't have someone like Dr. Coppersmith who knows how to handle the group and to make sure that opinions of all get expressed, even those who speak too quietly or are not very forceful, the process may very well fail.

We've all been on national research counsel committees, for example, where I happen to think that it's

often the case that the success or failure of the study is very much determined by the chair and the effectiveness of the chair in managing the process. I think it's another example of it. The unfortunate thing is we're dealing with humans.

Clarence Allen?

DR. ALLEN: Just to follow up on that, it seems to me the difference between a national research counsel committee and this operation is that your objective is really to come up with a consensus. In fact, a minority report removes a lot of strength from the total conclusion. As I understand this operation, we're not looking necessarily for a consensus. We're just looking for the weight of opinion, whatever disagreement exists.

DR. COPPERSMITH: Yes, I think that's an important consideration. On a probabilistic result like this or any aspect or say some input to the total system performance assessment, you want a characterization of the uncertainty in that parameter just to make it easy; say it's groundwater flux rate or something.

All you're trying to do is to get that uncertainty distribution, and to get that, you know that different experts might have different views of the world, that they may disagree with each other. They may have their own

uncertainties. You put that together into the probability distribution. That's the focus. That's what you're trying to get at. So there's no need for agreement.

If you have two experts that are very divergent in their mean estimate, let's say, and don't even overlap on the tails of their distribution, after this interaction and the communication and they still are there, they're still there. What that may mean is that there's a problem, it's highly uncertain. And if you need to have more data or some other ways, that's fine. But we tried from the beginning to say that we're not going for agreement. There's no need for agreement.

DR. COHON: Don Langmuir?

DR. LANGMUIR: In the previous couple talks back, Steve Frishman did not appreciate and complained, objected to the way this committee was constituted and operated. And I'd like his reaction now to what was just said. Can he be specific about what he objects to in terms of how the committee was constituted and operated?

MR. FRISHMAN: It could have been possible to have a broader range of views on the panel, and it's not the fault of the conveners of the panel. It is a product of the controversial nature of this whole program and the concern--there are people who were this not a contentious program

would have been on the panel, and there may well have been a broader view of the volcanic hazard based on different interpretations of the same information, maybe based on the need to get some new information.

So I think I tried to say that I don't have problems with the panel members. I don't have problems with the way the panel was operated. The problem is a much bigger one, and it has to do with the nature of this program and decisions that have to be made, unfortunately, that sometimes even are anti-intellectual decisions.

DR. LANGMUIR: Do you think that the conclusions would have differed, if, for example, there had been more people from Nevada with Nevada's views on the panel?

MR. FRISHMAN: I think the range of dots may have been different.

DR. COHON: Professor McBirney, did you want to--I don't mean to make you respond. It looked like you wanted to. I didn't want to miss the opportunity.

PROFESSOR MCBIRNEY: I think I know the volcanological community pretty well, and I cannot at the moment think of anybody that could have been brought in that would have contributed a different view from those represented by the panel.

You're correct, you could probably get people who

would give more interpretations out of the extremes of what we had. I found that in the end, the question really boiled down to a rather simple geological relationship, and that is what is the nature, structural nature, of these eastern boundary of Crater Flat? So whether Lathrop Wells erupted once or 10 times, whether Buckboard Mesa was part of the same geochemical suite of magmas and so on, turned out to be pretty much irrelevant.

The big question is what is happening between Crater Flat and Yucca Mountain? What is the structure there?

MR. FRISHMAN: And there is a diversity of views on that.

PROFESSOR MCBIRNEY: Yes, and that didn't come out until we hashed the thing out and had gone through several meetings and finally saw the geophysical interpretations and so forth.

So I think the largest degree of uncertainty in the probability is based on the interpretation of that one basic structural feature.

MR. FRISHMAN: And there are people who were not on the panel who have diverging views on that.

PROFESSOR MCBIRNEY: Oh, I don't see that could be any more diverse than what we had.

DR. COHON: Well, thank you.

I think Leon Reiter--oh, Leon and then--

DR. REITER: I have a question for Kevin. I think Tom Bjerstedt and you mentioned the SSHAC project, and I gather this was some sort of a model for that. I wonder if you just could spend a short time, just tell me what it is and who's behind it, and could this have any bearing or helping DOE in looking at some of these issues of how to deal with expert judgment?

DR. COPPERSMITH: You have a couple of view graphs on SSHAC, just because it's very timely. There was some warning. Leon told me that he may ask about SSHAC, so I brought a copy of the cover of the report. It is a new reg now. It's called Recommendation Probabilistic Seismic Hazard Analysis, Guidance on Uncertainty and Use of Experts.

SSHAC comes from the Senior Seismic Hazard Analysis Committee. This is a two-year study sponsored by DOE, NRC and EPRI, designed specifically to provide guidance on the use of experts, on uncertainty treatment for seismic hazard analysis. About two-thirds of the report deals with uncertainty treatment and experts, and it doesn't matter if it's seismic or not.

So from our point of view, members of the SSHAC committee, it was an opportunity to finally put down in writing detailed guidance for people carrying out these types of studies. And the spirit of--I think in general, I could

say the spirit of SSHAC, if not the letter of the conclusions is what we tried to follow in the PVHA.

A couple of issues that are important on the SSHAC study that I wanted to point out relates to some of the comments regarding the nature of these studies, these overall large expert judgment studies. One of the things we tried to focus on in SSHAC was the fact that they don't need to all be large time-consuming expensive studies. In fact, in the seismic hazard field, small studies are done all the time. Dozens and hundreds of studies are done for seismic hazard work, for designs of high rises in San Francisco and New York, Boston and so on.

Those are done usually with very small budgets, individual companies or individuals carrying them out, up to large, full-blown, multi-expert type studies. And we tried--I brought just one table. This is a real focus of the SSHAC, was to deal with the level that's required for a particular study. There are technical issues that might be non-controversial, insignificant to the results, A over on the left side, up to high contentious, very significant and highly complex technical issues.

The decisions that go into whether or not you're going to address those and how you're going to address them has to do with things like regulatory concern, the resources,

how much money you have available and the public issues, regulatory or otherwise.

What we came up with and described in some detail in the report are studied levels. I won't get into what a TI and TFI mean, but in general we deal with very simple studies carried out by a single group or a single individual up to full-blown, multi-expert studies. The goal in my mind and in the SSHAC's mind is the same in all of them, is to capture the total range of uncertainty of what we call the informed technical community would have on this issue. That could be one person sitting in his office and spending two days on it, trying to get his best expression of the total uncertainty, or it could be actually pulling in experts that represent the range in diversity of views.

What we're talking about, I think I'd put the PVHA in Study Level 4, but I think it's possible, and this group, DOE, NRC and EPRI, feel it's in many cases much more realistic and perfectly appropriate to have other levels to capture that uncertainty distribution. I think in keeping with what NRC said earlier, documentation is required in all of these, even in the simplest. We need to have documentation so that you can understand the thought process that went on.

DR. COHON: Thank you. Last question from Pat Domenico.

DR. DOMENICO: To any of you, the problem of volcanic hazards appears to be deterministically indeterminate. I have two questions.

Are these the kinds of problems that an expert witness should be restricted to or best suited for, and if you didn't--if you didn't do it this way, how else could you have approached this problem? Is there another way? So that's two questions there.

DR. COPPERSMITH: Well, gee, I'm not sure how an expert witness would deal with this type of thing. I don't understand the legal process.

DR. COHON: Excuse me. You meant expert judgment.

DR. COPPERSMITH: I'm sorry. Okay, judgment.

DR. COHON: In other words, what you did.

DR. COPPERSMITH: Yes, I said--rephrase it first.

DR. DOMENICO: Are these the kinds of problems that expert judgment are best suited for? Are they restricted to these kind of problems, or if you didn't do it this way, is there any other way to get at this problem?

DR. COPPERSMITH: These are the types of problems that I'm used to dealing with, that are specific technical issues, and the requirement and the need is uncertainty characterization of that particular technical issue, whether or not it's the frequency of floods, the frequency of other

types of natural hazards, or other uncertainties let's say in a groundwater contamination plume, the probability that exists at various locations or has a certain rate of movement.

Those are technical issues about which uncertainty exists, and the level of treatment of that uncertainty or how well you quantify it is often a function of the program, the project needs.

And so I think this was a case where it was an opportunity. There have been a lot of work; over a decade of data had been gathered. There continued to be a level of contention and uncertainty about the issue. To me, it's perfectly suited for a multi-expert type study.

DR. COHON: Thank you.

Don Langmuir? Really the last question.

DR. LANGMUIR: Yeah, really. I'm intrigued by this. To the extent that we can persuade people that this is the way it all should be done that one should characterize uncertainty, how about you working on the total TSPA for the site, prepare it toward the licensing? How would you feel about something like that?

DR. COPPERSMITH: Well, I've talked to Bob Andrews. To me, I think all these components are potential inputs to the TSPA. They are all at what I think Bob calls process level

model, components of the TSPA.

Since the performance assessment, like any risk analysis, is probabilistic, it has to have a good representation of uncertainty. All the modeling uncertainty and the parameter uncertainty along the way is where the science is. And unless that's in there, then you have a product that has no utility, and it has no credibility at the end.

So the degree to which this is the process of getting science into it, into the PA, it's a valuable way to go.

DR. COHON: Thank you, Dr. Coppersmith, and thank you, Professor McBirney.

We turn back now to Aaron DeWispelare, who will now speak about an expert catchment application with climate change.

DR. DEWISPELARE: Thank you, and good morning again. Again, my pleasure to make a second presentation, this one on the expert judgment elicitation on future climate, which was carried out a couple of years ago, as was mentioned, under the NRC project called Iterative Performance Assessment Phase 2.5.

As with the two previous presentations, I will focus primarily on the process. I do have a number of

back-up slides with the climate results. If that's desired, we can go into that after my presentation.

I will skip over this slide, we've talked about the motivation for NRC's interest in expert elicitation a couple of times already this morning, and go right to the high-level objectives for this particular study by the NRC.

Number one was to acquire expertise for the staff in the expert elicitation process to do a couple of things; to aid in the review of DOE's use of expert elicitation, and then to contribute to development of the guidance that was briefed earlier.

Secondarily, it was going to be an attempt at a real problem associated with the repository, not a hypothetical case, to maximize the experiences from it; to investigate techniques for aggregation; and finally, to produce information which could be of potential use for following iterations of performance assessment by the NRC.

In this presentation, I'll cover a couple of the same areas that Kevin Coppersmith covered in his presentation and a couple of different areas I'll stress.

Climate was picked as the focus of this expert elicitation for the reasons cited here. It seems to satisfy those which make it a good candidate for a potential use of expert elicitation, and that is that the current state of

climate, science and modeling provide only limited sub-regional, long-term projections. The residual uncertainties, therefore, associated with climate prediction are large and because of the infiltration coupling can have a significant impact on repository performance.

Additionally, there are many conceptual approaches existing to climate prediction, ranging from general circulation models to paleo characterization. And in the published record, we have various of those estimates, which some have been characterized as conservative, and that needed to be investigated.

So the typical kinds of projects are very similar to what Kevin Coppersmith had just said, where you need to-- you have significant uncertainty. Science can go only so far, and that makes it, perhaps, a candidate for expert elicitation.

These are the steps that we used in carrying out the elicitation. Basically, these are those that are included also in the guidance that was briefed earlier this morning. A couple of them have been combined for convenience and brevity. I won't spend time on all of those. I've talked a little bit about some of this already this morning. I would like to spend time on three or four of them, though, and talk about our experiences.

This was the elicitation team. In a second, I'll talk about how we chose the subject matter experts, the climatologists.

DR. ALLEN: They look like geologists.

DR. DEWISPELARE: Pardon?

DR. ALLEN: They look like geologists.

DR. DEWISPELARE: Yeah, that's a motley crew all right over there. They look like something. I'm not sure what.

We started the project. Dan Fehringer was the project officer. He very soon afterwards went over to the NWTRB staff, and Jim Park took his place. I and two other normative experts were involved with the organization and then the elicitation. Our generalists were represented by climatologists and performance assessment folks, and there are subject matter experts.

This is a picture of the whole group on a site visit that we had in concert with the elicitation.

We were looking for as defensible of an expert selection process as we thought we could. We thought we'd go as far as we could to find out what is feasible, and we started out by writing a letter to these professional societies, which have membership, including climatologists, described the problem and asked them to search their data banks for members that they felt would be able to address the

problem.

I'm quite surprised with the response we got from some of these organizations. Their staffs called back and asked for additional information, and they did a very good job, I thought, of screening their membership.

We received about 175 responses back, but because many of the members, if you will, belong to more than one organization, we received the same name from many of the same organizations. Ended up with about 42 nominations.

We wrote back to them, asked them for summary resumes, described the problem to them, asked them if they would be interested in working on it. Received responses back, and went through a screening process where we looked at time constraints. Did they have the time available to work at the problem in the scheduled period? Did they meet NRC's conflict of interest requirements? And then was there a variety of perspectives represented in the set of nominations that we got back from the climate area.

Following that, we had 26 nominations that did pass that screening. We did something a little bit unusual. We wrote back to each of the 26. We had summary resumes included, and we asked them under an air of confidentiality if they would rank the members of the 26 relative to those who they felt would do the best job in addressing this

problem.

We received responses back from nearly everyone. We correlated those responses and found that eight of the group were consistently ranked ahead of all the rest. The coefficient determination was about .95 for that set of eight, not that they were necessarily always ranked one, two, three, four, but in a couple of cases they were, two that were consistently ranked in the top three. But that group of eight were together. And then following that group of eight, the correlation fell way off, as they were spread out quite a bit.

We wrote back to those eight, contacted them and tried to get a commitment as to whether they could participate. Two of them declined, one for personal reasons, one for health reasons. That left us with six. We still had, we felt, a wide variety of perspectives represented relative to the climate area, so we picked the first five out of the remaining six, and had the sixth as the back-up member.

As was mentioned in our process, we also got the group together, defined the problem, presented some data to them, and eventually put together a set of information associated with the different perspectives. That was given to each member.

We also had them comment on an issue statement that was what we felt fairly well polished by the time they got it. It turned out we were wrong, that they went back and decomposed it considerably because there were definitional issues. Even though they were all climatologists, they were very particular as to what one means by certain terms. And so we went back and fixed it so that they were on agreement as to what the quantities were and what specific things when we were asking for them, they all understood the same area.

We asked them to come back after a training exercise, which I'll talk about in a little bit, ready to make their elicitations with their bases documented, their rational that they would be using.

Some of the variables that we asked them to--we would be asking them about and we told them to be thinking about were besides precipitation and temperature, things like storm intensity or a single storm event that would cause flooding, persistent wetting as you might find in a number of year period, like a wettest decade situation, seasonal variation, summer to winter, including precipitation, incident solar radiation and so forth.

We then told them that we would be asking for them in time slices or vignettes starting at 100 years into the future and moving on out as indicated here, and because of

the time we did this, 10,000 years was as far down the horizon as we went.

In one of the workshops that we had, we additionally had elicitation training where we--even though they were very familiar with the concepts of probability, none of them had been involved in a formal process like this. None of them had ever gone through the sensitizing, if they will, of their subjective probabilities or their encoding of uncertainty in that way. And so sensitizing them, familiarizing them with cognitive biases, and then practicing with them a little bit together turned out to be very beneficial.

I'm going to say a little bit more about some of the other steps in a summary here, but for the sake of time, let me skip. I'll just talk about one result slide, and then talk about a consensus activity that we had.

The results, the five experts together said that the regional climate control of the Rain Shadow, which is as a result of the location of the Sierra Nevada Range, is going to be the dominant control of climate in the Yucca Mountain region for the next 10,000 years.

The global control of the anthropogenic effects of the $CO_2$ in the atmosphere caused by fossil fuel burning would be the secondary control in the near term, which they termed

the near term 100 to 1,000 years. This would cause an average annual warming by as much as 20 per cent and average annual wetting at the site by 20 per cent. Again, that's not very much moisture; 20 per cent is a little over an inch. But that's the result.

Then in the far term, 5,000 to 10,000 years, the secondary control would be the global control called the Milankovitch Cycles or the minor orbital variation in the earth, which does control the amount of incident solar radiation and has been credited with the onset of glacial periods, for instance. During that period, there would be a gradual cooling by 20 per cent on average, and a wetting at the site by as much as 100 per cent. But again, we're still talking--classifying it still as a semi-arid environment, increasing annual precipitation from six to twelve inches.

The seasonal characteristics they said would continue; that is, the periods of summer, spring, fall and so forth would remain about the same during this period, most precipitation in the winter.

There was some disagreement among the experts on the time in between, what they termed the far term and near term, as to what the climate would be. And, in fact, the confidence intervals associated with these estimates were larger between 1,000 and 5,000 years than they were after

5,000 years, which is a little bit confusing to us at first, until the fundamental reason for that was that no one knows how fast the atmosphere is going to remediate itself, how fast the oceans can clean the carbon out of the air, and, therefore, the effect of the greenhouse warming was of some contention there. So there would be a transition period between the near term and the far term, and how fast and smooth that transition was, was of--

DR. LANGMUIR: Langmuir, Board. Before you move the overhead, a point of clarification. You verbalized to 10,000 years. I'm reading 100,000 twice on that document, on the overhead. Do you really mean 100,000?

DR. DEWISPELARE: No, I'm sorry, that really is 10,000.

DR. LANGMUIR: That is 10,000? Excuse me, I can't count zeros.

DR. DEWISPELARE: Let me now move on to some of the observations that we had.

The individual elicitations we felt were successful in that they were consistent and complete. We were able to get all the information that we needed in terms of the variables that we set out after, and we did it in a manner that we were able to stay affixed to the agreed-upon definitions and parameters and so forth.

We did have a consensus to try behaviorally

aggregating. We spent two days and found that up to this point, this group of five people, which had been very amiable, ate lunch together, had done normal things that human beings do together, this was a time when they dug in their heels and said, no, we have substantial differences here, and we don't see any more.

So I guess in Kevin's group of consensus, as we'll say here, further on down the line here, the second one here, that they indicated that they were very pleased with the process. They were very impressed with the rigor and the effectiveness, but that they all only would agree that their distributions were included in the overall distribution set. We had two in particular. One was a--we call him our dry guy, and one we called our wet guy. There was one individual who thought there would be very little change, and, in fact, a slight drying, and the other individual thought there would be a considerable change in wetness.

