UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

January 25, 2000

Alexis Park Hotel
375 East Harmon Avenue
Las Vegas, Nevada 89109

ADDRESSING UNCERTAINTY
REPOSITORY SAFETY STRATEGY
SCIENTIFIC PROGRAMS UPDATE

NWTRB BOARD MEMBERS PRESENT

Mr. John W. Arendt
Dr. Daniel B. Bullen
Dr. Jared L. Cohon, Chair, NWTRB
Dr. Paul P. Craig
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek
Dr. Donald Runnells
Dr. Alberto A. Sagüés
Dr. Jeffrey J. Wong

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Robert Bodnar, Virginia Polytechnic Institute & State U.
Jean Cline, JNLV

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(Cont.)

P R O C E E D I N G S

8:30 a.m.

COHON: Good morning. My name is Jared Cohon, and I'm the Chairman of the Nuclear Waste Technical Review Board, and it's my please to welcome you to this winter meeting of the Board.

We meet as a full board three or four times a year, usually in Nevada, often in Las Vegas, and at least once a year in one of the communities in Nye County where Yucca Mountain is located. We also try to meet in Washington, D.C. once a year, but we're smart enough to choose this time of year to be out here.

My congratulations to all of you from the Washington area who made it here, and who managed to stick snow duty to your spouses.

I want to make a special welcome to those from Nevada not associated with the program to be with us here today.

As most of you know, Congress enacted the Nuclear Waste Policy Act in 1982 which, among other things, created the Office of Civilian Radioactive Waste Management or OCRWM within the U. S. DOE, and charged it. in part, with developing repositories for the final disposal of the nation's spent nuclear fuel and high-level radioactive wastes from reprocessing. Five years later, in 1987, Congress amended that law to focus OCRWM's activities on the characterization of a single candidate site for final disposal, Yucca Mountain, on the western edge of the Nevada Test Site.

In those same amendments in 1987, Congress created the Nuclear Waste Technical Review Board as an independent federal agency for reviewing the technical validity of OCRWM's program. The Board is required to periodically furnish it findings, as well as its conclusions and recommendations, to Congress and to the Secretary of DOE.

The President of the United States appoints our Board members from a list of nominees submitted by the National Academy of Sciences as specified in the 1987 law. The Board is by law and design a highly multi-disciplinary group with areas of expertise covering all aspects of nuclear waste management.

I want to introduce you now to the members of the Board, and in doing so, let me remind you that we all serve on the Board in a part-time capacity. In my case, I am president of Carnegie-Mellon University in Pittsburgh, which is my day job, as it were. My technical expertise is in environmental and water resources systems analysis.

John Arendt--and, John, if you'd raise your hand--John Arendt is a chemical engineer by training. After retiring from Oak Ridge National Lab, he formed his own company. He specializes in many aspects of the nuclear fuel cycle, including standards and transportation. John chairs the Board's Panel on Waste Management Systems.

Daniel Bullen is professor of Mechanical Engineering at Iowa State--is that a cheer? Was that for Iowa State or for Dan? Dan is professor of Mechanical Engineering at Iowa State, where is also coordinates the nuclear engineering program. Dan's areas of expertise include nuclear waste management, performance assessment modeling, and materials science. Dan chairs both our Panel on Performance Assessment and the Panel on the Repository.

Norm Christensen unfortunately could not be with us today. In addition to being snowed in in North Carolina, he's got the flu. Norm is Dean of the Nicholas School of Environment at Duke University. His areas of expertise include biology and ecology.

Paul Craig is professor emeritus at the University of California at Davis. He is a physicist by training, and has special expertise in energy policy issues related to global environmental change.

Debra Knopman, who could not be here today, but is expected to join us tomorrow, is director of the Center for

Innovation and the Environment at the Progressive Policy Institute in Washington. She's a former Deputy Assistant Secretary of the Department of Interior. Previous to that, she was a scientist in the USGS. Her area of expertise is groundwater hydrology, and she chairs the Board's Panel on Site Characterization.

Priscilla Nelson is Director of the Division of Civil and Mechanical Systems in the Directorate of Engineering at the National Science Foundation. She's a former professor at the University of Texas in Austin, and an expert in geotechnical matters.

Richard Parizek is professor of hydrologic sciences at Penn State University, and an expert in hydrogeology and environmental geology.

Donald Runnells is professor emeritus in the Department of Geological Sciences at the University of Colorado at Boulder, and he's a vice-president at Shepherd Miller. His expertise is in geochemistry.

Alberto Sagüés is distinguished university professor of materials engineering in the Department of Civil Engineering at the University of South Florida in Tampa. Alberto is an expert on materials matters, and especially corrosion, with particular emphasis on concrete and its behavior under extreme conditions.

Jeffrey Wong is chief of the Human and Ecological

Risk Division of the Department of Toxic Substances Control in the California Environmental Protection Agency in Sacramento. He is a pharmacologist and toxicologist with extensive expertise in risk assessment and scientific team management. Jeff chairs our Panel on Environment, Regulations and Quality Assurance.

In addition, we have with us today two consultants who will speak later this morning, both on the general subject of addressing uncertainty when performing complex analyses and when making decisions. Dr. Daniele Veneziano is a professor at MIT, where his interests include engineering applications of probability and statistics. Dr. Warner North, a former member of this Board, heads the consulting firm NorthWorks, which advises clients in many aspects of risk assessment and decision-making.