And those were two of the polarizing, and the three were really in the middle of that group. And after two days, we threw in the towel and said, we've tried everything and we couldn't get any more consensus. And so the elicitation team did a mechanical aggregation of the results.

The participants all indicated that their trip to the site was very valuable. They were able to personalize.

Even though some were familiar with the climate and weather patterns in the southwest part of the United States, they were able to personalize the vegetation. They were able to see the relative humidity changes as they went to Lee Canyon and some of the surrounding areas there, and they were able to talk to the meteorologists and climatologists at the site and gain some additional information.

And lastly, although a variety of data, modeling techniques and simulations were used, the expert said that the results of the other slide, that it would still be categorized as semi-arid over the next 10,000 years.

And let me conclude with some lessons learned. We felt that the quality of the resulting judgments is strongly dependent on the conduct and consistency of the elicitations, and to do that, you need really a full compliment of the generalists and the normative experts available there to make sure that the questions are being interpreted and asked because this is a jargon, a language situation, and occasionally, you can diverge from what you feel is a fairly straightforward process, and having that set of group of people there saying, hey, time out, I think we've got a misunderstanding here without realizing it. Very important, and, of course, the expertise is a subject matter. Experts cannot be discounted.

We feel a defensible process for the selection of subject matter experts is feasible. We've used a variation of the technique that I've talked about three additional times since then for forming peer review groups.

Training of the subject matter experts is essential. None of them had, in our case, gone through something like this, and so they needed to be really educated as to what we were going to be asking them, and sensitized to the cognitive biases that are possible.

In our case, the mechanical aggregation was easier to implement, since we would have been there an awfully long time, the other way.

A site visit is valuable, and I think the comments that member Allen there made is we backed it up.

And finally, individual documentation is critical, getting it all down, feeding it back to them, as we heard earlier by Kevin, is paramount to the overall success of the process.

And that's all I have. Questions?

DR. COHON: Thank you. Questions, comments?

With regard to that point at which you arrived where there was this real disagreement among the five, I wonder if that would have been--if you think that that situation would have improved had you had more experts? It

seems to be a panel of 10 in that regard; that is, the probability of coming to loggerheads. A panel of 10 may be better than a panel of five.

DR. DEWISPELARE: For which particular problem?

DR. COHON: The fact that you now brought the five together, and you said, all right, we got differences here, let's try to work them out, and they would not.

DR. DEWISPELARE: Right.

DR. COHON: And I just wonder if a larger group would have a lower probability of coming to that kind of impasse than a smaller group. Do you have any experience with that?

DR. DEWISPELARE: I don't know that I've seen in the studies that anyone has any data on that, and I've seen studies on how many individual experts from a complex area like this will start to not add any independent information. As your group gets to a certain point, you basically covered --if you started out with some diversity, you basically covered the front. But I've not seen anybody--I don't know, Kevin, if you have, that has commented on the dynamics involved with a larger group. Typically, it's not any easier than with a smaller group.

DR. COHON: I'd be curious to know why you chose 10?

DR. COPPERSMITH: This is Kevin Coppersmith.

I think it is, for those studies that Aaron is

talking about where people have looked at how few experts can you have for a particular issue, obviously it's important what the issue is. Where it's been looked at in some detail, that normally there's sort of a break point at about five experts, and then getting to seven, and then going from seven to ten, or ten to fifteen is sort of asymptotic. That's for many technical issues that have been looked at.

The goal here, and I think what he was talking about in terms of head banging for two days and a consensus building, was they were trying to get at an aggregation that is behavioral, one that gets people to basically agree with an overall probability distribution.

That, what I would charge, and I think what they did is by allowing interaction of the experts along the way through workshops, field trips and other opportunities, there was a lot of behavioral aggregation that went on through the course of the study. And our argument will be, and aggregation for PVHA is that, in fact, most of it was behavioral. Even though we mechanically give them equal weight at the end, the interaction led to a behavioral aggregation.

But an impasse like that I think is not a function of size. It's a function of trying to force a behavioral consensus when you can't get it. That's the nature of the

problem.

DR. COHON: Professor McBirney?

PROFESSOR MCBIRNEY: The only thing that I can think of would be a possibility of getting people that are not from this country. I find it that people in the United States who have gone to similar schools, had similar associations, tend to think a lot alike. And when I got to meetings with Europeans, I find their way of reasoning is often very, very different. Not that one is better than the other, but in my field, some of the best research is being done in France. And their approach to many problems is quite different from ours, and they sometimes arrive at very different solutions.

And I understand that we were not able to include people from foreign countries, but it might have helped to have somebody from another country.

DR. COHON: Interesting. Other questions?

Thank you very much.

We turn now to another application of expert judgment. In this case it's the preparation of the 1995 Total System Performance Assessment. Dr. Robert Andrews from the Intera, the M & O, on TSPA will make the presentation.

DR. ANDREWS: Yeah, I think we've been hearing a lot this morning about formal expert judgment, and what you're going to be hearing for the next 15 minutes is a lot of

informal expert judgment, and in particular, that aspect's used in the last iteration of total system performance.

Just because it's informal doesn't mean it doesn't have a basis. There's a strong basis for a lot of the judgments used. We try to, in the documentation and in the presentation we gave to the Board in October, to explicitly indicate what the bases for those assumptions were and allow the reader or the listener to make his or her own impressions on whether that technical basis is sound or not.

Leon, in setting up this agenda, wanted me to hit on a few--the appropriateness, I guess, or our beliefs of the appropriateness of some of the assumptions of judgments--I'll use assumptions and judgments interchangeably in this presentation--that we used in the last iteration of TSPA. And I think I would say that all the judgments used or all the assumptions made are adequate for the intended purposes of this iteration, which were to evaluate the sensitivity of how this site, then associated engineered systems might be expected to perform, but virtually every one of them could be improved. And I'll hit on a few of the big ticket items and show where they could be improved.

This is the outline. I'll walk through a little bit of philosophy of the use of judgment within performance assessment in general and as it's implemented in total system

performance assessment, talk a little bit about evolution of judgment. I think it's important to point out as time goes on, information bases improve, and the ability to get to more process level understanding is enhanced. Also, additional uncertainties are raised when you start going more and more into the process level understanding.

We'll look at examples from TSPA and with some concluding remarks.

Firstoff, there is definitely a need for judgment. Virtually, we debated at one point within the TSPA document, putting a TBV, like the designers do, every place where we were raising a judgment or an assumption within the analyses. And we realized that we were quickly going to get swamped with TBVs in the document and double the size of the document, which was already voluminous. So we decided not to take that approach, but to clearly indicate, hopefully clearly indicate, where are those assumptions, what are the bases for those assumptions and judgments used when you have to make an assumption?

But firstoff, and I think Kevin alluded to this, is we try to, to the extent possible, base all of the judgments, all of the assumptions, on some process level understanding. That can be a process level model, as it is in the unsaturated zone, hydrology, if you will, which in itself is

based on observations or data.

So we try to build it upon a foundation of these processes and understanding that are embedded in these processes.

However, I think we all have to face the fact with, and the volcanic hazard has addressed this, and climate change clearly addresses this, is that the conceptual models at the base of the pyramid, if you will, and the parameters embedded in those conceptual models are all uncertain, and they are variable. And so what performance assessment attempts to do is account for that uncertainty for that spatial and temporal variability in its assessment, and ultimately to evaluate did that uncertainty or that spatial variability make a difference in the predicted outcome, i.e., total system performance.

And we looked at various measures of total system performance because we don't have a regulatory guide right now for what is the appropriate total system performance measured.

So we have to make assumptions, and we do make assumptions, and we're not shy about making those assumptions. Every step, which models to incorporate in the analyses, which models to not incorporate in the analyses, an example being how do you incorporate fully coupled processes,

thermohydrologic, mechanical, chemical, when you don't have process level models that fully couple all those processes. What parameter ranges to use, you know, and how are they distributed? Are the uniform? Are they a nice PDF, like Kevin showed for the recurrence interval of probability of intersection, you know, nice mean in the standard deviation, or are they some, you know, log uniform type distribution?

An important key aspect that we have to face up with is how do you incorporate the fact that there's spatial variability in this system, that we have the possibility of 10,000 plus or minus packages. That's 10,000 locations, 10,000 points in the drifts, if you will. There is variability from point to point within those packages and variability from point to point on each package. How do we incorporate that variability into the analyses is an issue that requires judgment, that has to be documented.

We have this one caveat, is that we try to, to the extent possible anyway, make those judgments as reasonable as possible, so i.e., based on information or observations, and in the absence of those information or observations, make them as conservative as possible. But in all cases, acknowledge that uncertainty and variability and evaluate in a series of sensitivity studies the significance of that uncertainty. And at the end of the day, prioritize the

information needs to reduce that uncertainty, and that information needs is two parts to that equation. Firstoff, is the significance, did it make a difference or not? And secondly is what is the ultimate or the range of uncertainty in that process or model or parameter to begin with.

I don't want to get buried too much in detail here, but just to weave a little story here that understanding improves with time, and the details that you can incorporate with time improve.

The Board has heard all three iterations of Total System Performance Assessment, the earlier one done in '91, reported to the Board, I think, in '92; in '93, reported to the Board in '94; and then you heard in October the results and conclusions of TSPA-1995.

If I just look at waste package degradation, in TSPA-1991, there was just a flat out assumption. Started failing at 300 years, and they had all failed by 5,000 years. Just judgment, total judgment. At that time, the definition of failure meant all containment was lost, i.e., the package had crumbled away.

In '93, we realized that was not a very good assumption. It might be better to base it on some observational pieces of information. So we had a temperature-dependent aqueous corrosion model of what was at

that time a two-layer package. In '91, it was only the SCP package, which was a single, thin-shelled layer of corrosion-resistent material, and this one had two layers, corrosion-allowance and corrosion-resistent, and we base it on temperature-dependent aqueous corrosion. So it had to get wet, i.e., you know, humidity is being thrown out. This was in '93. Some of the analyses were done in draft and some of the analyses were done in bore hole. So you had both types of emplacement options being addressed.

The temperatures used in that temperature-dependent calculation were derived from essentially repository scale, thermohydrologic models, and it was only temperature-dependent, no humidity or saturation dependency.

Failure at that time was still meant to imply once the first pit is gone through, the entire containment has been lost.

In '95, we went into a lot of detail on this in October, but essentially now, all the packages are in the drift, and we said it's both temperature and humidity-dependent, and there are data, which we showed you at that time. And I have one more plot here of that dependency of corrosion--of the corrosion-allowance material. We threw in the possibility of there being cathodic protection of the inner barrier. We'll talk more about that later, as an area

where it's an assumption. Its sensitivity was evaluated in TSPA-95. Recommendations are made based on that sensitivity, and we intend to improve that in subsequent TSPA iterations.

Also, important to point out in both of these cases, or in '95 anyway, that the variability from package to package and from pit to pit, localized corrosion to localized corrosion on a package was explicitly treated for. So you have a distribution of failures and a distribution of what is meant by failure, i.e., the first pit does not mean the package has completely crumbled away.

Looking at evolution within drift-scale, and I just picked a few here, drift-scale, flow and transport. In '91, it essentially assumed that whatever flux there was at the repository horizon went through the drift and went--well, of course, there wasn't a drift in '91. It was just in bore hole, but went through the package once the package had failed.

In '93, it was a little more mechanistic, if you will, saying that there is some relationship between flux and the saturated connectivity of the Topopah Spring. If it exceeded that, you had the possibility of there being advective flow through the package. If it was less than that, you could only have diffusive flow. Diffusion rate was dependent on--because we had no drift-scale kind of

assessments at that time, the diffusion rate was based on the water content of the rock adjacent to the drift and making it equilibrate with the in-drift materials.

In '95, we made this same assumption. Nothing had improved, if you will, from '93 to '95 to enhance that assumption. However, the diffusive release now based on in-drift saturations derived from, in fact, two different thermohydrologic models. Those two different thermohydrologic models primarily differ in their thermal conductivity of the in-drift materials, but they gave very different results in terms of in-drift humidities, in-drift temperatures and in-drift saturation.

So both of those are used in a sensitivity study. We don't know now which one's right; is it A or B, or some range between A and B? But we look at those two and evaluate the significance of them, which I think is the main goal or one of the main goals of performance assessment in general, is that given these alternatives are out there and we have ranges of models, we don't necessarily try to define the probability of each of those models. But if we say if it's Model A, here would be the consequences. If it was Model B, here would be the consequences. Did that make a difference? If so, then that tells the site program or the engineering program, you better focus additional resources on

distinguishing whether Model A or Model B are more correct.

We could have easily said there's a full spectrum of models and gotten the PDF from 10 experts, and maybe in some cases that will be a useful exercise, but that was not done for this iteration. It's like taking points and evaluating the consequences of that point.

UZ aqueous transport, in '91 it was all matrix dominated. In '93, there were two end members evaluated in one case where it was matrix or fracture dominated. But it had a very high matrix diffusion coefficient, so it essentially became matrix-dominated transport. And the second model, which was totally fracture dominated, i.e., no fracture matrix interaction. This one, the Board is well aware that the performance in this particular conceptualization then becomes dictated by the probability of a dripping feature intersecting a package. If it doesn't intersect, then there's no release; if it does intersect it, then there is release. So it becomes totally a probability of intersection issue.

In '95, we were essentially looking--we had two things really. One, the sensitivity to matrix diffusion was evaluated as part of the Calico Hills Systems Study. Those results, I think, were presented to the Board. And then we devoted a considerable amount of effort to looking at

alternative models of fracture transport and look at the degree of fracture matrix coupling and what impact that uncertainty had on total system performance.

Now, switching, and just looking at '95, and I don't want to go through the details, but let's just hit on a couple and talk about the adequacy of the judgments or assumptions used and where these could be improved, and not talk about what the significance of these assumptions are right now because those we have presented.

I start with the things that impact waste package degradation, and first start with the near field environment as represented by what happens in the drift in terms of its humidity, temperature, liquid content or saturation distribution. Here we used two different models. Those two different models had two different backfill properties. Those two different backfill properties created differences, as I think Steve was alluding to, differences in relative humidity, time relationships and temperature/time relationships.

I this it was adequate that we used two different models, but clearly, we have to have some basis for choosing them or saying that they represent the full range of possible outcomes of in-drift thermohydrology. In other words, they don't exceed either one of the end members.

Jumping down here to the per cent of corrosion-allowance material degraded prior to allowing any corrosion of the corrosion-resistent material, this is the famous cathodic protection. It was adequate that we tested the impact of that particular conceptual assumption. However, we used just one number. That number was 75 per cent. That number came from one individual essentially in the waste package degradation testing community. I think in the actual write-up, we just give a personal communication to that number because there's no data, or there's probably a lot of data upon which we based that number, but those data we did not have.

So we had one number. Is that number representative? Is it representative of all packages? Probably unlikely, so perhaps that number should have been given a distribution.

To show an example of how data uncertainty are incorporated in TSPA, I show here a graph of real data outside of this project, but it's very analogous. It's humid-air corrosion of a corrosion-allowance material in a range of humid-air environments. Most of this data is from the navy, as you might imagine their interest in this issue, and we have relatively long periods of observational time.

What we did is very simple fit to the data, but

say, look, the data themselves allow me alternative interpretations as I got out to longer and longer times, or even shorter times for that matter. So we try to capture that, in this case with a normal distribution, or it might have been log normal--I don't know exactly, I'd have to look back, where I show here that plus or minus two standard deviations.

So that variability of degradation is captured in TSPA. It is captured from package to package, and it's captured from pit to pit.

Is that distribution right? I don't know. Maybe it should have been a uniform distribution or log uniform distribution, and maybe that makes a difference. And we'll look at that this year to see if it does.

Examples of judgment used in EBS area, let me not focus on any of these except the one down here, and that's a conceptual assumption of how does advective dripping, if there is such a thing, if you do exceed saturated matrix conductivity, how does that interplay within the drift and with the package? Does it drop on the package or not? Does it drip through the package or not? There's no process level modeling done at that scale of advective flow within a drift, so we made three different assumptions, and each of those assumptions we evaluated the impact of.

This is just another example of a curve. In this case, Kunkis' data on effective diffusion coefficient inside the drift. You can see the data have scatter, not surprising. You know, there's heterogeneity in materials. Could have been uncertainty in his test, whatever. We're trying to capture all of that uncertainty with the curve fitting and with the bars around that.

In the geosphere, the really principal uncertainty is the range of percolation flux at the repository horizon. I have on the next figure, the next two figures actually--I'm not sure how we're doing time wise. Okay, we're in good shape then.

At each one of these boundaries, if you will, going from the precipitation to infiltration, a range of conceptual models exist to give a range of infiltration distributions given a range of precipitation distribution, precipitation varying in time and space. It could be clearly a long time, also, as Aaron pointed out over the 10,000. In fact, we're making million-year simulations, and clearly, it's varying over there.

There's a range of conceptualizations giving a transfer function, if you will, between precipitation and infiltration.

One would argue that all of the site scale models

of the unsaturated zone flow, one of their principal outcomes is to define the correlation or the distribution of given infiltration, what kind of percolation did I have? We happened to use two different assumptions is TSPA, and, in fact, we use a third, which said it's a full breadth of all the integrated infiltration rates.

But clearly, there's process level modeling going on as we speak, and I think the Board heard some results even yesterday of some of that work, to give a percolation flux distribution through the host rock, or proposed host rock.

Given that, PA has to do two more things. One is to define that distribution spatially over my 10,000 waste packages. So that's my $q_{percs}$ here to indicate there's 10,000 of $q_{percs}$ of concern. And secondly, within the scale of drift, how is that local flux distributed? What percentage of that local flux stays in the matrix by capillary forces, and what percentage of that flux potentially advectively comes into the drift and allows advective transport through the drift, of other conceptual issues here beneath the repository and percentage of water in fractures and matrix.

So what did we do? I think the next slide is just a word slide that goes along with that picture, just so all the terms here are defined. What we do is sort of look at did it make a difference or not. So this is actual

infiltration rate, that distribution, and we're looking at million-year peak doses, predicted doses. So everything is in here now, from the humidity stuff to the waste package degradation, to the diffusion, to the retardation of certain nuclides, et cetera, et cetera, et cetera.

And we're then looking at the bottom line result, and that we see that for a particular infiltration rate, if I just knew it exactly, I'd still have a range of uncertainty because of other uncertainty in other parts of the system, whether that be the saturated dilution or whether that be degradation or neptunium solubility or what have you.

So if I just picked point one, I'd see, well, I have a--and probably I don't have enough points on here to show you the complete range, but it's going from let's say a millirem up to 100 millirems, something in that range. As I got up to higher infiltration rates, now the peak dose or predicted peak dose is increasing not surprisingly. What you're seeing here is the neptunium coming out at the higher infiltration rates, and here you're dominated by technetium and iodine coming out.

So what do we conclude? The first conclusion is that the present state of our process level understanding required a number of alternative models exist and a number of assumptions required in TSPA to evaluate the impact of those

alternative conceptual models.

Well, I kind of jumped forward here, but, so we first have these alternatives that are out there and the assumptions, therefore, required. One could have picked Model A and excluded Model B and maybe justified it, but we felt, especially at this point in the time of Total System Performance Assessment, it's better to acknowledge both of those exist, or all three or all ten exist, and evaluate the impact of those alternatives. And that's what we tried to do.

We tried to make those judgments as reasonable as possible. We tried to document them to the extent that we can so the bases for that assumption is clearly articulated, and we also tried to make them conservative.

I think in the concluding remarks of TSPA-95, we tried to say, well, where were we maybe not even conservative or not conservative enough? Where could we have been more conservative, and we identified other areas where we could have been less conservative, and we gave ranges, well, if we were outside the bounds of what we even analyzed, what would have been the consequences of it, in both directions, to be fair.

We do a number of sensitivity analyses, almost too many of them because I think become--people get glazed over

after awhile after you see too many of them, to evaluate what made a difference and what didn't make a difference. We identify the synthesis and process level modeling that's required to enhance or improve the assumptions and judgments that we had to make in TSPA-1995. This is, in fact, the bulk of what goes on in the site characterization scientific programs area. This fiscal year is a synthesis of a lot of information and process level modeling that would feed into the next iteration of Total System Performance Assessment.

But I think we also have to realize that even at the end of the day, whether that end of the day is TSPA-1995 or TSPA viability assessment or TSPA license application, there will be uncertainty. So the ability to show the impact of that uncertainty and the impact of that variability to the regulator or to the manager, who has to make a viability decision, is key and an important part of what we do in performance assessment. So we try to evaluate that significance.

And so with that, I'll stop and entertain questions.

DR. COHON: Thank you. So why does DOE say there has been no clear benefit for doing formal expert judgment in performance assessment?

DR. BJERSTEDT: This is Tom Bjerstedt. I think by the

way that Dr. Andrews described our program, we're in an iterative phase of learning as we go along, and we're also learning as the NRC goes along with their iterative performance assessments. There may come a time when we would want to do something more structured and more formal in trying to reduce the range of possibility for a compliance demonstration, but we just don't see that benefit right now, and that's why we haven't explored formal expert elicitation solely as a feed-into TSPA.

DR. COHON: Other questions? Don Langmuir?

DR. LANGMUIR: I've been very careful to count the zeros before I ask this question. But on Overhead 15, you show the infiltration rate distribution, and what it looks like, qualitatively at least, is that you've got a $10^4$ possible range of doses due to changes in infiltration, and within the distribution of data points, which presumably includes a range of assumptions about things like source term and other parts of the program, the uncertainty range appears to be more like $10^3$ orders of magnitude. Inferentially, then, infiltration rate is the biggie. I'm assuming you picked it because it's the largest player in uncertainty in terms of site performance? You're nodding your head.

DR. ANDREWS: Why did I pick this plot instead of some other plot?

DR. LANGMUIR: Well, no, that's not the exact question. But I'm looking at the data distributions, which, of course, are all model data points, and I'm inferring from this that you concluded from the TSPA that infiltration rate is the key player in the performance of the site, whatever choice we might make of it and how it might vary through time.

DR. ANDREWS: Or percolation flux.

DR. LANGMUIR: Or percolation flux, yeah.

DR. ANDREWS: Yeah.

DR. LANGMUIR: And that further, that the uncertainty in the other parameters within all of your models, if you look at the distribution of points here, is $10^2$, it's $10^2$ or $10^3$ uncertainty in the dose.

So you're telling me in effect that there's a tenfold greater importance, qualitatively at least, to infiltration and percolation flux relative to any other choices of parameters in the models. Is that qualitatively true?

DR. ANDREWS: It's qualitatively true, you know, but a PA person always adds a "but" to that.

DR. LANGMUIR: Why don't you go ahead and do that?

DR. ANDREWS: And here comes the "but." And we did this in the document by saying where we felt the conservatism because there are clearly dictated by the assumptions in all

of the other processes and models that are embedded to give these results; degradation, solubility, dissolution rates, Kd's, et cetera.

So given those other assumptions, which we try to be reasonably bounded on or reasonably conservative on, your statement is correct.

If some of those other assumptions, and there are a few key ones in there that have a big impact, but if those assumptions are wrong--neptunium solubility would be an example of that. Neptunium would have been always down here. So you would have seen a curve that looked like this.

So I want to preface my "yeah" by the "but," which is it's dictated by some of the conservative assumptions, which are based on available information that are embedded in other parts of the total system model.

DR. LANGMUIR: Well, let me bring in my doubts here as a geochemist. My suspicion is some of the largest uncertainty lies in which you're adopting as your source term inputs, and that is strongly tied to what is decided on thermal loading, and on whether there's backfill or no backfill. Those are major players, I would think, in how wide a distribution the data points you might have, even assuming that this was--even with this plot as you've drawn it. You'd have much wider uncertainties perhaps than these.

DR. ANDREWS: The backfill or no backfill doesn't make that much difference.

DR. LANGMUIR: It influences whether or not you have the transport or particulates since it--species from a waste package outward into the--

DR. ANDREWS: Oh, the colloidal transport you're talking about?

DR. LANGMUIR: Yeah, it influences the--

DR. ANDREWS: Yeah, okay.

DR. LANGMUIR: The temperature influences the survival rate of the waste package through time--

DR. ANDREWS: Yeah.

DR. LANGMUIR: --which is a function of backfill insulating it. There's a lot of things in this that--and I guess I need to read TSPA-93 to see what assumptions were made with regard to source term, but my guess is that those are biggies, really big, and whether this really represents a true distribution of uncertainty.

DR. ANDREWS: When you look at a million years, you'll find that some of those things don't become as significant as you might surmise they would be if you were looking at 10,000 or even 100,000 years.

DR. COHON: Sounds like you could use some expert judgment. Thank you.

Other questions? Pat Domenico.

DR. DOMENICO: Well, you call this informal, right, expert judgment?

DR. ANDREWS: Yeah.

DR. DOMENICO: Which is--well, let me see how that's been used here. I think you said at one time that over a million-year horizon, retardation is not important because it's going to break through any, and retardation only delays it. Have I got you correctly on that?

DR. ANDREWS: For some of the key nuclides--

DR. DOMENICO: Yes.

DR. ANDREWS: --which have--are poorly sorbing.

DR. DOMENICO: I think, and correct me if I'm wrong, you might have said that over a million years, even the whole Calico Hills is not important because they're going to break through anyway. Is that--

DR. ANDREWS: Yeah, for the range of infiltration rates and--

DR. DOMENICO: Okay. Now based on--you're the expert, and you made that judgment.

DR. ANDREWS: No, that's not a judgment. That's a result.

DR. DOMENICO: That's a model result.

DR. ANDREWS: Yeah.

DR. DOMENICO: Based on that, the program has decided that it may not be necessary to investigate the Calico Hills via the TBM. So that's how this judgment has been used somewhat in this case.

DR. ANDREWS: Well, I think it's a lot of pieces of information, you know, in addition to impacts on performance, but that was one.

DR. DOMENICO: I think that some of us aren't too happy with that decision. I think he's sitting right over there. But that is an end result of what you call informal expert judgment, I think. Just a point.

DR. ANDREWS: Well, or I would have called that the application of performance assessment. You know, the judgment that we use that goes into the assessment is what I was trying to allude to. What happens as a result of all of those judgments or assumptions is a conclusion, and we do sensitivities to evaluate the robustness of that conclusion, i.e., can I, you know, make it tick to make a different conclusion?

And in some cases, as I was trying to say with Don's question, you can make a slightly different assumption not incorporated in the current iterations where a conclusion would be different. But we also try to document that fact. I mean, you can't do--well, it's difficult to do everything.

So you try to, in the time frame you have and the resources you have, do the major ticket items, but acknowledge that some of those things are outside of what you can do in that time frame.

DR. COHON: Thank you very much.

First, a brief announcement before we come to an absolute close here. Some of you were not able to get, I believe, the earlier NRC presentation that Michael Lee was supposed to present, but Aaron DeWispelare did. More copies are being made; yes, will be available this afternoon if you want it.

Just to wrap up, as we've heard over and over, and as we all know, the world is an uncertain place and no part of it is more uncertain than the problem that we're dealing with. It's sort of fitting that we're sitting here discussing this is Las Vegas, which has made an economy out of being smarter about uncertainty than the rest of the world.

Unavoidably, expert judgment is part of doing anything related to anything technical, and again, especially in the case of this particular problem. The question of whether one gets formal about that expert judgment is a separate issue. I mean, related, but is an important question. It stands on its own. Whether one uses it is

itself a judgment, an expert judgment, and that's also not avoidable.

What we heard today, which is very encouraging for this Board, is that DOE has responded positively and constructively to what this Board has suggested in the past, and we're very pleased to hear that. We're pleased also to see that the response moves DOE in a direction that brings it closer to NRC it seems, and that's also very healthy.

We heard some valuable cases where expert judgment, formal expert judgment and informal, had been applied to some interesting problems. There are many important lessons to be learned there. Just overall, the process matters. It matters a great deal how one goes about choosing experts and using them. And the management of that process matters a great deal.

What seems clear to us, to me anyhow, is that having skilled managers of that process, and we saw some examples of that today, is really key to having a successful project.

My thanks on behalf of the Board to all of our speakers. You were excellent. We learned a great deal. We thank you.

We are recessed now until 1:30.

(Whereupon, a luncheon recess was taken.)

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

DR. ALLEN: Good afternoon. I'm Clarence Allen, a member of the Board and will be chairing this afternoon's session. This afternoon, we'll be discussing two main topics: the review of the DOE's technical basis report on

surface characteristics, preclosure hydrology, and erosion at Yucca Mountain; and the management of defense waste and surplus fissile materials. There will also be a public comment period at the end of the day and again we hope those of you who wish to make presentations will sign up with the people in the back of the room.

Presently, I would like to offer a few comments on the technical basis report and return later to that part of the session to the comments on defense waste and surplus fissile materials. Early in 1994, the DOE announced that it had formulated a new program approach to the management of civilian radioactive waste. A central part of this approach was a decision to make a determination of technical site suitability with respect to the Yucca Mountain site in 1998. This determination would be based on evaluations and whether or not the Yucca Mountain site could adequately meet the qualifying and disqualifying conditions laid out in 10 CFR 960 which is the DOE's general guideline for the recommendation of sites for nuclear waste repositories.

The plan was for the DOE to make decisions on compliance or noncompliance of the individual guidelines after the technical reports; that is the scientific and engineering information needed to assess compliance had been reviewed by panels appointed by the National Research Council

which is the operating arm of the National Academy of Sciences, the National Academy of Engineering, and the Institutes of Medicine. Hereafter, I'll refer to that simply as the NAS because of the confusion between NRC, the Nuclear Regulatory Commission, and NRC, National Research Council. So, I'll refer to it as an NAS panel, but be aware that particularly the NAE was also involved.

The first technical basis report or TBR covered several areas including surface characteristics, preclosure hydrology, and erosion; sometimes, collectively called surface processes. It was completed in April of 1995 and submitted to the NAS for review. The NAS panel completed this review and issued its findings on November 30, 1995. During the same time period, the State of Nevada also conducted its own review of the TBR.

Subsequent to the TBR's completion, it became apparent that the program approach would not be viable under funding constraints placed by Congress. It is our understanding that no additional TBRs, NAS panel reviews, or 10 CFR 960 compliance assessments, including those related to surface processes, are in the offing. The Board, however, is very interested in hearing about the results of the one review. After all, aside from the prior and related submittal to the Nuclear Regulatory Commission of the topical

report on extreme erosion, the TBR represents the first attempt by the DOE to reach resolution on any technical issue associated with Yucca Mountain. It's no great secret that the TBR, like its predecessor topical report, was not greeted, shall I say, with overwhelming praise. We are interested in knowing the good and the bad aspects of the TBR and particularly what lessons, both specific and general, there are for all involved.

We'll start off this afternoon's session with a presentation by Ernie Smerdon, Vice-Provost and Dean of the College of Engineering and Mines at the University of Arizona. He was chair of the NAS Committee that evaluated the surface processes report. He will describe the review and its conclusions. He'll be followed by Carl Johnson of the Nevada Agency for Nuclear Projects who will assess the State of Nevada's review of the TBR. And, finally, we've asked Steve Brocoum who will be on the hot seat, I guess, of the DOE to give us the DOE's perspective on the TBR, its review, and what lessons the DOE has learned.

So, Ernie, would you commence your presentation, please?

MR. SMERDON: Thank you very much, Clarence, for this opportunity to make a report of the study of the NAS of the technical basis report. This is a report that we were

charged to review, and for those of you who haven't seen the final report which was released, this is the final report which basically is entitled Review of the U.S. Department of Energy Technical Basis Report for Surface Characteristics, Preclosure Hydrology, and Erosion. I will use a number of foils in this presentation. We, too, very quickly decided that if we used the term NRC as the sponsor of this study, it would be confusing. So, we had down both the National Academy of Sciences and the National Academy of Engineering.

The committee that was involved in this study was selected through the normal process followed by the National Research Council or the Academy. This is a list of the committee members. I might point out that the committee membership had broad representation with experts in each of the areas of hydrology and certainly the surface processes, erosion. This committee, as you will notice, was a committee that was certainly heavily represented by people from academia. I might say in the process of selection of the committee, one of the things that the Academy follows rigorously is to be certain that no member of the committee has any tie-in with the project that would lead to bias. As a matter of fact, this is an important consideration not only in the initial selection, but in a confidential process to be certain that there are no members that might have bias

against one way or the other regarding this particular project. I mentioned the fact that the academic representation was quite strong because that meant that the historic approach of academia involved heavily depended upon peer review of scientific results and was certainly heavily embedded in this process.

One of the first things that this committee did was looked at the statement of tasks and the schedule and agreed whether we, as a committee, could accept the statement of a task that was in the agreement between the DOE and the National Research Council. So, that was accepted early-on. The statement of task was fairly explicit and I want to mention what we were to do and what we were not to do. If you pick up a copy of the foils that are available, it points out with bullets several of the things that we were directed to do. We were to address these as a minimum. But, the statement of task--and it's not in this foil--said the committee will review only the technical and scientific analyses. The committee will not address regulation, compliance, nor will it address the suitability of the Yucca Mountain site as a high-level radioactive waste repository.

So, I want you to keep those two constraints in mind because I might point out that in our first meeting in late July of 1995 in this very room we had an open meeting so

that we could get input from any individual or organization that wanted to make input. So, we did have quite a number of information gathering sessions, and I suppose during that first day, I had to repeat this statement of task because there was a tendency to try to get the committee involved more in more activities than the scientific and technical analyses of this report which is the charge that we had.

The questions. Have the data been collected and analyzed in a technically acceptable manner? Do the data, given the associated and error and analytical uncertainties, support the technical interpretations and conclusions? Are there credible alternative interpretations that would significantly alter the conclusions? What testing, if any, would discriminate among alternative technical interpretations? If such testing is recommended, how effective would it be in reducing significant uncertainties?

I want to say that the committee's goal was to help the DOE improve the scientific quality of the TBR. I think it's important to remember what the chairman said at the beginning at the time we started this study. We were under the impression that there would be a handful, five or six, of these reports. So, one of our points that we discussed was that we wanted to provide whatever input that we could to help the DOE in improving subsequent technical basis reports.

The committee's evaluation is based entirely on scientific judgment. Now, this morning, you heard a lot of discussion concerning expert judgments; judgments are involved. We were judging the scientific adequacy of this report. The committee made no attempt to evaluate the science in terms of management decisions. That's an important consideration related to the suitability of the site as a high-level nuclear waste repository. Also, according to the charge, the committee did not evaluate whether the identified weaknesses in the science would have a significant impact on the management decisions of the site repository.

Now, regarding the sources of information that the committee views, first of all, naturally, the technical basis report and supporting materials--and we did delve into quite a number of supporting materials that were not included in the report. These were all cited in Appendix A of our report. As I said, we also received oral and written information from the DOE and its contractors, other federal and state agencies, and members of the public. These were at two public information-gathering sessions. Also, discussion with scientists on the three day excursion which we made to the site. Now, I might point out regarding the three day excursion, I noted that some of the earlier studies that

involved field trips, those field trips were made in March or April or something like that. Our committee made its field trip to the site on August 27, 28, and 29. Now, that may give you a clue concerning the intellect of this particular committee, but it certainly did have something to do with the schedule that we had committed ourselves to follow.

I want to point out the places where we went. First of all, on Sunday, August 27, we stopped at Lathrop Wells, a couple of sites here, and then we went on to Beatty for an open session on that Sunday afternoon. And, I want to publicly again thank the people at Beatty for opening up their community center for this public meeting. The blue here, those are the first two sites that we visited on Sunday. On Monday, we visited these sites here and these are all listed in the appendix, one of the appendices of the report; I believe, it's Appendix E. But, anyway, the red circles here are the sites we visited on Monday and on up on the top of Yucca Mountain here. And then, on the following Tuesday, we visited some sites on Crater Flats and also along the western slope of Yucca Mountain itself. So, those were the sites that the team visited or committee visited during the field excursion. I might say that that was extremely important to us because, as you know, I am an engineer, but I'm convinced that geologists just will never be comfortable

assessing something unless they have been out and looked at it on-site. So, this was very important to us.

I will have an individual foil on each of these conclusions. And so, I have about eight or so foils following that regarding--but regarding our conclusions on the distributions and relative ages of surficial deposits, identification of surficial deposits is based on traditional and accepted techniques. Better age control is needed on surficial deposits to estimate erosion rates. I'll comment a little bit more on that later. Surficial data on the west side of Yucca Mountain, Crater Flat, should be integrated in the TBR. Surficial mapping efforts need to be better integrated with efforts to evaluate hillslope erosion processes.

The assumption that streams are presently in dynamic equilibrium, that assumption was not supported in the TBR. Possible effects of climate change--and, this was a fairly important one because the issue of climate change was not covered in the TBR to any extent. But, anyway, we think that the possible effects of climate change on fluvial erosion should be addressed. The effectiveness of debris flows and landslides as erosive agents of the landscape under present and possible future climatic conditions should be addressed.