Tomorrow, we also have two invited speakers who may or may not be with us today. I'm going to introduce them. If they are, I'd ask them to raise their hands. Not only are they here, they're displayed. Dr. Robert Bodnar from Virginia Tech will give us an overview of the capabilities and limitations of fluid inclusion studies, and Dr. Jean Cline of the University of Nevada, Las Vegas will describe the fluid inclusion studies she is working on for the Yucca Mountain project. Glad you could be with us.

Many of you know and have worked with our staff who are seated at the side of the room there. There they are. I want to introduce to you a new face, someone who's about to join us, Dr. David Diodato. Dave, if you'd raise your hand?

Dave is a hydrologist who received his doctorate from Penn State University in 1997 and is now completing a post-doctoral project at the USGS. Dave will officially join the staff at the end of February, and we're very pleased he was able to arrange his schedule to join us for the meeting this week. We're delighted to have him with us. Welcome, Dave.

Now, let me summarize for you very briefly the agenda for the next two days.

We will begin this morning with two overview presentations. First, we will have an update on the OCRWM program in general, and then Russ Dyer will talk about the status of the Yucca Mountain Project. Our third overview presentation will give us the views of the National Association of Regulatory Utility Commissioners. NARUC is an association of the state public service commissions who oversee the electric utilities who pay a large share of the cost of this program. I understand that several of the Commissioners were here in Las Vegas, in part, for a tour yesterday of the ESF at Yucca Mountain, and we look forward to hearing NARUC's views later on this morning.

The final presentation of our overview session is a late addition to our agenda. We understand that Nye County has recently begun the second phase of its drilling program and a representative of the County has offered to provide us an overview of Phase 2 and, possibly, some preliminary results from the drilling.

Our first technical session is titled "Addressing Uncertainty." We all know that there's a great deal of uncertainty involved in making long-term projections of repository performance, but we are not all agreed on what we should do about it, specifically, how to estimate uncertainties, how to display them in ways most useful to decision-makers, and how to determine whether compliance with regulatory criteria has been achieved.

We also may be faced with uncertainties that we all know exist, but which are very difficult to quantify. A prime example is an above-boiling design for the Yucca Mountain repository. Over the past year, the Board has expressed its doubts about the adequacy of current technical information to support an above-boiling design. We are skeptical whether it is possible to project adequately the effects of high temperatures on the coupled thermal, hydrologic, and chemical processes that are important in evaluating repository performance. We hope that this afternoon's session will include some discussion of ways to deal with this type of uncertainty.

To explore the subject of uncertainty, we will have invited presentations, including those of the consultants I introduced just before, and we will have a panel discussion involving not only our speakers, but also representatives of the State Of Nevada and some of the local governments, who are those potentially most affected by a Yucca Mountain repository. We look forward to some lively and informative discussion.

Tomorrow's meeting begins with a session on the repository safety strategy. After an update on development of the strategy, we will hear talks about the principal factors and their application in seepage studies and drip shield design. That session will end with a presentation on the simplified performance assessment capability being developed by the Yucca Mountain project.

Our final session will be an update on the scientific programs that support the Yucca Mountain project. In addition to an overview presentation, we will hear about work on natural analogs, fluid inclusions, and the Busted Butte studies that support the site-scale flow and transport modeling effort.

Throughout the meeting, we understand that in the back of the room, and on the side of the room, there will be a poster display with more information about the Nye County drilling program and a demonstration of the DOE's simplified performance assessment capability. We urge you to take a look at these poster displays, which look very interesting.

Finally, let me say a few words about the opportunities we've provided for public comment and interaction during the meetings. This is something that's extremely important to the Board. We try to give the public as many opportunities as we can to participate in our meetings.

Tomorrow, we, the Board, invite you, the public, to join us before the meeting for a continental breakfast and, more importantly, some informal, off-the-record conversation, though you may find breakfast more important than the conversation. We hope this will provide an opportunity to get to know each other better, and for you to express to us any thoughts or concerns you might not be willing to express in the more formal atmosphere of our meetings. The continental breakfast will be held here in this room, and will begin tomorrow morning at 7:15.

We're planning three public comment periods during the course of the next two days, one at the end of today's sessions, that is, this evening, another just before lunch tomorrow, and a final opportunity for comment at the end of the meeting tomorrow. Those wishing to comment should sign the Public Comment Register at the check-in table where the two Lindas are stationed. Are they in the room, or are they outside? They're there. Okay. Right in the corner over there. They'll be glad to help you in signing up and being prepared to comment publicly when the time arises.

Let me point out, and I'll remind you again later, that depending on the number of people signing up, we may have to set a time limit on individual remarks.

As an additional opportunity for questions and continuing something we've tried out successfully at some of our recent past meetings, you can submit written questions to either Linda during the meeting. We'll make every effort to ask these questions, that is, the chair of the meeting at the time will ask the question during the meeting itself rather than waiting for the public comment period. We'll do that, however, only if time allows.

And, as has been clear from my review, we have a very tight agenda and it very well may be that time will not allow us to do this. If that's the case, that is, if there's not enough time during the meeting itself, we'll ask those questions during the public comment periods.

In addition to written questions to be asked by us, we always welcome written comments for the record. Those of you who prefer not to make oral comments or ask questions during the meeting may choose this other written route at any time. We especially encourage written comments when they're more extensive than our meeting time allows.

Finally, I need to offer our usual disclaimer so that everybody is clear on the conduct of our meeting and what you're hearing and its significance. Our meetings are spontaneous by design. Though this is scripted, my remarks, everything else about the meeting is not. It is an unscripted event. Those of you who have attended our meetings before know that the members, and especially these members of the Board, do not hesitate to speak their minds. But let me emphasize that is precisely what they're doing when they're speaking. They're speak their minds. They are not speaking on behalf of the Board. They're speaking on behalf of themselves. When we are articulating a Board position, we'll let you know. We'll make that clear in our comments. Otherwise, we're speaking for ourselves.

With those opening remarks out of the way, it's now my pleasure to introduce our first speaker. Most of you know that a new director was recently named to head DOE's OCRWM. The new director is Dr. Ivan Itkin, who comes to the program after a long career of public service in the state legislature in Pennsylvania and, before that, work at the Naval Nuclear Propulsion program at the Bettis Laboratory near Pittsburgh.

Dr. Itkin has a doctoral degree in mathematics from the University of Pittsburgh; a master's degree in Nuclear Engineering from New York University; and a bachelor's degree in Chemical Engineering from the Polytechnic Institute in Brooklyn; and an Honorary Doctorate of Public Service from Chatham College in Pittsburgh. President Clinton nominated Dr. Itkin on August 6, 1999. He was confirmed by the Senate on November 19th, and he was sworn into office on December 2nd.

It's my great pleasure to welcome my fellow Pittsburgher, Dr. Ivan Itkin, to his first meeting of the Board.

ITKIN: Good morning. Let me just say as the so-called new kid on the block, I'm very impressed, Dr. Cohon. You mentioned the part-time character of this Board, but I am hearing the agenda, and I can see that it's not going to be part-time over the next couple of days. And I appreciate this type of a meeting. I think it's very, very productive, and I hope that all of you who are present today feel, as well, that this is a good meeting. These meetings are important, and I hope that with the presentations over the next couple of days, not only you, the stakeholders, but also we, the Department, will gain insight as to what is important in continuing the design of this program.

Now, I wanted to first thank Dr. Cohon and the members of the Board for this opportunity to address the Board. As I mentioned, or as Dr. Cohon mentioned, I just started. I was sworn in on December the 2nd, and I guess it's not even two months that I've been in office, and I'm trying to, quick, being able to get my wings to fly, and I'm trying to catch up with all of the years that this program has been in effect. So bear with me. I'm learning. I'm on the learning curve, but my learning curve is expediential.

And I have, as I've been reviewing the scientific and technical issues of the Board, which has been addressed in recent reports and letters, and I found them to be most interesting and most helpful.

Now, I value the important independent oversight role that the Board plays in the Civilian Radioactive Waste Management Program, and I'm looking forward to learning more about the Board's concerns as this meeting progresses.

This morning, I will provide my perspective on progress of the Civilian Radioactive Waste Management Program, and the broader issues that affect the program. Russ Dyer, our project manager, will follow with more details on the Yucca Mountain Project. And later today and tomorrow, our team will discuss the latest update of the repository safety strategy and the recent progress in the scientific program. Our team will also discuss uncertainty in repository performance, a topic that I, too, will briefly address in light of its importance to the determination of site suitability.

The first topic I will discuss is program funding.

The Administration requested a total funding level of $409 million for Fiscal Year 2000. Congress enacted a total funding level of $351.2 million, about $58 million less than our request. And to accommodate these reductions, we have been reevaluating our science and engineering activities, taking into account the improved system performance and our recent changes in the referenced repository and waste package designs.

We are prioritizing the activities most important to developing the information needed to support a Secretarial decision on whether or not to recommend the site to the President. Based on the repository safety strategy, we are emphasizing those activities that most effectively address uncertainties in the performance of the repository.

The Department has developed its budget request for the Fiscal Year 2001, which the President will release on February the 7th, just a couple of weeks from now. Now, our objective, building on the momentum achieved over the last four years, remains to develop the documentation needed to determine if the Yucca Mountain site is suitable, and to support a Secretarial decision on site recommendation and, if the site is recommended, submit a license application to the NRC.

In the budget process, we have requested to makeup for some of the funding shortfalls of the past few years.

Public confidence, and that of the Board, in our scientific and engineering work is paramount to a credible determination of site suitability and the successful completion of site characterization. The timely completion of our planned scientific and engineering work is central to maintaining the confidence of the public in our efforts. I plan to communicate this theme to Congress during the upcoming budget hearings.

The next topic I will discuss is legislation. As you know, both houses of Congress considered legislation on the management of spent fuel and high-level wastes last session, specifically H.R. 45 and S. 1287. The Administration opposes H.R. 45 because it would place an interim storage facility in Nevada prior to completion of the scientific and technical work necessary to determine where a repository will be located and would weaken environmental protection.

The President has stated that he would veto S. 1287 because it would preclude the EPA from establishing standards for Yucca Mountain. Last year, Congress did not approve any legislation, and there has not yet been any floor action on these bills in the current session.

Despite opposition to the pending legislation, the Administration remains committed to resolving the complex important issue of nuclear waste management in a timely and sensible manner, consistent, however, with sound science and the protection of public health and safety and the environment.

To address some of the utilities' concerns with waste acceptance, the Secretary has put forth the concept of taking title to spent nuclear fuel at reactor sites, and he has encouraged the utilities and other stakeholders to participate in discussions on how best to implement such an idea. Both H.R. 45 and S. 1287 adopted this concept and would authorize the Department to take title to spent nuclear fuel at reactor sites.