Conclusions on the ages of hillslope deposits. The analyses of hillslope ages are inadequate because they're based on a single geochronological method, the cation ratio dating, and are applied at only one type of hillslope deposit or principally to that type, the heavily varnished hillslope deposits. Different dating techniques should be applied as a check and different geomorphic surfaces should be dated to obtain estimates of the spatial variability of hillslope ages.

Regarding the conclusions on long-term rates of erosion, we believe that the analyses of erosion rates is too narrowly focused on estimating averages, spatial and temporal averages for a comparison with the regulatory standard. The analyses should be expanded to assess the spatial variability of erosion and especially to identify those areas of the landscape that may be eroding much faster than the average. This is an important consideration. The analysis should consider the range of erosion processes operating at the site and again the possible effects of climatic change.

Regarding conclusions on the potential for surface flooding, we believe that the process there followed accepted engineering practices and the values and assumptions that were used appeared to provide conservative estimates. By that, I mean appeared to provide overestimates of the maximum

flooding depths that might occur. Nonetheless, we make the comment that work on the sensitivity of changes in these assumptions in terms of the flooding depth, it would have been helpful to have had that in the report.

Now, I want to make a comment here that one of the things that we were highly sensitized to is the fact that any document like this, it's a scientific and technical document, but it's also a document that has a great deal of emotional factors involved in it because this is such a sensitive issue. So, we felt that in the future TBRs it would be helpful to consider better explaining the implications of ranges. We came in our discussions to the notion that in many of these issues it was very important to look at different hypotheses to come up with what we term bounding values to show what the range of results might be at some future time under different sets of assumptions.

But, in any event, back to this. The subsurface flooding potential from deep seepage on surface infiltration and rising water tables should be addressed. The distribution, volume, and age of perched water in our judgment could be better addressed. It doesn't appear to the committee that the perched water will pose problems during the construction and operation of the repository, but we felt that case was not made as well as it might have been made in

this report.

Conclusions regarding water supply. There's quite a lot of information on the water supply, but the TBR lacked a clear statement of the technical questions that must be addressed. We had a good bit of discussion of that and that's in the report. It is likely that the water supply availability can be established by means of bounding calculations, but those calculations were not provided as well as they might have been.

Conclusions on overall effectiveness. Obviously, this is a product of significant national importance. and this, the fact that such a report, the scientific and technical analysis, should meet the highest standards of scientific quality. And, as I will comment more on later, we think that one of the best ways of assuring quality and assuring that every possible consideration has been incorporated in the report is through the process of peer review, and I will come back to that later.

Recommendations for improving effectiveness of the TBR. We focused a good bit of thought on how the effectiveness could be improved, and we think that one thing would be a more clear definition of the audience for which the TBR is being written. And, I think, we recognize that this TBR inevitably would be written for multiple audiences.

It will end up that way. That's not an easy assignment, and I sympathize with the problems that writers have, but we think from the point of view of not only the scientific and technical considerations, but also the fact that the public has such deep interest in it, that's a factor that should be considered to the maximum extent possible. It should have a clear statement of the technical questions to be addressed and hypotheses used to test each of these considerations.

More on recommendations for improving all available scientific and technical information on issues in the TBR should be cited and discussed. Unfortunately, that wasn't always the case. The TBR should provide a complete discussion of the analyses supporting the technical interpretation, alternative hypotheses, and methods used to test them and uncertainties, and additional data needed to address these uncertainties.

We think it is important that the TBR be prepared with the direct--and, I want to emphasize that direct; it has two little red lines underscoring it--involvement of the scientists involved in the site characteristic studies, and those scientists should be identified in the report. The report should also provide a discussion of how data and analyses where selected and integrated. Again, come back to the bounding calculations where multiple methods of analyses,

they may not change the results, but as I think indicated in some of the presentations this morning, they give you more confidence in the range of answers that might result on a specific issue. That, of course, helps improve the credibility and helps reduce the uncertainties.

On recommendations concerning improving the effectiveness, we felt that the TBR could have had better graphics which would have made it easier to read, and it should have included a process of more thorough internal peer review by the scientists who worked on the report and certainly it should have included external peer review. Now, I want to point out and commend the DOE because that is precisely what this NAS study was, an external peer review of this report. It might have been better to have had more peer review prior to the release of the report, but in any event, we made a very strong point concerning the peer review.

Now, I want to reflect on the process. And, in reflecting on the process, I again want to say that the DOE provided us with every item of interest that we wanted. I thank Jane Summerson for that and her staff. Anything that we wanted, it was provided. To the best of my knowledge it was provided in a very timely manner. We think the process worked well. Our goal was to provide something that would improve the process. We were very interested in that.

We believe the DOE should be commended for seeking an external review of its work. Now, we are aware of the fact that--I remember someone asking a person who was being ridden out of town on a rail and says, well, what do you think of it? And, he answered, well, if it wasn't for the honor of it, I'd just as soon not be doing it. And so, there may be some of that element regarding this, but I want to emphasize that our committee strongly feels that the DOE should be commended for seeking this external review which was a very thorough review. It was very demanding.

The committee members, I want to tell you that every meeting--we did not have any members who missed committee meetings. We worked our schedule out and, as I said earlier, from the time we had our first meeting in this room until the final report which had gone through a review including an external review by the Academy involving seven reviewers from the time that we started was 135 days until the report was completed. So, it was a process that we had to keep moving, and I think in reflection we'd probably say that more time would be desirable. But as I said, our hope was that we could get our response back to DOE so that it could be a maximum value to DOE as it proceeded with the other technical basis reports. We received some complaints about lack of time, but I think I made that point.

Now, in closing, I want to say that there were a lot of people from different agencies that provided a lot of helpful input, went out on the mountain in late August with us and stayed with us so that they could be there to answer questions. At each stop on the field excursion, there were groups; either DOE or in some cases the state and in some cases both, DOE always, you know, made input explaining what happened there, and we learned a lot.

I would be remiss reporting on behalf of the committee if I didn't thank the committee. It was a very, very hard-working and effective committee, and I'm sorry they can't all be here to help me answer any questions that might come up. But, I also want to thank Kevin Crowley up here who was the staff director on this particular project and Rebecca Berka who was our logistics support water person when we were on the desert being sure that we had enough water and didn't get dehydrated. So, Kevin, I want you to extend my appreciation to Rebecca on behalf of the committee.

That concludes my remarks, and I'll be happy to answer any questions.

DR. ALLEN: Thank you, Ernie. You're 20 seconds early; well-planned.

Are there questions and comments from the Board?

(No response.)

DR. ALLEN: Well, let me ask one question on--one of the statements of the report was these criticisms are not directed at the individual who compiled the TBR, but rather at the management process that led to preparation of the TBR by someone other than the scientists whose work was used in the report. You've touched on this a bit. Could you amplify that?

MR. SMERDON: It is our understanding that DOE, like all agencies, is always working under time frames. If you prepare a research proposal, you have to FedEx it to the agency in Washington. You have deadlines. And, I think that our constructive comment there would simply be that if our understanding in terms of the short period of time involved in the preparation of this report was factual with primarily being one person, the committee's intent was we did not want to criticize that individual because we think that it was a good product. We just believe it would have been a better product if there had been more feedback, constant feedback, if there was a single writer or coordinator to have had the scientists involved very much. I think that is part of the first step in getting the review, you know, underway. So, we may have been sort of dancing around some words there, but the point--and, the point that I want to stress very strongly--keys back to this peer review process because we

believe from a science and technical basis that having peer review will improve the quality of the product. I think any of us who have published any paper in referee journals sometimes have--when we get the comments back, whether we want to admit it or not initially, the product is improved by having those comments.

DR. ALLEN: Other questions from the Board, the staff? Leon?

DR. REITER: Yes, two questions. The first one is we know of and you may have mentioned the fact that Clarence made a previous report on topical report on erosion that at that point was not well-received by the NRC. Apparently, it looks like some of the same things that were criticized then reappeared again in the technical basis report. Is that correct?

MR. SMERDON: I'm going to ask Kevin to comment more on that. My field--and I think someone up here mentioned this morning that the chair of a committee is--part of the job is to keep the process moving. My field does not relate to geologic erosion. So, I didn't have my head down in the barrel up to my ears, you know, looking at that process. But, I'm inclined to think that your comment is correct.

But, Kevin, do you want to add something to that?

MR. CROWLEY: No, I think, you are right, Ernie. In

fact, I think it would be fair to characterize the committee's mood as somewhat distressed when they learned that the U.S. NRC comments on topical erosion were not addressed by DOE in the technical basis report. In fact, they were distressed to learn that that review was not even referenced in the technical basis report and that it was discovered by the committee when--I think, it was Mike Bell of the U.S. NRC came and gave a presentation to the committee and pointed this out.

DR. ALLEN: Leon?

DR. REITER: Yeah, just one other question. You pointed out, Ernie, in the beginning the care with which you were concerned about making sure you're not making regulatory decisions or compliance decisions. I know a lot of people wrestled with this, the difficulty with which one can make these evaluations without as much context as possible. And, I was reading over the recommendation you've made now and I noticed--maybe you can explain this. The topic of erosion, you seem to be very carefully--or the report seems very carefully towing the line in not making any sort of judgment about compliance. But then, when I was looking at the conclusion of the last three topics, you seem to be sort of --at least, this report, you take a little bit of a difference. For instance, on surface flooding, you say,

although not well-documented, appear to provide for conservative estimates of maximum flooding depths. Although in perched water, although it does not appear to the committee that perched water will pose a problem during construction, TBR does not make data to use this point. And, finally, it is likely that water supply availability can be established by means of bounding calculations, but such calculations are not provided in the TBR.

Am I misconstruing this because--

MR. SMERDON: Leon, that relates to this whole issue of bounding values. Let's take the probable maximum flooding which involved estimates of hydrolic roughness at the surface that might occur during--now, I'm talking about a surface flood--and also bulking factors and things like that. What we were saying is that those appeared to be conservative, but there was no analyses in the report to test the sensitivity. And, if you'd made different assumptions, it might have been less conservative. Would that have appeared to compromise the general conclusion? So, it was those kinds of things that we were referring to. It's a delicate issue to focus strictly on the science particularly when there's people that are wanting to ask you, well, do you think it will work? You know, what should you do? And, you respond, well, that's--we're not going to make any personal comments on what we

think. We're going to keep our nose to the wheel, so to speak, in terms of analyzing the scientific and technical adequacy of that report. I think, in general, the committee did a pretty good job of doing that, although I would acknowledge that we may have got on the fringes in some of these areas.

Does that answer your question?

DR. REITER: Yeah.

MR. SMERDON: More or less?

DR. REITER: I was wondering why perhaps that would seem to be limited to the last three topics and not to erosion. I was wondering is there a reason for that?

MR. SMERDON: Well, we mentioned the fact that there were a lot of dating methods that could have been used that weren't used in the report. And, the confidence in the erosion estimates, we think could have been increased if there had been more dating methods used. And, secondly, if instead of looking at averages or tending to look at averages and what might be a general erosion, take special attention to look at some of the erosions that might occur as a result of localized erosions, stream cutting into--or gully cutting into the hills, landslides, debris flows, these kinds of things. The committee just simply did not have time to go into looking at all of those details, and our intent was to

help improve the process and hopefully make suggestions so that future TBRs would benefit from this one.

DR. ALLEN: Okay. Thank you very much, Ernie.

MR. SMERDON: Okay.

DR. ALLEN: Let's move on. We're right on schedule.

MR. SMERDON: Thank you. Thank you for the opportunity to make the presentation.

DR. ALLEN: The next speaker is Carl Johnson, Administrator of Technical Programs for the Nevada Nuclear Waste Projects Office, who will talk about Nevada's response to the DOE report.

MR. JOHNSON: Thank you, Clarence.

The Board asked me to talk about the state of Nevada's review of the DOE technical basis report. I'm going to basically cover three broad topics; first, a little background, then talk about the state's review process, and then go into some of our specific comments.

To go back right to the basics, the Nuclear Waste Policy Act passed in '92 requires DOE to conduct these four major actions as part of the Public Law 97-425. The important one that the technical basis report focused on was determining site suitability under the 10 CFR 960 siting guidelines. Since that law is still in place, I believe, as of this morning anyway, DOE is required to conduct that

evaluation.

Under the program approach that DOE put in place in 1994, one of those areas was to conduct a technical site suitability evaluation process. The purpose was to determine the suitability of the site, and it developed a three step process to do that. The first one being to develop the technical basis documentation, and then secondly, to develop a guideline compliance assessment based on the technical basis report, and then thirdly, to develop conclusions relative to siting compliance.

The technical basis reports provide the primary scientific basis for its later assessment. And, the reports as they were envisioned would present the available data or the current understanding of the subject including discussions of the uncertainty, consideration of alternative models or hypotheses, and then address the bounds on the conditions and processes identified.

The first technical basis report which was on surficial processes, the report was issued in May 1995 and it provides a description of the site conditions related to siting guidelines for ease and cost of siting, operation, construction, and closure; the expected preclosure groundwater conditions; and on erosional processes.

As a result of the DOE's development of the new

program approach and their development of a technical site suitability evaluation process, the State of Nevada sent a letter to DOE in December 1994 informing the DOE it would conduct its own independent oversight review of each one of the technical basis reports. Recited our responsibility for commenting on DOE's findings and the basis for those findings regarding the suitability of Yucca Mountain. We also indicated that we expected to receive all of the materials including all the references that would support each one of these technical basis reports, simultaneously with any information that was submitted to the National Academy of Sciences. We also expected that DOE would give equal consideration and weight to our comments, as well as they would to the NAS's comments. And, we then committed that we would conduct our review in a time frame similar as possible with the schedule that NAS would commit to for their reviews.

The focus of the state reviews were on the validity of the scientific data and the interpretations of that data and the adequacy of the treatment of uncertainties. We prefaced all that with the belief that the burden of proof was on the DOE to demonstrate that they had a clear and complete understanding of site conditions and the natural processes involved at the site.

Our review was again basically a scientific

judgment review. We addressed a whole series of questions. Our list is longer than the NAS's list, but includes many of the same questions that they addressed in their review. I'm not going to read each one of these because I think you can read it in your handout. I would focus though down on #10 and that has to do with expert judgment which was the focus of the discussion this morning. We thought that was a significant part of our review.

The principal state reviewers were selected because of their expertise not only in the Great Basin and the arid west, but also because of their qualifications in the areas of the topics covered by this technical basis report. John Bell, the Nevada Bureau of Mines, looked at quaternary geology and geochronology. John Fordham of Desert Research Institute looked at water resources and flood potential. And, Martin Mifflin of Mifflin and Associates looked at subsurface hydrology and quaternary geomorphic processes. They were supported by a much longer list of supporting reviewers to cover very specific areas of the review. One that comes to mind is age dating which was emphasized in great detail in the technical basis report. So, we had a number of specialists looking specifically at that particular issue.

Now, let me turn to the state's review. We

initiated our review in August and we issued our oversight technical basis report review on December 20, approximately I think two weeks or maybe three weeks behind the NAS. Let me first go through some general comments we had on the technical basis report, and then I'll get to some specifics in the individual topic areas.

I think, overall, as you pour through these comments, you're going to see a lot of similarities to the comments of the NAS. The technical basis report should focus on the scientific and technical understanding of the site rather than on guideline compliance. The purpose of the report was to set the basis, the scientific basis, for the guideline compliance assessment document. But, it appeared to us that the technical basis report, in fact, was the guideline compliance assessment document.

We thought--and, I think the NAS saw something similar--that the report should pose technical questions that relate to the guideline subject, but pose questions in such a way that the report itself develops a complete response that itself conveys an understanding of the site and its conditions. We suggested a number of questions that should have been proposed. As an example, we certainly aren't wed to these, but we threw them out as an example. I'm not going to read each one of those, but again if this document is

supposed to be a technical basis report presenting a scientific understanding, then at least myself as an earth scientist would expect to see a lot of things like maps and cross-sections and various things like that. There were a few of those in there, but there certainly could have been a whole lot more to better convey their data and the interpretations and that sort of thing; simple things like a topographic map, a surface geologic map, a geomorphic map, cross-sections which illustrate their interpretations, tables of data and other graphics that portray the actual data that is used in making their case.

I think a very important and key part is the technical basis report should have included all available relevant information. What was included was a small subset of their own database. The report didn't even include all of the DOE's database. I think it took the field trips to bring out all of the DOE's database that were available. And, I think, of course, the fact that there are others not only work sponsored by the State of Nevada, but others both in the academic community and elsewhere that also have data and published information that could contribute to the overall database of the report.

DR. ALLEN: Five minutes, Carl.

MR. JOHNSON: Okay. I think a big flaw is the report makes the assumption that the conditions at the site today are the conditions that is going to occur at the site in the future. Our point is that conditions have changed or conditions will change. The geologic history shows us that. So, that needs to be addressed. Lastly, I think the report fails to meet the standards of what at least is viewed as a comprehensive scientific document that tests various hypotheses.

Let me briefly touch on the surface characteristics that the major comment was that the geochronology used a very outdated database that DOE admitted there was new information there that basically made the old database obsolete. But, that was not presented in the report.

The TBR concluded that hillslope was at least 2 orders of magnitude lower than the U.S. average. Certainly, the data didn't support that and when you added in the available data from other sources, certainly a different conclusion was derived. I think that the NAS in their conclusion on this remark was they commented on this conclusion as being scientific fantasy. The rates of erosion in FortyMile Wash and other tributary streams were poorly supported by the database and that more recent and other relevant information would significantly alter the

conclusions there.

Surface flooding potential, the presentation was severely limited. We believe that even in preclosure, you just can't consider the two portals. There are many other things involved in surface facilities; the transportation route, the utility lines, a whole host of other things that also have to be considered under the topic of surface flooding and whether flooding will adversely affect it. The evaluation of subsurface flooding was incomplete. And, lastly, the water resource potential was very limited in that. There was no discussion of how much water was needed. Therefore, we couldn't define how much water might be available. There was no discussion of whether the water could even legally be developed. There was no discussion of what might be the competition for that water in both the present and in the future.

Just some final thoughts on technical basis reports and that is important things that I think need to be in any future technical basis report or whatever we want to call them in the future is we need to talk about what we know and, more importantly, what we don't know so we can get at what are the uncertainties. And, secondly--and, I think also very importantly--that our understanding of the site needs to be based on data, not on assumptions, beliefs, and opinions.