Another broad area of activity affecting our program is other countries' approaches to waste management internationally. Our program is being closely watched on the international scene to see how the United States proceeds with geologic disposal. Two recent international meetings have reaffirmed the need for geologic repositories.

The Department sponsored a three day international conference on geological repositories last fall in Denver. In a joint statement, the delegates recognized the need for the continuation of work on the safe and secure geologic disposal of radioactive waste, and supported cooperative work to achieve public understanding of technical and safety issues related to the safe geologic disposal of radioactive waste.

The National Academy of Sciences held a workshop on disposition of high-level radioactive waste through geologic isolation on November 4th and 5th of last year in Irvine. The themes included recognition of the eventual need for geologic disposal, the importance of public participation, the role of science in policy issues, and an acceptable regulatory framework. The Academy expects to issue a report on the workshop later this year. And the timing of this report should allow decision-makers to consider the Academy's findings as a determination is made on the site recommendation.

I would now like to address some of the issues that have been raised by the Board. In November, we sent the response to the Board's August letter on the scientific investigations program. Earlier this month, we responded to the Board's November letter on the repository safety strategy, the model validation, the treatment of uncertainty, and the technical investigations.

The Board has raised two important concerns that the Department will address, that is, the need to clearly present the uncertainties associated with our projections of repository performance and the need to ensure the adequacy of the models we use to assess the overall repository performance. We agree that both issues are important to develop a credible basis for site recommendation and look forward to further interaction with the Board as we determine the best ways to address them.

At your last meeting, Acting Director Lake Barrett briefed you on our selection of a repository design concept for the site recommendation and license application. The design selection process responded to the Board's recommendation that lower temperatures would reduce the uncertainties in long-term repository performance and increase confidence in a site suitability determination. We balanced all significant factors, including long-term public safety, inter and intra-generational equity, worker safety, and cost. The details of the design continue to evolve as more details of the waste characteristics and engineered barrier properties are incorporated.

The Board has asked what time of closure the Department would assume as a basis for site recommendation. We adopted a thermal goal that the drift walls would remain below boiling if the repository were kept open for 126 years, although it could be closed after 50 years from the start of emplacement.

We are examining the sensitivity of repository performance to these thermal-related uncertainties at each of this range. Such an examination is consistent with the recommendation of the NRC's Advisory Committee on Nuclear Waste that further analyses must be done before a determination can be made on a choice between a "totally below boiling" temperature repository, and on in which some boiling takes place. For the determination of site suitability, the Department will use a range for the time of closure, with the appropriate range and thermal goals based on our analyses and the design evolution. Use of a range preserves the flexibility for future generations to determine when to close the repository.

Let me now return to one of the themes of the Academy's November workshop, in particular, an acceptable regulatory framework.

The Energy Policy of 1992 signaled a broad shift from a generic to a site-specific regulatory framework for evaluation and decision-making for a repository at Yucca Mountain. Finalizing this regulatory framework is central to determining the suitability of the Yucca Mountain site for development as a repository that would protect public health and safety and the environment.

Both NRC and EPA proposed site-specific regulations last year. The public comment periods for the regulations have now ended, and we understand that NRC and EPA are now working to complete the final regulations. The Department submitted public comments on both the NRC and EPA proposed regulations.

The Department strongly endorses NRC's proposed use of risk-informed, performance-based licensing criteria to implement the radiological protection standards. Our comments on the EPA proposal emphasized that the technical aspects of the rule should not only protect public health and safety and the environment, but also be a fair test of the safety of a repository that is demonstrable in a rigorous licensing proceeding.

The Department issued a revised proposal to amend the site suitability guidelines for Yucca Mountain on November 30, 1999, as the third leg of a site-specific regulatory framework. We modified our 1996 proposal to amend the guidelines in response to public comments, including those of the Board, and in light of Yucca Mountain site-specific regulations proposed by NRC and EPA.

The proposed guidelines use the latest analytical methods and the best science available in order to support a site suitability determination. If suitable, this determination will accompany the other information required by the Nuclear Waste Policy Act to be considered by the Secretary as a basis for a site recommendation.

Originally, we planned to hold two public hearings in Nevada on the proposed suitability guidelines last week and end the comment period on February 14, 2000. However, in response to requests from the State of Nevada and others concerned about the overlapping hearings and comment periods for the draft EIS, I decided to delay the hearings on the proposed suitability guidelines until February 2nd in Pahrump and February 3rd in Las Vegas. I also decided to extend the public comment period until February 28.

I now want to address how the program will complete the work necessary to support a determination on site recommendation. In July, 1999, we released the draft EIS, a significant milestone for the Department. We have held 18 of 20 scheduled public hearings on the draft EIS to provide the public with opportunities to receive information and comment on the draft. The last two hearings will be held next week, and the 180 day comment period ends on February 9, 2000. A final EIS will be released before the Secretary's decision on whether to recommend the site.

The program is working towards completing the technical documentation necessary to evaluate site suitability and support a Secretarial decision on site recommendation. Our selection of the next generation design concept was a significant step in this process. We have updated the repository safety strategy and refocused our site characterization efforts to reflect the impact of the selected design on reducing the uncertainties in estimating long-term repository performance.