I throw up this last slide kind of as a postscript to our review of the technical basis report. Since our review and the NAS review, Department of Energy has abandoned the technical basis report and the technical site suitability evaluation process. Their present strategy has gone to a viability assessment which we talked about or heard about at great length yesterday. Included within that viability assessment will be the development of a series of technical synthesis reports which we haven't heard a whole lot about other than we have a schedule for development of those reports. The state's concern with that is that there appears to be no provision, no check points, or anything in the viability assessment process which provides for external peer review of those synthesis reports or for oversight reviews on our part. We have great concern in there. I bring this up because I think the Board ought to be concerned about this, also.

That's it, Clarence.

DR. ALLEN: Thank you, Carl.

Are there questions from the Board?

DR. LANGMUIR: Carl, it was hard to follow everything that's been said in your presentation and the previous one from the Academy group. But, I guess I'd ask both of you to comment on what differences if there were any significant

differences in how you viewed the technical basis report in your conclusions to your views of it. If you basically agreed--it sounded as if you in many ways agreed on what you viewed the shortcomings to be.

MR. JOHNSON: I've had a chance to read the NAS's report, and with the exception of details which mainly stem from a more extensive knowledge of the Southern Nevada region, our reviews are very similar, if not the same, our review comments as the NAS.

MR. SMERDON: Thank you for the chance to comment. I haven't seen the state's report. So, I can't comment on the state's report; though I can say that I don't think the word "scientific fantasy" are in the NAS report at any spot. And so, I want the record to clearly show that. It is obvious from the presentation that there are a lot of similarities in conclusions. But, I have not seen that report. So, I can't comment.

DR. ALLEN: Bill Barnard had a question or a comment.

DR. BARNARD: Carl, were any of your reviewers outside the state of Nevada or had no physical or financial connection to the state or were they all state people?

MR. JOHNSON: We had some reviewers from the Denver area and from the Arizona area mainly with dealing with the age dating aspects of it. But, the principal reviewers were all

in-state people.

DR. BARNARD: Thank you.

DR. METLAY: When this process of the National Academy review was first proposed by the DOE, the State of Nevada raised some concerns about how the process might work. In retrospect, having seen how one part of the process worked, one review of one report, what do you think are your views now about the sensibility and the credibility of the process that DOE proposed in the past?

MR. JOHNSON: I think I would respond to that by saying that the state spent a lot of time agonizing with Department of Energy over the NAS review process. In most cases after a lot of agonizing meetings, many of our concerns were addressed, and I think overall I would say that what resulted in the NAS review turned out to be a good one, at least on this first report review.

DR. CANTLON: Carl, just kind of a nitpick. On your general comments, your Item 4, technical basis report ignores the near certainty that the present interglacial will end soon with the transition into the next glacial episode beginning in the next few thousand years. Isn't near certainty a little bit of an overstatement?

MR. JOHNSON: I thought somebody would pick up on that because it disagreed with the climate change presentation

we've heard this morning. Our reviewers, the folks that deal with quaternary geology and climate change in the state of Nevada, have a different point of view and that is based on the history in the Great Basin. Their view is it is highly likely that the Great Basin is going to become much wetter and much colder in the future.

DR. CANTLON: As a native of this grand state, I know we all hope water will come soon.

DR. ALLEN: Thank you, Carl. Let's move on.

The final speaker on this subject before the break is Steve Brocoum of the DOE who I know from the schedule has less than half the time of either of the two previous speakers. I don't know whether this is because you felt you could wrap it up easily in 10 minutes or whether you didn't want to be on the stand for more than 10 minutes.

DR. BROCOUM: I wondered if I needed a bulletproof vest today. We actually agonized about this talk and those of the staff members that have been working on the agenda know that we agonized right up to the last minute before putting us on the agenda. You know, since we're not doing suitability process any more and internally we've kind of gone over this and in our own thinking, we haven't been thinking about suitability process now for several months. In fact, when we were getting ready our work for fiscal year '95, at that

point we already knew that we would not be doing the program approach any more. I asked my management, you know, we're going to get this report in December and should we plan activity to analyze and respond to it? I was told we will just accept it. So, we have no formal activity in our work scope issue to analyze it. So, in a sense, we really have not spent a lot of time analyzing it.

At one point during the talk, I will turn and ask Mr. John Stuckless to help me a little later on in the talk. This was supposed to be the first in a series of planned technical basis reports for addressing technical site suitability, This one addressed the technical basis that would have been used to support the guideline compliance assessments for the disqualifying and qualifying conditions in 10 CFR 960 for surface characteristics, preclosure hydrology, and erosion.

The process that we developed, I guess it was in '94, called for an independent peer review of the technical basis report to be completed and we negotiated with the National Academy of Sciences and they formulated their panel of experts in May of '95. Wrong date here. And, I think, I had a bet with Kevin Crowley. As I recall, one time in my office I bet him it would not be done by December 1, but in fact, they had it in our hands on December 1. We had it in

hand.

DR. ALLEN: That's the first time in NRC history.

DR. BROCOUM: It may well be. You know, when we planned the first in the whole series, it was very important to us that it be done in a timely fashion, and I think it was important to the NAS to demonstrate they can do that and they've certainly demonstrated that. We appreciate the efforts of the National Academy in doing the report in a timely fashion and in providing a thorough review.

We feel you can break down their comments into three categories. First is that their technical basis report did not effectively communicate the data and its conclusions. The second, that the report did not consider all relevant and available data and information. And, the third is that the scientific design and approach and the methods were inadequate. When the report came in, these comments did not come as a surprise. We had some feedback in the field and we, of course, had the comments on the topical report from the NRC.

Some reaction by DOE. That the technical basis report did not effectively communicate data and conclusions. The report summarizes technical information related to specific regulatory issues and, as such, not from all the available research and it was not meant to be a research

document. There seems to be some discussion about that today.

Now, in my view having thought about it, this was probably one of our weakest areas; the fact that when we planned the suitability process, we tried to separate the technical arguments from the regulatory arguments. The reason we did that is we were trying to keep the technical arguments in kind of a pure area of science and pure technical review. As we were completing this technical basis report, we had a lot of internal comments on that issue. How do we put it in context? Should we put the requirements of 960? And, we had a lot of our own debates. So, although it sounded like a good idea at the time when we separated the technical from the policy and the regulatory, it was obviously very complex to execute. It was very difficult to convey and it was hard for people unfamiliar with the program to understand. So, I think that's a valid comment, but you have to put it in context.

We agree that the report was ineffective in properly conveying well-articulated arguments and that such arguments are key to the success of this kind of a document. We need to be more cognizant of this in the future and we need to allow via time in the review process for that to occur.

We feel that the management process was not as effective as we expected. We have taken action to more closely manage these kinds of deliverables from our contractors. It has been pointed out by several people that there was only one author on this report. Most reports that are produced by the DOE or for the DOE are usually done by a group of authors. For example, look at the waste isolation strategy we're doing right now. That has a group of people working on it. Every major document that this program has done has involved using multiples of authors. This report was done differently by a single author who tried to synthesize and bring in all the information that was available. I think we need to learn a lesson from that and make sure in the future on reports that we're more careful and get the proper people involved. So, we accept that criticism.

The second category; that the technical basis report did not consider all relevant and available data and information. Again, this was not meant to be a research tone; it isn't. It was meant to make arguments to be able to address specific regulatory requirements. The other thing I need to make out is in areas where we might affect waste isolation and containment, we are required by our program to use qualified data for making those kinds of decisions.

Other information can be used in a corroborative sense. But, if the information is not qualified, we cannot base decisions on that. That's part of our QA approach. That's a very important issue that we have to keep in mind. It's not an excuse, but it's just a fact of life of our program. It's a requirement that's put on us by the NRC. So, by design, not all available information was deemed relevant for addressing this regulatory issue. I think, in the future, references from non-DOE sources that are available in a citable form will be acknowledged. If it's qualified, it will be used in the decision making; if it's not, it will be used in a corroborative sense.

The third category comments is that the scientific design and approach or method were inadequate. Some of these comments, of course, paralleled the comments we got from the NRC. Looking back at the NRC topical report, that report--if I can digress for a second--was originally based on legal advice, very narrowly focused. One of the comments was that we relied on a single dating technique to derive technical conclusions. That's also a comment from the NRC. We are using other methodology to verify our dates since then, and we are doing sensitivity studies to see how the different dating techniques, we've bound the ages, or how it influences the curves used to determine ages of the boulders.

Another comment was that the areal extent of the erosion study was too local and did not look at a variety of deposits or locations that might exhibit higher rates of erosion. We did focus on local erosion rates, but we are also familiar and did address erosion on a regional basis. These things could have been addressed in the TBR. They may not have been adequately.

On this one, I'm going to ask--by focusing on relict boulder deposits and with the dating technique used, there was a systematic bias towards selecting the oldest and most stable deposits. At this point, I would like to ask the help of Mr. John Stuckless from the USGS who is I think going to make a statement. Is that right, John?

MR. STUCKLESS: Yeah, I have a couple--

DR. ALLEN: John, let's try to keep it brief. We're already past the scheduled time or very close to it.

MR. STUCKLESS: I have every reason to keep it brief. I've got a plane.

The report kind of reminded me when I got my first comments back on my dissertation and Professor Compton said it's a nice piece of work. Now, if you would just completely rewrite it in English. I think Steve has made it obvious that we were asked to write one thing, and the NAS reviewed something somewhat different. But, in particular, with the

reliance on a single dating technique and Kevin's comment earlier about not responding to the Nuclear Regulatory Commission's comments, the panel was given our qualitative data showing very low sedimentation rates around the mountain which was then a backup for the low erosion rates that we calculated. They were shown Stage 4 carbonates underneath the boulder deposits which generally are conceded to take at least 200,000 years to form, again supporting the low erosion rate. This particular one about focusing on the boulder deposits that were the most stable was very intentional. We looked at what had been eroded between them as the most rapid erosion rate using these then as the yardstick against which they could be measured. The most critical thing in this showed up in one of Carl Johnson's slides where it said we concluded erosion rates that were 2 orders of magnitude lower than the norm for the southwestern U.S. If it had been twice the erosion rate as the norm for the southwestern U.S., we still would have met our regulatory standard. I think that's a critical factor here. That is, we could be off 4 orders of magnitude and still meet the erosion rates that are necessary for licensing.

The poor design of this particular study falls right out of that observation because we never did design a study to do this. We did a climate study and a tectonic

study. The people in those two groups realized that erosion was not likely to be a problem; therefore, we recommended to DOE that they not spend money writing a study plan and then doing an extensive study because it looked like a non-issue to us. Unfortunately, we did not consider the political aspects of this and it sort of has evolved into a bit of an issue. I'm not quite sure how we'll get back out of this. But, we will have, as Steve has shown, considerable data; one again, not designed for erosion, but designed in the climate program and the tectonics program, as well, that will give us a better handle.

I have a report completed last Friday that says indeed some areas on that mountain probably erode at the average rate or slightly below for the southwestern U.S. and still then for 2 orders of magnitude away from where it's going to be a problem.

DR. BROCOUM: It's more like two points on this second bullet. One is keep in mind there's a minimum of 240 meters of overburden over the repository horizon, anywhere over the repository horizon. That's the minimum number. And, secondly, the hydrologic interface is 80 meters down.

In conclusion, we feel that erosion, regardless of all these issues, is not a big issue at Yucca Mountain, and we're focusing on the erosion because that would affect the

long-term performance of waste isolation at Yucca Mountain. We are addressing the comments from the NRC and we will be submitting them to the NRC. If--big if--if we go and prepare our license application, we will obviously recast our arguments and address these issues when we write our license application. So, those are our comments.

DR. ALLEN: Thank you. Thank you, Steve.

Would it be fair to say that there was somewhat a mismatch here? You've emphasized time and again this was not a research document, but was aimed at specific issues of regulatory importance. And, yet, the NAS was asked specifically to evaluate it as though it were at least a science from a research point of view which they did and would pay no attention to regulatory concerns. Did this represent a mismatch from the word "go"?

DR. BROCOUM: I think, in retrospect, you're correct; it was a mismatch. I remember debating that in-house and we wrestled with it and, you know, I guess if we were doing it over, we know today we would cast that review differently and put it more in context of what we're trying to address in the regulations.

DR. ALLEN: Would you agree, Ernie?

MR. SMERDON: Yes. I was thinking as Steve made his presentation which incidentally I think our committee would

be very pleased. I was very pleased by the presentation because I think that it indicates that DOE has taken the comments that we have tried to make in the spirit that we have tried to make it. The point I was going to make is that I was kind of thinking of Cool Hand Luke, you know, when he says what they've got here is a problem with communication. I think that one thing that we saw during the process of this review was the level of communication increasing continuously. I think that's a tribute to all the people involved. And, I think, in retrospect, you can always go back and say we may have been looking at different things and it's not our point to address this further other than to say that the audience--in one of our recommendations was the audience needs to be more clearly identified and perhaps that is a constructive thing. But, I agree.

DR. CANTLON: This is addressed to Steve. DOE clearly has a responsibility and certainly and abiding interest in keeping the support of the scientific and technical community, perhaps, more so even than the general public, because if the scientific and technical community loses confidence in the basis on which DOE is making its decision, you're in serious trouble. And, it seems to me as this document was prepared, the scientists that generated the data ended up quite remote from what was said abut their data. Is

there anything as you look at that process that you could assure the Technical Review Board that going ahead in TSPA and other places where you're going to be absolutely dependent on the quality of the scientific undergirding of those decisions--that you can assure this Board that your scientists really stand behind the statements you as an agency are making about that assessment?

DR. BROCOUM: You're making a very important point. It's something that I have thought about personally a lot. Let me just make a couple of comments on if we did a future report like this. When this report was done, the DOE managed the M&O and managed all the other participants separately. Since the report was written, since the middle of last year, with the exception of the USGS, all the other participants are now part of the M&O structure. So, say, if the M&O was writing another version of this report, it would be very--since now they in a sense manage the technical work, they have that responsibility to make sure all the correct inputs are in. So, I would expect to see an improvement because of these comments from the NAS, but because of our own recognition that was an issue.

With respect to the TSPA, I have spent a lot of time talking to Mr. Abe Van Luik and to Jean Younker and to other AMs on how we are going to manage the next TSPA. We

want to make sure that the assumptions, the parameter values, the probability distributions that go into the next TSPA are those that can be supported by, what I call, the end elements of the program. In other words, if we put an assumption about cathodic protection, it has to be supported by the engineers. And, if you put an assumption about flux rate, it has to be supported by the hydrologists. So, we are thinking of how we're going to set up a management structure to do that to make sure that happens. So, it's something very high on my level of concern.

DR. CANTLON: Thank you.

DR. ALLEN: Other questions from the Board?

DR. DOMENICO: I just have to support what you two said. This is applied science. You use bounding calculations. I'm not sure National Academy of Science is the right board to review this because they're going to look at it from a pure science perspective. I think what we heard from Stuckless, he's telling us that, you know, oh, we needed this because it's 2 orders of magnitude more than we need. That's applied science. That's judgment and it's bounding calculations and I think there has to be room for that and I think there has to be--the reports, I think, are maybe inadequate for some of the reasons we heard, but I think that one needs a better--a more receptive sounding board. And,

you may not be able to get that out of the Academy. That's not against the Academy. I'm just saying it's the nature of the Academy.

DR. BROCOUM: We looked at several different avenues when we thought about the peer review and at the time the Academy seemed to be the best avenue in terms of independence, in terms of ability to do this kind of review, in terms of having confidence of the stakeholders and that kind of a thing. So, that was the decision we made when we decided to go down this route.

DR. ALLEN: Questions from the staff?

(No response.)

DR. ALLEN: Well, we're within 30 seconds of being on schedule. I think this has been a very revealing session. I think we appreciate the forthrightness with which all of the participants have participated here. I personally have found it not only interesting, but I think very valuable.

So, we'll take a break now for 15 minutes until 3:15.

(Whereupon, a brief recess was taken.)

DR. ALLEN: I have one announcement to make. That is that some of you have noted that we ran out of copies of the viewgraphs used by Carl Johnson. If any of you would like copies of those, please see our staff in the back of the room

and get your names on a list and we will send those to you.

I must say that I feel much more comfortable--or I felt much more comfortable in introducing the first group of talks which deal with geology with which I'm somewhat familiar than do I have in introducing the following three which have to do with the disposal of high-level waste and surplus fissile material from defense activities. But, these also require geologic disposal. It was decided long ago by President Reagan in 1985 that they would be commingled with civilian waste in one or more repositories rather than building a defense only repository.

Under the current DOE division of responsibilities, the Office of Environmental Management or EM renders defense waste into waste forms suitable for disposal and the Office of Civilian Radioactive Waste Management or RW accepts the waste and transports it to a repository and disposes of it. Steve Gomberg of RW was to have described, as you will see in your schedule, the process that brings us up to date on the status of the coordination between RW and EM. He is not able to be here. Diane Harrison will instead make that presentation.

After that, we will switch horses still one again. The United States and Russia have mutually declared large tonnages of their respective stockpiles of weapons grade

uranium and plutonium to be surplus. At issue is how one is to dispose of these materials, particularly the surplus plutonium. Bill Danker of DOE's Office of Fissile Material Disposition will bring us up to date on progress in this area, particularly progress regarding modes of ultimate disposal. Among other things, he will cover the draft programmatic EIS that his office is about to release.

Many of the technical details underpinning the draft programmatic EIS that Bill's office is preparing are generated by OCRWM and its M&O contractor. This includes long-term criticality estimates which are very important for decision making in this area. Diane Harrison is responsible for this work, and she will be reporting to us in her second appearance about it.

Let me again emphasize that those of you who wish to speak in the public session following these three talks, please make sure you sign up in the back.

Our first speaker on this topic is Diane Harrison.

MS. HARRISON: I was thinking that I was going to suggest that Steve Gomberg was a casualty of the Blizzard of '96. And then, I got to thinking about who was getting ready to stand up in front of the Nuclear Waste Technical Review Board and give a presentation and decided that I was the casualty of the Blizzard of '96. I'll do my best to provide

Steve's presentation. He had asked me to give this talk because we have been working very closely and I provided coordination effort for him in providing the Yucca Mountain Project's participation and the defense waste activities.

The idea here was to provide a description of the potential waste forms requiring disposal in a geologic repository, provide a status of the current treatment and management activities that are ongoing. I know Carl Di Bella had asked for this presentation, in essence, to update some of the new Board members. He wanted to emphasize the integration between RW and EM that is ongoing. I'm going to summarize some of the key spent nuclear fuel, the DOE-owned spent nuclear fuel, and high-level waste considerations and provide an update on the plans and activities to incorporate the waste forms into the OCRWM program.

The current waste management system is baselined for commercial spent fuel and canistered high-level waste. That's broken out into the 63,000 metric tons heavy metal of commercial spent fuel and 7,000 metric tons of the defense high-level waste glass. There's quite a bit of information available on these two waste forms. As a result of the March '94 General Counsel determination that there is statutory authority to dispose of the DOE-owned spent fuel in a repository, of course, contingent upon payment of fees, we

are planning to incorporate the DOE-owned spent fuel into our baseline. In this manner, the DOE spent fuel would displace some of the high-level waste allocation. It would displace some of that 7,000 metric tons. In addition, we are evaluating other waste forms for their appropriateness in disposal or acceptability into a geologic repository.

The spent nuclear fuel estimates in the year 2030 show that we have about 85,700 metric tons of the commercial spent nuclear fuel that will be stored in pools or dry storage systems at the nuclear utility storage sites. We are also estimating about 2,750 metric tons of the DOE-owned spent nuclear fuel. This is generated from weapons production, the Navy fuel, research and development, other activities, and these are all stored across the DOE complex primarily at the Hanford site, Idaho, and at Savannah River. The major constituent is the end reactor of spent fuel at Hanford. Of the 2700 metric tons, the end reactor comprises 2100 metric tons of that.

A brief status of the DOE spent nuclear fuel activities. The programmatic spent nuclear fuel management and the Idaho National Engineering Lab Program's record of decision issued in June of 1995 stated it was to provide a safe interim storage and management of the spent nuclear fuel at specific locations until alternate disposition. In

essence, what it did was it identified certain locations across the DOE complex for specific types of spent nuclear fuel. Also, it stated that all the DOE-owned spent fuel will be stabilized and characterized and prepared for ultimate disposition and that the planning basis for some of, if not all, the DOE-owned spent fuel in a geologic repository, but the ultimate disposition was outside of the scope of that PEIS. The nuclear weapons nonproliferation policy concerning the foreign research reactor spent nuclear fuel currently has a draft EIS out that assesses both direct disposal and chemical separation options.

Now, the high-level waste requiring geologic disposal, some of which is produced from nuclear weapons production, is expected to produce up to 6,000 canisters at the defense waste processing facility, up to 9,000 canisters at Hanford in Washington, and up to 800 canisters at the Idaho National Engineering Laboratory. That high-level waste from commercial reprocessing is expected to produce up to 310 canisters at the West Valley Demonstration Project. These numbers are based on utilizing a DWPF canister for all the evaluations.

A quick status of the high-level waste production. The West Valley Demonstration Project completed their operational readiness reviews in November 1995. It is

expected to begin borosilicate glass production in June of this year. For the defense waste processing facility, proficiency runs are planned for completion this month. And, Secretarial approval is needed before start of glass production, and I believe that's scheduled for around March of this year. For the Hanford waste, there is a draft EIS out which addresses the tank cleanup including the cesium and strontium capsules that are located at Hanford. In addition, there have been issued a draft RFP for privatization of that activity.

Integration between the EM and RW programs for the DOE-owned spent nuclear fuel is accomplished primarily through the DOE-owned spent nuclear fuel steering group. That group was authorized by the director of RW and the assistant secretary for EM back in July of 1994. The following chart actually discusses the responsibilities a little better. The steering group continues to identify key issues affecting the ability to accept transport and dispose of the DOE-owned spent nuclear fuel in the repository and continues to develop and recommend data needs, testing programs, and other activities that are necessary to allow integration of this new waste form into the waste management system. The steering group has also been providing--with the intent to provide early guidance to EM on the acceptability

of the waste forms for disposal. Looking at the various forms of the DOE-owned spent nuclear fuel that's out there, some of it, we can say is suitable for direct disposal. Some of it, however, we feel might require some conditioning or a pre-treatment. Some of it would require some processing. So, we're providing that sort of input into the EM program.

The integration of the high-level waste activities is done primarily--that's been an ongoing relationship for some time. We participate in the quarterly meetings that EM holds on the high-level waste status and issues. We participate in the EM quality assurance audits and surveillances. We participate in the waste acceptance technical review group, and that's a group that reviews the documentation that demonstrates the high-level waste form compliance with RW's waste acceptance requirements. And, finally, the director of OCRWM has concurrence on the DWPF and West Valley start of radioactive operations.

Just briefly to go through some of the waste form requirements, of course, they must meet the criteria defined in 10 CFR 60.135 or all of 10 CFR 60, actually. For 135, it addresses that it must be a solid and non-combustible materials. An very important criteria is the waste form must remain sub-critical for long periods of time. For some of the DOE-owned spent nuclear fuel, this is something that we

really need to investigate. Currently, the plan is to exclude any RCRA listed materials from the first repository.

The waste package design of which the waste form is a part of the waste package, you are not allowed to have any explosive, pyrophoric, or chemically reactive materials. And again, the pyrophoric and chemical reactive requirements are a couple of areas that require some investigation, particularly perhaps like the end reactor fuel which is a metal fuel. Lastly, the waste interactions must also be evaluated. Solubility, hydriding is another important area that needs investigation for the DOE-owned spent fuel. And, lastly, it's kind of new, and I think this probably primarily comes along because of the Navy fuel that is being considered as addressing safeguards and security and material control and accountability.

The waste form is a key interface in the operational performance of a waste management system. The characteristics of the waste form, of course, helps define the design of the waste package and waste handling equipment, transportation equipment and facilities, and the repository surface and subsurface. The waste form also performs as part of the engineered barrier system which has its set of requirements; substantially complete containment from the waste package and then controlled release or limited release

from the engineered barrier system after the containment period. And, lastly, the long-term criticality control, of course, must be maintained.

The preliminary requirements for Disposal of DOE-owned Spent Nuclear Fuel in a Geologic Repository document--that's the title of a document--has been completed in January 1996, just this month. The plan is to revise RW's baseline to incorporate this into the--incorporate the DOE-owned spent fuel with the other materials. The importance here is we were provided a one-third to two-third allocation between the DOE-owned spent fuel and the vitrified high-level waste for their 7,000 metric tons for our planning purposes.

Some of the preconditions for acceptance. Of course, fees must be paid in accordance with some interagency agreements, some memorandum of agreement. The total DOE capacity must--for the DOE wastes is still limited to the 10% or the 7,000 metric tons. All of the spent nuclear fuel characterization and testing and other activities need to be conducted under the RW's quality assurance program. All appropriate NEPA reviews must be performed before final acceptance.

Some of the key near-term activities that are ongoing. Again, the requirements, a document has just been finalized in January. Continue to identify the data needs,

the information that we need, on this material. There's quite a bit of data that's out there, but nothing that compares to what we have on the commercial spent fuel in the way of leach rates, long-term performance, those sort of characteristics. We need to develop a memorandum of agreement. This one comment here, characterization and assessment of the key categories of DOE-owned spent fuel, in the steering group, EM provided us with their priority for looking at the fuel and the end reactor fuel was the highest priority; it is the highest quantity. And, there are some near-term treatment activities that they are planning that we need to assess. The Navy fuel is the second priority. The TMI, Three Mile Island, rubble was their third priority. And, lastly, we need to address the NRC safeguards and the materials control and accountability.

Some of the other wastes under evaluation may require geologic disposal, but they're not yet in the planning basis for the waste management system or RW program. These include the immobilized weapons-usable fissile materials and the mixed-oxide spent nuclear fuel. You'll be hearing about both of these in the following presentations. The cesium and strontium capsules that are at Hanford, the Greater-than-Class C low-level wastes, and the RCRA mixed wastes, again these are--no decisions have been made

regarding the acceptance of these materials into the repository. We're working on some of them. Not all of them are being evaluated at this time.

DR. ALLEN: Okay. Thank you, Diane.

Any questions from the Board?

(No response.)

DR. ALLEN: Questions from the staff?

DR. BARNARD: On your last slide, you mentioned Greater-than-Class C low-level waste and RCRA mixed waste. Are both of those commercial?

MS. HARRISON: Yes. Greater-than-Class C is commercial. The RCRA mixed wastes, I don't know. I'm not certain what Steve was intending behind that. I know the Greater-than-Class C is that waste generated at the utilities, et cetera, but I'm not sure about the RCRA.

DR. BARNARD: Okay. Has DOE made a formal decision that Greater-than-Class C low-level wastes will go in a repository?

MS. HARRISON: The DOE hasn't made a decision. It's in the 10 CFR 50, I believe. The regulation says that it would require geologic disposal, and that is the approach that EM is taking.

DR. ALLEN: Other comments or questions?

(No response.)

DR. ALLEN: Okay. Thank you, Diane.

Let's go on then to the discussion of the disposition of surplus weapons plutonium by Bill Danker of the Office of Fissile Materials Disposition of the DOE.

MR. DANKER: Greetings from the land of heart attack snow and five-year-olds with stomach flu. I can say without exaggeration it's a pleasure to be here in 70 degree weather. My boss, Mr. Canter, regrets he can't be here today. The last plan was that he would be briefing his own senior technical review group. With the weather the way it is, I have no idea whether that's going on or not.

But, I'd like to emphasize one point in starting out and that is we may not have all the answers. I'm fairly sure we may not have all the questions, but in real time, this fairly young organization and project is sharing real time with a whole bunch of organizations, where we think we stand now. I might cite a few examples. Over the last couple of months, we've briefed the interagency working group that is charged with responsibility of plutonium disposition. I'll talk a little bit about their role. We've briefed the defense nuclear facility safety board. We've held a series of meetings with the Nuclear Regulatory Commission on a full range of issues. We co-sponsored a workshop with the Office of Environmental Management on immobilization technologies in

December. We look forward to a series of public meetings, between six and eight or so, over the next couple of months once we release the draft programmatic environmental impact statement. And, frankly, have profited from the 12 public meetings we had in late 1994 which scoped the activity we're involved in and, as a matter of fact, modified some of the criteria that you'll see later. I'll repeat later, but we appreciate Diane's active involvement from early-on in advising us on repository performance issues.

I've got a lot of material and a limited amount of time. So, I think I'll tend to skip over a few of the viewgraphs that you see in your package, but the toastmaster sort of says you ought to tell them what you're going to tell them and I hope to leave you with a sense of context of why our office was established. I'll also explore with trepidation the so-called spent fuel standard. We're still, frankly, trying to wrestle that one to the ground. We'll review the plutonium disposition alternatives that we deem reasonable and that show up in the programmatic environmental impact statement. And then, finally, segue to Diane's talk.

Back in the early 1980s, I spent five years at the IAEA. I had a Russian boss and three Russian colleagues and I was as surprised as anyone when the wall came crashing down. So, it's a brave new world with new challenges. The

good news is major arms reductions; the bad news is we didn't quite anticipate the rapid reductions, and we're faced with what some have termed "clear and present danger posed by the separated fissile material" deemed to be surplus to national defense needs.

In September of 1993, the President issued his nonproliferation directive which included the need to study how to disposition this surplus fissile material. DOE is the lead support agency to the interagency working group chaired by the Office of Science & Technology Policy and also the National Security Council.

In January of 1994, Presidents Clinton and Yeltsin agreed to jointly study these issues and we're doing that. Draft reports will be prepared this spring. At the same time, the National Academy of Sciences came out in January of 1994 with their first volume where they cited "clear and present danger posed by the materials". The Secretary of Energy cited this in establishing a crosscutting project of which I'm a part and that later evolved into the Office of Fissile Materials Disposition which incidentally was instituted by statute reflecting the importance that Congress places on this task.

I'm not going to dwell on this because, as I understand, the Secretary is planning to hold a meeting

within two weeks to further declassify some of the apparently classified inventories. But, the bottom line is that the 50 metric tons of plutonium that we've used as an assumption for scoping out our activity continues to be a useful assumption and will be refined over time.

Reducing the global nuclear danger is the key motivator for our office and nonproliferation is a key driver. As I indicated, we are struggling with the concept of the spent fuel standard and we'll talk about that on the next slide. How urgent is the situation? We've basically been told it's urgent, but take time to do it right. We initially went into our scoping meetings with something that said it's probably too late if you start within 20 years and complete the activity within 50 years, and we were admonished and took to heart public comment that said you probably ought to cut that in half.

The last bullet simply notes that in addition to the environmental safety and health considerations covered in the programmatic environmental impact statement, other factors are considered in reaching a decision on plutonium disposition.

This has always generated lots of discussions. I've participated in four of the 12 meetings around the country and in every case substantial portions of the

discussion centered on this and what you mean by that. It's really a perspective; it's not a standard. As a matter of fact, some spent fuel doesn't meet the standard, but it's really an attempt to put this 50 metric tons of plutonium in the context of the greater amount of reactor grade plutonium and spent fuel. I think, if you've got 30,000 metric tons of plutonium in spent fuel pools in casks around the country, that probably is 300 metric tons of plutonium and, of course, as Diane indicated, that's growing. The NAS, IAEA, and others are concerned about reactor grade plutonium being diverted to weapons use. As a matter of fact, the NAS report is a good reference on that issue. But, in trying to create large, heavy radioactive forms to simulate spent fuel, there's lots of room to disagree on how to do that. As a matter of fact, at the workshop just in December, Arjun Makhijani from the Institute of Energy & Environmental Resources proposed a hot can concept where instead of having the radiation barrier embedded in the matrix, you put the plutonium in the glass and then impregnate the container with a radiation barrier. Suffice it to say that for all of--the message has come through earlier in the day that you need to get the experts involved. We hope to do that this summer; get nonproliferation experts independent of our program involved in the assessing the disposition forms that we're

studying.

We use these criteria in an initial screening that I alluded to earlier and they have been refined. The first criterion focuses really on physical security as distinct from the kinds of things I used to do at the IAEA which is more--it's detection of diversion as opposed to physical protection and really the distinction is that the IAEA doesn't trust the host nation and assumes that they're the diverter. I might use base launch as an example of how we use these criterion in the initial screening. If a large payload were aborted, you fail your environment safety and health criterion. If you tried to split that payload up into a whole bunch of launches, then you might fail on cost-effectiveness. It's those kinds of tradeoffs that were done on the initial alternatives.

Sometimes, pictures are more useful than words and this is what I've been attempting to say. While we're using the National Environmental Policy Act compliance process to, for example, interact with the public, have scoping meetings, comments on the draft PEIS, and so on, there are a whole range of other factors that lead into the record of decision. It says records here because we've split out highly enriched uranium into a separate EIS, and so I'm primarily talking to you today about the plutonium disposition. But, reports on

those factors, as well, will be made available in reading rooms to inform folks commenting on the PEIS, as well. And, at ROD, we will select one or more alternatives. The decision process will involve other agencies through the interagency working group. The decision ultimately resides with the President.

This slide notes the major groups of alternatives we've retained as reasonable for disposition. I might note we're not seeking a referendum on whether plutonium is an asset or a liability. We've long since given up hope of reaching consensus on that and, frankly, clearly there's strong feelings on that. How we disposition this plutonium to increase proliferation resistance will be based on assessing all of the preceding criteria.

This slide lists the alternatives evaluated in the programmatic environmental impact statement due out next month. Data reports for each alternative will be made available in the reading rooms. We'll talk about these a bit more in later slides. So, I think we'll move.

Our notice of intent to produce a programmatic environmental impact statement on this subject was issued a year and a half ago and was the basis for the scoping meetings I alluded to earlier. The implementation plan was issued thereafter and, as a matter of fact, I've got one sort

of scribbled up copy of both of those. This is the notice of intent and this is the implementation plan, and if people are interested in copies of those or other reports I reference, please just let me know and we'll get copies to you. As I've noted, the draft PEIS is due out next month, and we're driving toward a decision later this year.

About a year ago, we screened an initial list of 37 disposition alternatives down to about 10. This is documented in the March 1995 report. The second bullet shouldn't really say preferred alternatives. We're in the process of identifying the alternatives we want to study in more detail. Some of the activities we're pursuing this year include an independent review by the nonproliferation experts that I alluded to earlier. We're also focusing on formulations of glass and ceramics to get a better handle on various compositions. In a later slide, I'll talk about a demonstration we just are in the process of down at the defense waste processing facility. I might note that the approach we've taken over the last year is to have alternative teams with crosscutting technologies represented by people right on the team. So, it really is a good systems engineering approach of having ownership of the product by the people with responsibility in those areas.

This slide might be an opportunity to indicate our

current thinking on where we're headed. Regarding reactors and the mobilization, it seems unlikely that new facilities would be constructed. For example, the Secretary of Energy has indicated no new reactors would be constructed for the tritium mission which has a longer planning horizon than ours. Borehole variance would probably be reduced to a single approach using an immobilized form. The direct form might be slightly cheaper, but postclosure performance is expected to be better with the immobilized form. The bottom line is there's no basis yet to eliminate the borehole alternative. I might note that I think 15 or 20 years ago, it was eliminated for the high-level waste mission. Given differences in the mission in terms of volume and heat and so on, we're re-evaluating that in our paradigm for application for our mission and right now it retains--it will show up as a reasonable alternative in the draft PEIS.

This is a brief summary of the reactor disposition group of alternatives. Bottom line is mixed-oxide fuel utilization is an international fact of life. Clearly, it's favored by the Russians who start their talks by saying that plutonium is our national treasure. The schedule is dictated by availability of fuel. Right now, we don't have--there is no current production scale MOX-fab capability in the U.S. that drives schedules in this area.

In non-reactor disposition options, the NAS report recommended use of the defense waste processing facility, as well as a reactor as its top choices. They recommended studying the borehole a little further in that it offered promise of being faster and cheaper. I might add along with the CANDU option are the only options independent of the U.S. Federal Waste Management System. The NAS noted also you might send good stuff to reactors and plutonium which couldn't meet fuel-fab specs to immobilization. We'll be looking at hybrid combinations this summer. I think this is probably best displayed on the next slide, but I might just clarify repository impacts indeed are looking at all of the alternatives.

This slide shows that for the borehole we're not adding a radiation barrier, but are relying on the geologic isolation to provide proliferation resistance. For forms going to a high-level waste repository which might maintain a retrievability period of up to 100 years, we're planning on adding a radiation barrier. And, again, if these forms prove not to be acceptable at a repository, that's grounds for disqualification.