We continue to gather and analyze relevant site characterization data, some of which you will hear about later today. Based on detailed process models that describe system performance, we are generating another major iteration of the total system performance assessment. This design, site, and performance information will be the basis of the site recommendation consideration report.

Although note specifically required by the Nuclear Waste Policy Act, we are issuing the consideration report in November, 2000 to inform the public and provide a basis for public comments. We plan to hold public hearings in Nevada on the site recommendation consideration report after it is issued. Along with the final EIS, the Secretary will then have updated information for a site recommendation report to the President, which will include technical supporting data and comments from the public, States, Native American tribes, and the NRC.

As Program Director, I plan to continue guiding the program on a sound course, building on the accomplishments of my predecessors. The program's work is now focused on the activities most important for developing the information needed to determine if the Yucca Mountain site is suitable for development as a repository and, if suitable, to support a Secretarial decision on whether to recommend the site to the President. I am confident that the scientists, engineers, and others contributing to the Yucca Mountain Project have been developing the necessary understanding of the processes affecting repository performance.

We are now developing the documentation to communicate the information we have learned. Comments from the Board on the site recommendation consideration report and throughout the site recommendation process will be essential. My goal is to ensure that this information is portrayed in such a way that answers the questions of our stakeholders, including the Board, gains the confidence of the public, and provides a sound scientific basis for decision-making.

Before I close, I would like to make an important announcement about our M&O contract. We are approaching the end of the 10-year contract with TRW, which expires in February, 2001. Although there is never a good time to recompete a complex project such as this one, we have decided, consistent with Departmental policy and Congressional appropriation intent, to recompete the M&O contract. We anticipate that the draft solicitation will be available about January 31, and public comments on the draft solicitation will be due on February 28.

Thank you for the opportunity to share my views with you today, and I will be happy to answer any questions, Mr. Chairman, if it's appropriate at this time.

COHON: Thank you very much for those excellent remarks. We appreciate it very much.

Questions from Board members?

Dr. Itkin, I wonder if you could--if you're at liberty to be any more specific about the budget requests for the program? Are you able to discuss that?

ITKIN: I really can't. February 7th, we will have the budget roll-out, and at that time, things will be more specific. Let me just suffice to say we have requested of the Department and of the White House additional funding at this critical time, and we will know how the administration views our request on the 7th of February. And then as we go to hearings on the Hill, we'll get a glimpse as to how Congress might view these budgetary desires on our part.

COHON: Thank you. Don Runnells?

RUNNELLS: Runnells, Board. Could you explain just a little more fully the DOE comments on the EPA proposed standards? Just clarify.

ITKIN: Let me say we strongly support the Administration's position that EPA, who has its traditional role as setting radiation protection standards, to be allowed to continue. We would oppose the Administration, strongly oppose, any legislation that would take that authority from EPA. We have written to the EPA, we have commented to the EPA telling them our feelings on the specific standards. We believe that the NRC range is more appropriate for the site design than what the original EPA has done.

Obviously, we are committed. We hope that we will be able to influence EPA in its final determination. Having said that, irrespective of what happens, we are bound by law to follow those standards, and we will do our level best to design a repository that would meet the EPA's requirements, whatever they might be.

COHON: Thank you. Richard Parizek?

PARIZEK: Parizek, Board. Concerning the change possibly of contractor, the M&O contractor, what sort of slippage might be involved in that in trying to meet SR schedule if this transition occurs, or a new M&O contractor is appointed? How much learning time is there? You know your own feeling about coming onto a complex process.

ITKIN: Obviously, any time you recompete, there are concerns raised about the potential for slippage. We have discussed this with our contractor. We've discussed this with others that serve with the contractor. And we have made it quite clear to them that we will not tolerate any slippage in schedule. We will work with them. We will try to provide the necessary resources to them this year to be able to meet those goals.

So although we do exhibit some concerns, we have made it clear that those that support our efforts are not to lose their sense of focus. This is too critical a year, and we have gotten, I must admit, we have gotten assurances from the M&O management that they will make our schedule.

COHON: Thank you again for your great presentation. Welcome to your position and to your first Board meeting. We hope this is the first of many.

ITKIN: Thanks very much, Jerry. Thank you.

COHON: Let me just point out it's not our custom to applaud for speaker, although it's welcome. We just don't want to start a precedent here. But it's completely appropriate.

Russ Dyer is the project manager of Yucca Mountain site characterization project. In that role, he has overall responsibility for the study of Yucca Mountain as a potential site for the nation's first high-level radioactive waste repository.

This morning, Dr. Dyer will provide us an update on the status of the project. Welcome, Russ.

DYER: Thank you, Dr. Cohon. And welcome to Las Vegas for all of you that are fleeing the weather on the East Coast.

These are the topics I'm going to cover today. Actually, I'm going to set the stage for the presentations through the remainder of today and tomorrow in these first three talks. You heard from Dr. Itkin already about what some of the FY 2000 priorities are, and I'll put a little more detail on that.

We'll talk about addressing uncertainty. Of course that's getting ready for a fairly large discussion on that dialogue. Repository safety strategy is another issue that will be discussed in considerable detail here. I'll talk a little bit about the status of the EIS process, talk a little bit about the status of the DOE rule making effort, and a little about our path forward.

Fiscal year 2000 priorities. Dr. Itkin talked about the importance of putting together the basis, the credible basis for the site recommendation, the site recommendation consideration report. That's not just a document that hangs there. It's got to be built up from a base with building blocks, and these are the building blocks that really lie under that document, or that report.

The Yucca Mountain site description, a series of analysis and model reports, the nine which, in turn, roll up to the nine process model reports. These are feeds to design and to performance assessment. The system description documents, direct feeds to design, a preliminary preclosure safety evaluation, and of course a total system performance assessment. And we're working not quite night and day, but it seems pretty close to it, trying to get those series of documents in place this year.

This is a simplified time chart just showing the major products that feed the major deliverables in this calendar year. November of 00 is our site recommendation consideration report here, the yellow.

The total system performance assessment supporting the site recommendation is scheduled for October. All of these nine process model reports, and we'll go through the acronyms at some later time, I think Jack Bailey talked to the Board before about these. We have one in house, the integrated site model. The other eight are due in, Rev. 0 is due in this spring. Those will, in turn, feed the TSPA and the site recommendation consideration report, working toward a site recommendation in Physical Year 01.

Of course that's not the only thing that we're doing in the project. The remaining things on the next couple of pages are high priority activities that I wanted to just touch on briefly. They're not necessarily listed in any order of importance, so don't get a message here that because it's the last thing, it's the least important.

Conducting testing to address the uncertainties identified in the Repository Safety Strategy, a little later, we'll talk about the Repository Safety Strategy itself, we'll talk about treating uncertainty, and then we'll talk about some of the testing program also.

We are continuing the evaluation and evolution of design, and the operational concept. We're about at the point where we've got a design we're fairly comfortable with. We're looking at what we can do by changing some of the operational parameters, in feeding that into the site recommendation design and completing implementation of quality initiatives.

We have a large volume of legacy documents and databases that have been collected over the 20 plus years that the project has been in business, and putting that-- going through all of that, putting that all into a current quality framework is a major task.

Of course the NEPA process continues. We will finish up the public comment hearings on the draft EIS and continue with the supporting activities to finalize the EIS. We will complete the public hearing process on the proposed site suitability guidelines, 10 CFR 963, and work toward finalizing those guidelines. And finally, preparation of the site recommendation consideration report for internal review in FY 00, with the report coming out in FY 01 triggering hearings in FY 01.

The next area I'd like to touch on, and this is one that we will have considerable dialogue about, is the area of addressing uncertainty. Just a few preparatory comments. Uncertainty will remain at the site recommendation and throughout the licensing process. We as a project, as a Board, as a nation, must be prepared to make decisions with due consideration of this uncertainty.

Identifying and clearly articulating the nature and significance of uncertainties is a key element for evaluating site suitability and presenting a defensible basis for the site recommendation.

We're identifying the key uncertainties through the Repository Safety Strategy and post-closure safety case. We're addressing these uncertainties through current and planned testing and performance assessment sensitivity and importance analyses.

We are considering how uncertainties can be communicated to the public, to the scientific community, and to decision makers. Some of the techniques we've looked at are the use of a simplified TSPA. I think Mark Nutt will talk to you a little later about development of the simplified TSPA, which is an attempt to help communicate this black box technology and make it a little more transparent to all involved. And it also allows the lay person to develop an understanding of how uncertainties can be dealt with in this system.

We're also developing a range of documentation to better communicate our understanding of system performance. The Repository Safety Strategy was one effort to flush that out. We're looking at a summary or overview of the Repository Safety Strategy, the documentation behind the total system performance assessment, all of this. One of the objectives of it is to help explain how uncertainty is identified, how it's treated, how it's mitigated, or how it's dealt with.

The Repository Safety Strategy is another area that we'll be focusing on a little later in the proceedings. We've talked about the Repository Safety Strategy before. We recently went through and updated the Repository Safety Strategy. This is an evolving concept that looks at and incorporates our understanding of the natural system and the evolving design and operational concept all into one overall philosophy, if you will.

Rev. 2, the prior version, documented the basis for the plans and was based on the viability assessment basis of knowledge, the design in the viability assessment and our understanding of the different physical system properties and processes that we laid out in the VA.

Whenever we updated our design through the license application and design process last spring, that brought a different system concept in, and we went back and looked at that system concept using the Repository Safety Strategy philosophy, and updated the RSS to Rev. 3. It updates the safety case, updates the plans to address key uncertainties regarding the initial post-closure safety case for the site recommendation.

It incorporates the EDA II design, our current baseline design. It includes preliminary total system performance assessment and barrier importance analyses for enhanced design. And it refines the list of factors for the safety case, and identifies a subset of principle factors for repository performance.

This, of course, is not the end all and be all. We expect, as our understanding of the system and the design concept changes, that we will also evolve the RSS. Right now, we're looking at putting out Revision 4 of the RSS in early 2001, and that will further develop the basis for the principle factors.

Now, the Repository Safety Strategy focuses our testing in areas important to the safety case. I think another way of saying that is that it identifies hypotheses that are amenable to testing, and that has been the basis for prioritizing our testing program. Mark Peters will talk quite a bit about what is going on in the testing world right now. I'm just going to talk about a few things that are our current version of the Repository Safety Strategy, identifies, as important, areas of uncertainty. And we have focused parts of our testing program on it.

The question of seepage, we've talked to the Board about before. Unsaturated zone flow and transport, that's not a surprise. I think that's been on the list since we started characterizing Yucca Mountain. Thermal-hydrologic coupled processes, a very complex field, still a lot of questions in that arena. Saturated zone flow and transport, another area that has some questions about it. Mark will talk about that, and I think you'll hear shortly from Nick Stellavato of Nye County about some of the activities going on in data collection associated with saturated zone flow and transport.