As noted earlier, we'll focus attention this summer on use of existing facilities for both glass and ceramic focusing more detail on, for example, the canister concept

which I'll talk about in a minute at Savannah River or the adjunct melter concept which simply means having a smaller melter adjacent to the defense waste processing facility given some problems associated with using the current melter that's there and also the ceramics at existing facilities.

Starting on New Year's Eve and finishing the morning of January 3 of this year, two defense waste processing facilities--and, maybe at this point I'll break with tradition and use the second viewgraph machine if I'm allowed to do that. I'm not sure how clear that is, but it basically shows--I think, this is the 8-can frame. It sort of looks like two Foster beer cans tied together. These are the small cans with plutonium in the glass. Actually, of course, we're using a surrogate for the plutonium. And, there's sort of a--in the spiral and the framework and then that framework would go into the DWPF canister which you may be familiar with. During that time, they were poured full of borosilicate glass to demonstrate this can-in-canister concept. Pours went well averaging 24 hours to pour about 3800 pounds of glass up to about eight feet in the canisters. Our plans are to radiograph one of them--I think the 20-can canister--next week. We'll end up doing destructive analysis on both and look for any significant voiding or framed up emission, that kind of thing.

I might mention that a previous speaker made a correlation between intellect and August tours and I chose August to--I brought it on myself. I pretended I was a piece of plutonium and wanted to walk through the can-in-canister flow sheet and it was, I think, 95 degrees and 80% humidity down there when I did that. I don't think I'll do it again.

Regarding the immobilization options, in November of '94, there was a screening process where they started with, I think, 72 or 73 different identifiable forms and screened those down to three forms and then to basically six approaches. As I indicated earlier, PEIS data has been developed and will be going into the reading rooms. We've initiated dialogue with the NRC on this subject. Dr. Kushnikov from Russia who is the co-chair of the joint study on immobilization was here last month, and we're driving towards having a draft report in this area fairly soon.

Diane, this is my segue to you. I've mentioned the first bullet before. It's always good when you speak well of people in their absence, and I think Mr. Canter is to be commended for a decision made early-on to involve RW in this process from the beginning. You also have the choice of doing your work and then asking people for their concurrence or comment or review at the last minute right before ROD. That hasn't happened. We've had a very beneficial dialogue.

It's paid dividends in helping us establish baseline configurations. Diane will talk a little more about that. But, the bottom line is if we establish that a form coming out of these dispositional alternatives can't go to repository, then that's grounds for elimination.

Thank you for your attention. I'd be pleased to address any questions you might have.

DR. ALLEN: Thank you, Bill.

Questions from the Board?

DR. CANTLON: Yes. As I understand it, by one of the calculations the Defense Department is behind a few hundred million dollars in cost sharing on construction of the repository or exploration and R&D for siting the repository. It seems to me that this doesn't create a very hospitable climate for the utility funded waste fund bearing the big brunt of it and all of the hassle on the budget now. What assurance is there that the Defense Department (a) is going to fill in what's missing in the repository development scheme, as well as paying its full share for the disposal cost?

MR. DANKER: That's the old paradigm, Office of Defense Programs; where the new paradigm, Office of Fissile Materials Disposition. That's the facetious answer. The bottom line is I can't defend allocation of funds in support of high-

level waste going to the repository and how that's been handled within the Department in the past. I'm aware it's a problem. I can only tell you that we're up a notch from there. We're basically trying to figure out what are groups of technologies that are appropriate? Is the fundamental approach we're taking on a spent fuel standard the right way to go? Downstream from this record of decision would be project-specific EISs and specific applications of certain technologies that would then raise all kinds of issues of space in repositories and specific configurations and compositions. It's a very valid concern that you raise and I can't answer it.

DR. CANTLON: A followup question. Clearly, the Russians, having spent a good portion of their GMP creating their plutonium reserves, view it as a national resource and are planning to use it in a MOX system, but they're not the only ones. France and England also have systems to do that, and Japan is planning to do it, China is planning to do it. To what extent is it a rational U.S. position essentially to equate that possibility of it to such a low priority? It seems to me it's continuing to go down as the option as opposed to where it was, let's say, a few years back.

MR. DANKER: I have been an advocate all along for making sure that we're well-plugged into the international

context in which we operate. I guess, I have a couple of comments. One is that if you deem progress on meeting the spent fuel standard or increasing the proliferation resistance of the separated fissile material form, then if one country chooses a different path to meeting that same objective, we shouldn't object and we shouldn't enforce the method that we might choose on them. So, again, we can establish our own criteria and come up with our own disposition path for meeting that objective and should tolerate their independence in meeting the same objectives. So, that's one perspective on it. But, I think, we need to respect the fact that--I mean, quite frankly, I'm a missionary's kid and I grew up in Tokyo, Japan. And, I think, it was two years ago I heard a Japanese representative stand up and people talk about the lack of economics on plutonium utilization and the gentleman started out saying no gas, no coal, no oil equals no choice. It's less economics and more a national security kind of consideration. Everybody has their own approach and I think we need to be aware of that, but again a good comment.

DR. LANGMUIR: The Russians for some time have been talking about the possibility of a deep borehole for radioactive waste disposal. You talked around the extent of involvement that might be anticipated or at least being

discussed between the Russian problem with disposal and the U.S. problems with disposal. And, when I see the deep borehole emplacement option as one of the ones we're considering, all I can think of is another hornets' nest just like Yucca Mountain in a different place. In my experience with deep disposal of toxic wastes, they often come back up. In that case, it's typically because they're under pressure, but nevertheless, this would be a hornets' nest is another uncertainty area that could get all sorts of folks in the environmental groups excited and give them another target and probably put this off as something that wouldn't happen for decades, if it ever happened, as opposed to trying to battle with the one battle we have which is the repository at Yucca Mountain and put it in there which certainly seems more likely to get approval ultimately, if ever. That was a statement, I guess; not a question. I guess, I'd like you to--

MR. DANKER: I have no basis to allay your concerns in that area in terms of identifying what we've called silver bullets or disqualifying conditions. Early-on, people said, well, you'll never get it licensed. So, we had a meeting with the NRC and we said can you help us write the basis for disqualifying us on a licensability standpoint and we came away from that meeting without that silver bullet.

Basically, they said if you can establish a national consensus that this is indeed a clear and present danger and the country as a whole wants to do something about it and you establish enabling legislation, there's nothing from a licensability standpoint that will stand in your way.

But, you're raising excellent points regarding site characterization and, you know, all of the issues associated with finding a site. We've certainly been admonished. We've tried to go around and have one-on-one meetings with key stakeholders. And, when you talk to Tom Grumbley, he'll talk WIPP to you; when you talk to Dan Dreyfus, he'll talk Yucca Mountain to you. It's an excellent point.

Again, I have no basis to allay your concerns. But, I might mention that we're meeting with the Russians on the subject. We have a task associated with deep geologic disposal. Dr. Tatiana Gupalo is working with Dr. Bill Halsey from Livermore and they will also have a joint report in this area that again is not consensus; it's developing mutually agreed-upon facts in that subject area.

DR. DOMENICO: What do you mean by immobilization with regard to the borehole disposal? I mean, where does the immobilization come in?

MR. DANKER: The current concept is as opposed to the direct emplacement. The immobilization they're talking about

is coming up with one-inch diameter ceramic pellets that get embedded in the grout and emplaced in long, skinny buckets right in place.

DR. DOMENICO: So, you'll be disposing of a solid?

MR. DANKER: Yes.

DR. DOMENICO: Presumably, in a liquid environment, in deep brine, or something of that sort?

MR. DANKER: Yeah, yeah. Theoretically, two to four kilometers deep, right.

DR. DOMENICO: There's another--well, I thought Oak Ridge had experience with borehole injection a long, long time ago in the late '50s and I don't think that turned out too well. Now, there's another consideration that's called the no migration petitions which you're dealing with EPA on issues of the disposal of anything in a borehole if it's hazards of some sort. And, that's a pretty stringent operation, too.

MR. DANKER: Yes, sir.

DR. ALLEN: Further questions from the Board or staff?

DR. METLAY: In your public meetings, could you tell us to what extent this notion of a spent fuel standard has met with controversy or consensus? To what extent does it seem like a fairly sensible way from a variety of perspectives to proceed?

MR. DANKER: I can say that it for some reason really touched a nerve with a lot of people, and it was among the more theological discussions I've had. But, I think, you're safer when you back away to the words that I showed on the viewgraph which really is a perspective. It's not a standard. It's a perspective. It doesn't mean you've solved the bigger problem. It says simply don't go nuts with this theoretically smaller volume beyond the plutonium. That is also something that poses a problem in terms of diversion to weapons. So, just maintain that perspective, and if you sort of stay at that level, it's okay. But, my goodness, we got into some heated discussions. Spent fuel doesn't meet the spent fuel standard. You're shooting at a moving target. You're going to pay a billion dollars for radiation barrier that's dying off at a half-life of 30 years and it's going to--you know, there's all kinds of issues. And, quite frankly, I think one of the things that's going to help us is having the independent team of experts. After all, what we're after is proliferation resistance and give us an unbiased, independent assessment of the relative proliferation resistance offered by these forms. So, it's really a perspective.

That's an excellent question because it's not going to go away soon, and we continue to wrestle with it. As I

said in the beginning, we may not even know all the questions yet, but this is something we're struggling with because it really is at the heart of what we're trying to do.

DR. ALLEN: Okay. Thank you, Bill.

MR. DANKER: Thank you for your attention.

DR. ALLEN: We appreciate it.

Let's move on then to the final scheduled talk of this Board meeting on repository-related technical analyses supporting the disposition of surplus weapons usable plutonium. Again, Diane Harrison will be the speaker.

MS. HARRISON: I guess, this is the last leg of the Bill and Diane Show tonight.

I'm going to talk about the repository analyses that we're providing to MD and our participation on that program. I'd like to explain a little bit more how that is working with MD. I'm going to provide a high-level--I believe, this is what Carl had asked for, a high-level description of the disposition forms that we've been evaluating in the repository analyses task, describe the approach that we've taken in our analysis, discuss some of the results, and then let you in on some of the ongoing and future work that we have planned.

As Bill had mentioned, RW is supporting the Office of Fissile Materials Disposition by analyzing the feasibility

of disposing of these plutonium disposition forms in a high-level waste repository. We've developed data for the storage and disposition of weapons usable fissile materials programmatic environmental impact statement and from now on known as the PEIS. We've been conducting some of the technical analyses for the record of decision. RW is in a way a contractor to MD on this activity. MD has provided funds for an agreed-to scope of work for this activity. So, it is not at all funded out of the Nuclear Waste Fund. MD is already setting an example here, I think, of how they're very up front and willing to pay for some of the analyses.

Now, the plutonium disposition forms, Bill mentioned these. Those that are going to a high-level waste repository are either the reactor disposition forms or they're immobilization and disposition forms. There are two reactor forms. They're both spent nuclear fuel, mixed-oxide either from a boiling water reactor or a pressurized water reactor. In the immobilization disposition forms, we have the borosilicate glass, ceramic, and the glass-bonded zeolite. There are three, I'm calling them, variants within the glass, plutonium immobilized in glass. Two of them are what I'm calling a defense waste processing facility-like form because that's what we're accustomed to working with. They look an awful lot like a glass log from DWPF. The other

glass form is a can-in-canister form which Bill showed you some of the testing that's ongoing. The plutonium and ceramic is another can-in-canister form and there are two variants to that, and I'm going to show some schematics. The last form is the plutonium in a glass-bonded zeolite.

For the MD program, Oak Ridge National Laboratory has the job of evaluating the reactor alternatives and developing the reactor characteristics, and they developed and calculated some spent fuel characteristics for the purposes of the PEIS and the other subsequent evaluations. And, there is a report out that goes into great detail on the MOX, PWR, and BWR fuel. I just thought I'd throw up some of the important characteristics that you might find interesting.

For the BWR, they used a model, an existing GE BWR-5 reactor. The important characteristics; at discharge, the burnup was 37.61 GWd/MTHM, and the plutonium content about 3.4 kilograms per assembly. For the PWR, they used a combustion engineering, a CE-80+ reactor is the model, and the burnup at discharge, 42.6 GWd/MTHM and a minor 20.15 kilograms of plutonium per assembly. Fairly significant quantities.

Starting immobilized forms, the borosilicate glass plutonium form, as I said, looks a lot like a DWPF glass

cylinder. I mean, we provided--RW provided MD with our waste acceptance requirements and MD's attempt to blend in with what would be an operating repository and provide minimal impact and to utilize existing technology that's out there, you come up with waste forms that look a lot like what we're already evaluating. What we analyzed was a glass that had 84 kilograms of plutonium. They added gadolinium in this instance as a neutron absorber, about 55 kilograms. As I said, there are two waste forms and Bill mentioned them. One is an adjunct melter and one is a Greenfield alternative. The difference here is in the Greenfield alternative, Cesium-137 from the cesium chloride capsules was added as the radiation barrier. That is with the intent to provide a deterrent. For the other waste form, the differences in that, they're using the adjunct melter at Savannah River and the radiation barrier is the high-level waste.

Now, you already saw a better, a real picture actually, of the can-in-canister form. The one that we analyzed had about 51 kilograms plutonium, 34 kilograms of gadolinium combined in the whole canister total, but those were actually in--the gadolinium and the plutonium were in the glass in the small canisters here. Then, the high-level waste glass is poured around it. It provides a separation of facilities there. From the outside, as you saw, it looks an

awful lot like and would be handled a lot like a DWPF canister.

I'll ask if I say anything wrong, Bill is the responsible person in the immobilization activities and he can correct me if I'm out of line a little bit.

For the ceramic can-in-canister, the plutonium and gadolinium is immobilized in small ceramic cans or pucks or bellows or whatever you want to call them. Then, those are stacked in a smaller than a DWPF canister, about 2.4 meters by .3 meters. Then, the outer void space is filled with a zeolite material. And, I mentioned there are two can-in-canister forms and here the difference is where the Cesium-137 goes. For the Greenfield ceramic, the Cesium-137 is in the small cans with the plutonium and gadolinium. In the can-in-canister form, the cesium, the radiation barrier, is in the filler outer packing material.

Lastly is the glass-bonded zeolite, an even more complex configuration. In this waste form, the plutonium and the gadolinium and the cesium are immobilized in glass-bonded zeolite in these small pucks and these hockey pucks of glass-bonded zeolite are then placed into what is termed an ANL-W, Argonne National Laboratory West, a small high-level waste can. With a slight modification of a DWPF canister, there's no pour spout. Two layers of four, therefore eight of these

small canisters, would be accommodated within the larger canister.

In the analysis, the approach was taken that we assumed there was an existing Nuclear Waste Policy Act licensed repository. That was the basis. For each waste form, we evaluated the regulatory and statutory implications. In order to do some of the analysis, we had to develop a process flow for handling these waste forms, the disposition forms. And, we evaluated the long-term performance in the repository, specifically some criticality evaluations and some total system performance assessments. We used a comparison against commercial spent fuel and defense high-level waste as a performance measure. The idea behind this and what drove us to this was since there was no existing high-level waste repository licensed, we could not truly say whether or not this waste form was acceptable and this waste form is not acceptable. The idea being if these waste forms performed the same as or better than the commercial spent fuel and the high-level waste that a repository would be licensed for, then it would be feasible and possible that these new disposition waste forms would also perhaps be acceptable to the repository.

The results of the regulatory and statutory analysis indicated that clearly the Nuclear Waste Policy Act

permits consideration of the MOX spent fuel for disposal in a repository. It meets the definition of spent fuel. However, the immobilized disposition forms are not explicitly identified in the Nuclear Waste Policy Act. But, the Nuclear Waste Policy Act allows for certain materials to be defined as high-level waste through rulemaking. Therefore, rulemaking or clarification in authorizing regulation would be required. Some decision would be required before we could accept these materials. No special environmental or licensing requirements were identified for these forms and, of course, the NEPA process would need to be followed for disposing of the plutonium waste forms in a repository.

Real quick, of course, the 50 metric tons of plutonium and the MOX spent fuel or the immobilized waste forms would--this is part of the logistics that we had to evaluate--would require between 10 and 17 years receipt schedule of the repository. This would likely be within operational periods of any existing facility designed for 70,000 metric tons of material.

Criticality results. There were several assumptions that were made in these analyses. One, we assumed intact fuel rods, intact assemblies, and intact waste package. We assumed large capacity waste packages. We assumed full burnup credit for the principal isotopes and

assumed the waste package was flooded. Now, for the MOX BWR, that would be a 40 assemblies per waste package and we used, of course--to evaluate criticality potential, we calculated the effect of multiplication factor. For the MOX BWR 40 assemblies waste package, we calculated a k-effective of .74. For the MOX PWR spent nuclear fuel, a large waste package, contains 21 assemblies, and the calculation yielded a k-effective of 1.04. The requirement in 10 CFR 60 is a k-effective of less than .95. So, we looked at a smaller waste package, 12 assemblies, and calculated the effective multiplication factor and came up with 1.01. Since that was still above the requirement, we did a calculation for 4 assemblies of the MOX PWR and came up with .93.

This is information that we provided back to Oak Ridge and to MD for them to consider in their technical risk evaluation to look at how the design or operation of the reactor maybe could be better managed or better designed. This was an example of the iterative process or the information that we're providing back to the MD program.

For the immobilized alternatives, again we assumed intact for this analysis or assumed intact waste form. We assumed a DWPF waste package, 4 canisters per waste package. We assumed full credit for the gadolinium neutron absorber. And, we also assumed that the waste packages were flooded.

And, in the calculations of the effective multiplication factor, k-effective were all less than .95. Again, this is intact. One of the things that we learned was actually how little we know about some of these waste forms and how they degrade.

Further repository analysis results. We did do some shielding analysis. We did some structural and thermal calculations and no special significant issues were identified. Nothing came up. We did a total system performance assessment based on TSPA-93. Most of these were done last year and didn't show any significant differences when these waste forms were added to a repository for commercial fuel and high-level waste. And, this is sort of intuitive when you have such a small mass, small quantity of material, compared to such a significant inventory of commercial spent fuel. Again, the major result is we know very little about the plutonium glass and the plutonium ceramic waste forms. This is probably the most important result. We don't understand the plutonium solubility in different types of glass. We don't understand the relative dissolution rates of plutonium and gadolinium or whatever other neutron absorber is there. We don't understand the degradation modes for the glass and the ceramic logs. This is where you see where we lead onto the next viewgraph where

we have identified some of the ongoing and future work.