The near field environment, waste package and drip shield performance, another area; and finally, natural analogues, and you'll hear from Ardyth Simmons and John Stuckless about some of our natural analog studies.

Just some of the things that are going on; as you know, the cross drift, the ECRB, we bulkheaded off the end of the cross drift and isolated a section of the cross drift, let it return to ambient conditions to see what happened. We just went into the cross drift about a week ago. We have some of the preliminary observations from that, and Mark will talk about those tomorrow afternoon.

Nye County has been I think involved in a very successful saturated zone data collection program. This was Phase I, some of the drill holes that they put in for Phase I. They started Phase II. I heard last week that they've completed the first hole just south of the test site, and Nick will talk to you in considerably more detail about Phase II of the Nye County drilling program.

Paul Dixon is going to talk to you about some of the results coming out of the Busted Butte test in the Calico Hills, the non-welded tuft of the Calico Hills. Some of the flow and transport tests that we performed in there, we're beginning to get some of the results out of those tests.

Let me shift gears a little bit now. That was kind of a preview of what you're going to hear over the next several days. The rest of the things I want to talk about is just to touch on some of the things that Dr. Itkin mentioned in passing, the status of the EIS process. We've held 18 of our 20 scheduled public meetings to date. We've identified 1469 comments out of the 697 comment documents received as of January 20th.

As Dr. Itkin said, the comment period is scheduled to conclude on February the 9th, and the comment response document will be prepared and included as part of the final environmental impact statement.

The final environmental impact statement will incorporate changes as appropriate to reflect the resolution of the public comments, and the best available information from science, repository design, and performance assessment.

As our underlying building block documents evolve, we'll reflect that. If there are any major changes, we will reflect those changes in the EIS also.

Status of Department of Energy rulemaking. This is the 10 CFR 960, 963 rulemaking. The proposed Yucca Mountain site suitability guidelines, 10 CFR 963, were issued for comment on November the 30th of last year.

Under the proposal contained in 963, DOE may determine that the site is suitable if the required evaluations show that the potential repository is likely to meet applicable radiation protection standards for the pre-closure and post-closure periods.

On January the 14th, we announced the extension of the public comment period from February 14th to February 28th, and the hearings that were originally scheduled for January, were rescheduled for February the 2nd in Pahrump and February 3rd in Las Vegas.

As part of the other actions going on associated with this rulemaking, we'll also consult with the Council on Environmental Quality, the Environmental Protection Agency, the U. S. Geological Survey, and the State of Nevada during the comment period. And like the original 10 CFR 960, we'll need to obtain NRC concurrence prior to issuing the final guidelines.

The path forward. We are moving toward a decision on site recommendation in 2001. The main day to day task that we have in front of us is documenting the technical basis for that decision, evaluating the suitability of the Yucca Mountain site, and completing the final EIS.

There's going to be uncertainty associated with this decision, but we believe we'll be ready to take the step in the incremental process laid out by Congress for decisions leading up to repository development.

With that, Dr. Cohon, I'd like to conclude, and I'd be happy to take any questions.

COHON: Thank you, Dr. Dyer. Dan Bullen?

BULLEN: Bullen, Board. Russ, I want to thank you for the overview of what's going on, and I want to ask a quick question in response to the evaluation of the designs that you're taking a look at.

In Lake Barrett's response to our letter of last summer, I guess his letter is dated sometime in September, one of the points that he noted was that design options that increased the efficiency of heat removal will be evaluated. And I was just wondering if you could tell us where that evaluation is and update us on where that might be in the program that you've laid out for us.

DYER: Okay. Of course that's still in process. One of the things we looked at was an extended period of ventilation. We find, if you'll remember, EDA II, one of the things EDA II did several things, changing of the repository design from the A design, the emplacement drifts were spaced further apart, and we used an inside-out waste package, if you will. We also added a drip shield, and we added backfill in that design.

As we evaluated what backfill added for you, it appears that if we want to use an aggressive ventilation scheme to try to keep the temperature of the system down, backfill doesn't help you very much. So we just sent a letter to the M&O last week instructing them to pursue a design concept that does not have backfill in it, but of course there are potential impacts from that also. We've got to look at what the robustness of the drip shields would be in that environment, and so forth.

So we are pursuing it. As always, I mean, the design is evolving. Followup?

BULLEN: Bullen, Board. Along those lines, Dr. Itkin mentioned the fact that besides extended ventilation period, are there any other design modifications that are under consideration to keep the repository below boiling?

DYER: Well, I'm not sure I would call them design considerations. I alluded to it briefly. But by managing the waste stream going in, thermal management of what's going in, you can do about as much there as you probably can with physical design characteristics. And that's where our attention most recently has been focused.

BULLEN: I guess that the emphasis that the Board would like to make is that in our letter, we're very interested in, I guess the word Lake used was low as reasonably achievable temperatures, or as low as reasonably achievable design, and so those kinds of considerations should be something that we, you know, we'll ask questions about over the course of the next two days.

DYER: That's good.

COHON: Richard Parizek?

PARIZEK: Yes, Parizek, Board. Admittedly, the comments that you received 1469 out of 697 comment documents dealt with the draft EIS. Have there been any questions raised that might drive the program in a slightly different direction? I mean, obviously, that's a lot of people weighing in from different perspectives. But does that do anything to, say, the science and engineering studies that are underway and cause any modification, or are those comments specific to the draft EIS?