One of the things we need to work on are those degradation modes. How does this waste form degrade and then what is the criticality potential of that degraded waste form? That's what we're working on and we've started this fiscal year. We are continuing to interface. We plan to interface further with the R&D that's being done at Lawrence Livermore National Lab. I think, I forgot to mention that. Lawrence Livermore National Laboratory has the responsibility for development of the immobilization forms, the formulations, the characteristics, and they provide us with the data. They do the waste forms. They say, here, this is what you would get in a repository and then we do the analysis. And, we feed back to them and say we need to refine the formulation. There might be some problems. In this instance, we're saying we don't understand how these waste forms degrade or the leach rates or degradation modes. So, they are conducting the R&D program.

And, lastly, of course, we're going to continue our general support to the fissile materials disposition program to MD, to Bill. We're going to be supporting the PEIS, public review, and comment period coming up. We're going to support the second and third phase analyses that he had up there briefly, some of the technical evaluations, the

experimental work that's ongoing. And, we'll provide input to the screening of the waste forms if we identify any discriminators. Any differences in the technical risks at the repository level, we continue to provide that to support a record of decision in the fall.

DR. ALLEN: Thank you, Diane.

Are there questions from the Board?

DR. LANGMUIR: Diane, you mentioned that there's nothing much known about dissolution rates of plutonium and gadolinium and the types of glasses. I would assume that we're talking about the glass as a carrier for those radionuclides and there's, as you maybe know, 10 or 15 years of kinetics and dissolution of borosilicate glasses from long-term work at DOE and subcontractors. I would assume that those rates would apply to the same glass you're going to put plutonium in. The issue then is what form of plutonium do you create by dissolution? But, the limiting rate is going to be that of the glass itself which I think there's a lot of data on, isn't there?

MS. HARRISON: Yeah. John Bates who you're very familiar with and John Plodinec and all those people are participating and working with Lawrence Livermore. We're hoping to get some input from them on helping us develop the degradation scenarios. I think it's more the relative

dissolution of plutonium and gadolinium in the glass and the relative leach rates. How does the gadolinium--does it form anything special compared to the plutonium so that you would have the separation once the things start to degrade? Those are the sort of things that we don't--my understanding is that we don't understand. I don't have a sufficient understanding to have been able to do the analysis last year; that's for sure.

DR. LANGMUIR: One other thing that occurred to me. I profess ignorance and maybe others can help me out here. But, your canisters in which you would have these different kinds of forms, solid forms, for the radionuclides, for the plutonium, they all show a 3/8-inch thick stainless steel wall. Presumably, we're going to have the ability to mix these defense wastes with commercial wastes as a means of providing some sort of control of a thermal-loading in a repository. I would then think that it's worth comparing the degradation rates of 3/8-inch stainless steel on the defense waste to the degradation rates of the two and three layer materials that are being proposed for commercial fuel that would perhaps be buried with them in a repository. I wonder if that sort of thing has been thought about by your program. I mean, this has got to be a consideration of the DOE when you decide if it's a suitable mix or that you can understand

its performance. It's this mixed performance we're talking about now, right?

MS. HARRISON: Okay. I'm going to--I think Peter was raising his hand. Did you want to respond to that, Peter? I'm not sure I understand the question.

MR. GOTTLIEB: Yeah, I think--

DR. ALLEN: Your full name, please?

MR. GOTTLIEB: I'm Peter Gottlieb from the M&O.

I think there may be a slight misunderstanding here. There is a waste package for the high-level waste. Although the high-level waste is initially in the 3/8-inch stainless steel container, we have a waste package in which those containers are not only packed four to a container which then makes it similar in size to the waste package that we have for the commercial spent fuel. The waste package that would be used for these immobilized forms would be similar. Now, that waste package is similar in performance. It has an inner and an outer barrier just like we have for the spent fuel and it's similar in performance, although it may be made of somewhat different materials for reasons which I can go into if you're interested in a technical discussion on that.

DR. LANGMUIR: Okay.

MS. HARRISON: Okay. Thanks, Peter.

DR. ALLEN: Other questions?

(No response.)

DR. ALLEN: Staff?

(No response.)

DR. ALLEN: Well, if not, thank you, Diane.

MS. HARRISON: Thank you.

DR. ALLEN: And, I'd like to thank all the speakers on the afternoon program, both parts of it. Your adherence to schedule was absolutely amazing, at least to a college professor. We all appreciate it.

For the public comment section, I'll turn it over to John Cantlon.

DR. CANTLON: We have four people that have signed up for public comments. I'll call them one at a time. I'll recall the ground rule. Maximum of five minutes, come to one of the microphones in the aisle, identify yourself and your affiliation, and we may or may not get a response from the Board.

Mr. Steven Poole?

MR. POOLE: Good afternoon, members of the Board, staff. For the record, my name is Steven Poole. I'm with the NIEC, also known as the Nevada Indian Environmental Coalition. Currently, I serve as the environmental coordinator for the NIEC. NIEC, if you're not familiar with it, is a consortium

of 16 Federally recognized Indian tribes located on 24 reservations or colonies within the state of Nevada. NIEC is governed by a board of directors. Each board member is a duly elected leader of the respected Tribal government. I'm here today authorized by the board to speak to you on behalf of these Tribal governments. I've attached a list of the tribes that are members of the NIEC and would like to read this into the record, if I could, please.

In no particular order, the Battle Mountain Band Council, Carson Colony Council, Dresslerville Community Council, Duck Valley Shoshone-Paiute Tribe, Duck Water Shoshone Tribe, Elko Band Council, Ely Shoshone Council, Fallon Business Council, Goshute Band Council, Las Vegas Paiute Tribe, Lovelock Paiute Tribe, Moapa Business Council, Pyramid Lake Paiute Tribe, Reno-Sparks Tribal Council, South Fork Band Council, Stewart Community Council, Ft. McDermott Paiute-Shoshone Tribes, Te-Moak Tribal Council, Walker River Paiute Tribe, Washoe Tribal Council, Wells Band Council, Woodfords Community Council, Yerington Tribal Council, and finally, the Yomba Tribal Council.

The NIEC reservations and colonies represent 1.2 million acres of land on 24 Federally recognized reservations and colonies. These lands are found in a wide range of areas from Las Vegas Colony located in Clark County here to the

Yomba Shoshone Indian Reservation located in rural Nye County in central Nevada. Although similar terrains may be found throughout the region, climate, economic, industrial, and political forces play important roles in how each tribe must separately deal with environmental issues.

At your last Board meeting in January of 1995, the vice-president of NIEC's board of directors appealed to this Board and voiced the following concerns. One, the U.S. Department of Energy has refused to assist the tribes located within Nevada to address the problems associated with the Yucca Mountain Project, even though DOE has given this help to nine counties in Nevada, one county in California, and the State of Nevada.

Two, DOE has continued to ignore NIEC's request for an agreement to become a cooperating agency for the involvement of the NEPA process to address the environmental impacts of the proposed multi-purpose canister system.

Three, DOE has ignored Federal law which requires that they honor tribal rights created by Federal regulations as promulgated under the authority of the National Environmental Policy Act. These regulations require Federal agencies, such as DOE, to address impacts to tribal resources early in the NEPA process. All NIEC tribes have resources that will be affected by the multi-purpose canister system

because this canister will be used to transport radioactive material to Yucca Mountain and, therefore, tribal resources located within Nevada will be at risk.

Four, violating these special tribal rights under the NEPA regulations means that DOE has breached Federal trust obligation, their own Indian policy, and President Clinton's executive order. DOE apparently has issued its intention to develop another EIS for the construction, operation, and closure of the radioactive waste repository at Yucca Mountain. Again, this EIS will address impacts that are related to the transportation of radioactive material to Yucca Mountain. This means again that tribal resources will be at risk.

Lastly, NIEC has asked DOE to honor the Federal legal rights of the Indian tribes that are members of NIEC. We have yet to receive a response. As we requested in January 1995, we ask you again today to see that DOE follow Federal law and policy in its dealings with the tribes represented by the Nevada Indian Environmental Coalition.

Thank you for your time.

DR. CANTLON: Thank you.

The next commenter is Sally Devlin.

MS. DEVLIN: Hello, everybody. I'm Sally Devlin from Pahrump, Nye County, Nevada. Nye County is where Yucca

Mountain and the Nevada Test Site are located. I live 50 miles from the mountain and 30 miles from the test site. So, we are the down-winders, if you know what that means. Again, if you remember at the Beatty conference, I yelled at everybody every acronym I knew, and I've sure learned a lot of them over the last three and a half years. I'm back again because again I don't see DOD, I don't see NDOT, I don't see DOT, and all the pertinent things that really affect us in Pahrump. I am hoping that Louise will find our site suitable for a '97 meeting because everybody that went to Beatty went on 95, and they forget that if anything happened on 95, as my worst case scenario was presented--right, John--that you'd have to go 160 through Pahrump and we have a two lane highway and it's not going to be improved too much. The fire department couldn't even get over the hill from Vegas. We have no FEMA training. We have nothing in Nye County, and we are totally neglected and, as I say, I consider us down-winders.

I did want to say that I hope that someday there is communication between the acronyms. I was very happy that Dr. Smerdon taught me a new word, but before that--I don't know if he's here--I do want to say I'm glad he went to Beatty for lunch rather than to Lathrop Wells. And, for you that don't understand, I'll tell you later, children. And,

he taught me a new word and I love this. We are surficial deposits and I rather think that fits Pahrumpians. Isn't that wonderful? I love it. I'm going to use it when I get home.

Our concern, of course, is that we really hope you continue your expenditure in new science, old science, on the MPCs, and improvement. This is the first time I've seen about plutonium and the other fuels. I think it's wonderful because you keep sending these books by the pound and you know I really do read them. But, we are very concerned. And, I told everybody before I left Pahrump and you know that Dale was here and he's our CAB representative and everybody was sick or they don't know if they have a job. And, of course, after hearing Mr. Barnes' report, I really feel I ought to kiss everybody goodbye and worry about what's going to happen if there is going to be the '97. So, that is again one of my questions.

I always enjoy these reports and this is always a marvelous meeting and I enjoy seeing all my old friends again. One question, I really don't have an answer, but it's kind of provocative. That is if Yucca Mountain closed and this is our horror, would everything go to NTS which would be 20 miles closer and now with Bechtel in charge, which is a private corporation--and, you know I'm very political--what

would happen to us? And, I wonder would there be an NWTRB? So, I have questions. Any answers from my friends on the Board? Any rumors, anything exciting? I see my newspaper friends here. Anything for me to take home to Pahrump?

DR. CANTLON: I'm afraid not. We only deal in facts, not rumors. And, we have no facts on those issues. Thank you.

Mr. Don Shettel?

MR. SHETTEL: Don Shettel with GMI. In the past, I've been a technical consultant for the State of Nevada and Nye County. I have some comments, both essentially on one page of Jean Younker's talk. I originally thought these would be more appropriate yesterday, but perhaps they're more relevant today after hearing about total systems performance assessment.

The first comment regards colloids. Apparently, Dr. Younker thinks colloids are not important. The question is what is the basis for this statement? Experimental work at Argonne National Lab involving unsaturated drip type experiments on both waste glass and spent fuel indicates that most radionuclides are released as colloids, at least in colloidal form. These experiments did not include other manmade materials, such as canister metals or concrete. Conversely, it could be said a smaller amount of

radionuclides would be considered dissolved in the solutions in these experiments. If the DOE considers a colloid, one possibility is perhaps the DOE considers colloids are filtered out somewhere outside of the EBS. Then, this would perhaps raise a criticality issue and its need to be revisited. In any case, the issue of colloids is probably far from resolved.

The second item is also perhaps related to colloids and source term. Dr. Younker mentioned that at least she considers a canister conversion is no longer important. Two years ago and I think it was at this meeting, Dan McCright mentioned that the rate of microbially-induced corrosion is 5 to 9 orders of magnitude greater than inorganic corrosion rates. Therefore, the question is why is DOE not discussing the microbially-induced corrosion? Have they completed experiments on this and discounted it as unimportant or perhaps Dr. Younker is only referring to inorganic corrosion or perhaps there's some other reason why they're not considering it now.

Obviously, a 5 to 9 order of magnitude increase in corrosion rates would have a very significant effect on performance assessment calculations. In fact, inorganic corrosion might be considered insignificant under certain conditions. Microbes have a high reproductive rate. They

are subject to mutations including radiation-induced ones. Thus, strain or strains might quickly adapt to maximize utilization in the repository environment under certain conditions; specifically, corrosion of canister materials. Thus, the question is biological processes need to be considered in performance assessment and otherwise and these also need to be coupled with thermohydrological and geochemical processes, as well, in any performance assessment calculation.

Thank you.

DR. CANTLON: Thank you.

The last speaker is Robert Williams.

MR. WILLIAMS: Thank you, Dr. Cantlon.

Most of you recognize me as retired from EPRI now 18 months ago. Briefly, I spent 10 years at General Electric and 20 years at EPRI following fuel cycle programs. As many of you know, I love to come to these meetings to get a status report on the program, to see how the new team and the people that we've passed the baton to are persevering, and what the new problems are and whether there are old solutions to the new problems or whether there are new solutions to the old problems.

Very happily--and I hope I can persuade you in four more minutes that it's the case--that an old solution will

remedy most of the problems that were surfaced in today's agenda. The old solution is to go back to the regulatory approach that was used for the first reactors. Now, many of you have heard the buzz word "phased licensing approach" and you think it's the hair-brain scheme of Max Blanchard, Tom Isaacs, Bob Williams, and a bunch of curmudgeons back in 1989 and 1990. No, the phased licensing approach is what was used on the generation of reactors from 1960 to 1975 and basically it recommended that reactors were a new--something where all scientific data was not available. Now, I want to remind you that we're playing that same game here in repositories. This repository is the first of a kind endeavor and buy into the idea that absolute proof could be had right up front in a regulatory hearing was a mistake of major dimensions. It happened in the 1980 to '84 time frame. By then, reactors were ready to go to a one step process, but repositories weren't.

So, in my remaining time, I'd like to briefly just hit a few points on how the phased regulatory approach would address the problems of the DOE waste isolation strategy. You know, in a nutshell, Jean Younker stood up and said here are some technical hypotheses. Now, if we put them in the context of a phased licensing approach where the proof of these hypotheses can proceed and parallel with the project,

we don't have to spend three billion dollars up front. We do the same thing that the reactor designers did and say we will proceed at risk with a technical hypothesis that's conservative, and in the course of events, we will validate it with scientific data.

In the case of the scientific and expert judgment, I think all of us periodically cringe that we are engaged in an exercise of Naval contemplation that no real scientific data will be brought to bear and we'll have the best guesses of experts as a basis for proceeding. I overstate to make the point. I think that if we make the expert judgment group come up with a testable scientific hypothesis that is to be validated as part of this ongoing step-wise process, then we bring some real science. We bring the experimental method back to what is inherently a scientific hypothesis testing game.

Now, the third element of this is to address the public credibility issue. I think all of us cringe at the idea that this process and this project will be run by political decisions made in Congress with the bare majorities that will change two or three times before the process comes to fruition. So, I, for one, would like to see sort of a gentlemen's agreement that there is an ongoing regulatory process, that there is not a one-step process that then in

the view of the intervenors is a license to rape, pillage, and burn, and then the view of the runner of the repository has a license to say bye-bye, folks, I'll see you in 50 years when I want to close this thing. Instead, there is a gentlemen's agreement right up front to have an ongoing process of technical and regulatory review.

Finally, we resolve the problem of this technical basis report. Now, the technical basis report grew out of some ideas that many of us played a role in that was basically how do you put to rest a technical issue in advance of this magic regulatory hearing that keeps receding over the horizon to 20/10 or 20/20 or wherever it is. The concept was to capture, in something that was like a topical report in reactor licensing, the essence of a particular issue. Erosion was selected as a no-brainer, something that could be relatively easily accomplished. But, I think that in the context of the phased licensing process that, indeed, it was. Everybody agrees that erosion is not a process. So, the improvement to the technical basis report for erosion in the context of a phased or continuous licensing process would be one of many items that would be remedied as the program moved forward.

So, thank you for your time. But, I make this statement because I think it's crucial that DOE make this as

a central element of their presentation to the budget committees. Or, like my friend, Ms. Devlin, I wonder if we'll be back here in July or at least a year from now.

DR. CANTLON: Thank you.

We have one more speaker. Identify yourself?

MR. NIELSON: My name is Richard Nielson. I'm the director of Citizen Alert. It's a statewide public interest group. My question is for Bill and Diane. A large percentage of your presentation was based on the fact that the repository would be licensed. I don't know if that was based on expert judgment or what. But, in the event that that didn't happen or that doesn't happen, what would be the preferred alternative or the suggested method of disposition of plutonium?

MR. DANKER: On one of my slides, I mentioned that there are two of the options deemed to be reasonable alternatives for plutonium disposition that are independent of the high-level waste program. Actually, there is a third which is the no-action alternative which is keep it where it is, continued storage, which the National Academy of Science wasn't very fond of. They thought that the threat over time was a significant vulnerability. The CANDU option, part of that is--well, you know, the Canadians have their Federal waste management system, the borehole. There have been comments

about relative advantages and disadvantages of that. I can only say that there is going to be language in the draft programmatic environmental impact statement that makes assumptions about having a viable Federal waste management program, and if that is not the case, then not only our program, but a whole number of other programs are going to have significant issues to deal with.

That's, I guess, my response.

MR. NIELSON: Okay. I had one more quick question. That was in regards to the funding mechanism for the Board itself. With the decline in funding for the program and the cuts to some of the oversight of local and state governments, I was concerned about the funding mechanism for the Board and how long the Board will be able to maintain its role.

DR. CANTLON: We serve, like most agencies, at the pleasure of the Congress. We get an annual appropriation and we, like everybody else, fight for our appropriation.

MR. NIELSON: Okay. So, if the program disintegrates or goes on and on, your funding will just be at the discretion of Congress?

DR. CANTLON: Right. If this program terminates, I'm sure we'll terminate it.

MR. NIELSON: Okay.

DR. CANTLON: Maybe before.

MR. NIELSON: Well, that was what I was worried about that it would be terminated before.

Thank you.

DR. CANTLON: Thank you.

All right. If there are no further speakers, then I declare this adjourned. Let me thank all of the speakers and all of the audience. It's been a great session.

Thank you.

(Whereupon, at 5:00 p.m., the meeting was concluded.)