DYER: We haven't evaluated those comments yet. They've just been pigeon holed so far.

COHON: Jeff Wong?

WONG: Jeff Wong, Board. On Viewgraph 18, on the bottom on that bullet, I'd like your comments on what you think the term of "likely" means.

DYER: I'm sorry?

WONG: The term "likely," the repository is likely to meet applicable radiation standards. I mean, the question I have is it likely to meet it 51 per cent of the time?

DYER: No. That's the probabilistic context of the standard. We assume there's going to be a probabilistic standard.

COHON: John Arendt?

ARENDT: Arendt, Board. How are you handling the comments that you get on the EIS? Do you attempt to reach a consensus or just how do you handle all the comments?

DYER: Well, right now, the comments are coming in, and we're essentially segregating them into like topics, if you will. The actual comment resolution process dealing with the comments hasn't started yet, and won't start until after the comment period closes on February 9th.

ARENDT: I guess then my question is how do you intend on handling them?

DYER: Well, we're going to have to go through, address comments. There will probably be a process put in place where questions of fact can be dealt with pretty easily by checking something. Questions that propose different alternatives or different ways to do things will need to be evaluated. If there is merit to the suggestion, that will be raised up through the management chain.

ARENDT: Okay.

COHON: Any other questions for Dr. Dyer?

(No response.)

COHON: Thank you very much, Russ.

As I mentioned in my opening remarks, yesterday, some members of the National Association of Regulatory Utility Commissioners toured the Exploratory Studies Facility at Yucca Mountain. Today, we are pleased to have with us Mr. Greg White, who serves as Executive Advisor to members of the Michigan Public Service Commission.

Mr. White will give us NARUC's views on the U. S. program for management of spent nuclear fuel from commercial nuclear power plants, including the Yucca Mountain project.

Welcome, Mr. White.

WHITE: Thank you very much.

Chairman Cohon, distinguished members of the Board, I'm Greg White. It's my privilege to appear before you today on behalf of the National Association of Regulatory Utility Commissioners, commonly referred to as NARUC, of which the Michigan Public Service Commission is a member.

I am filling in today for Michigan Public Service Commission Chairman John Strand, and Commissioner Robert Nelson, both of whom toured the mountain yesterday, but were called back to Michigan and had to catch a very late flight back last night.

Chairman Strand serves as NARUC's Chairman of the Subcommittee on Nuclear Issues and Waste Disposal. I serve as the Chair of the Staff Subcommittee on Nuclear Issues and Waste Disposal.

I appreciate this opportunity to share some of our views on the nuclear waste program and the Yucca Mountain project. It's been since 1991 that NARUC address the Board, approximately nine years. Now is a good time for us to return to share our thoughts.

Who is NARUC? NARUC is a quasi governmental organization founded in 1889. Within its membership are the governmental bodies of the 50 states engaged in the economic and safety regulation of utilities. More specifically, NARUC is comprised of those state officials charged with the duty of regulating the retail rates and services of electric, gas, water and telephone utilities operating within their respective jurisdictions.

I would like to take just a real quick moment and introduce to you Brian O'Connell, who's handling the viewgraphs for me. Brian is the Director of NARUC's nuclear waste program office, and I'm sure many of you will have an opportunity to get to know Brian in the coming years.

NARUC has been a stakeholder in the matter of disposal of spent nuclear fuel and high-level waste since the passage of the Nuclear Waste Policy Act in 1982. We have benefitted from the work of this Board. We appreciate your work, and we hold the Board's able staff in the highest regard.

So what is NARUC's interest in the nuclear waste program? Well, the primary thrust of NARUC's interest in the program can be boiled down to simple terms. We represent the electric consumers or ratepayers who are paying for the repository program.

How so? Well, in addition to setting forth the objectives of the Civilian Radioactive Waste Management Program, the Nuclear Waste Policy Act established the Nuclear Waste Fund to pay for it. Basically, Congress and those parties participating in the policy debate in the Seventies and the Eighties agreed that the beneficiaries of nuclear power should pay for the disposal of the waste by-product. We supported that principle then, and with reservations, we support it today.

The collection of fees as payments to the Nuclear Waste Fund has been the most efficient aspect of the nuclear waste program. To my knowledge, the establishment of the standard contract with the nuclear utilities that began the fee collections is the only program deadline that's ever been met.

Ratepayers in 34 states that consume nuclear generated electricity have been paying a surcharge of 1 mill per kilowatt hour on their electric bills to the nuclear utilities, who in turn send those aggregate payments to the U. S. Treasury. To date, electricity ratepayers have paid more than $16 1/2 billion into the Nuclear Waste Fund.

In 1984, NARUC established the Nuclear Issues and Waste Disposal Subcommittee so that we could stay on top of the program and be vigilant on the Nuclear Waste Fund and its proper use.

In 1990, NARUC established the Nuclear Waste Program Office when it became apparent that just passing the Nuclear Waste Policy Act wasn't going to make things happen.

In 1993, we held a dialogue amongst stakeholders, leading to NARUC's principles of nuclear waste policy objectives, including urging development of a central interim storage facility pending the permanent repository availability.

I want to make it clear, however, that NARUC went to great lengths to avoid naming Nevada as the site. We have no interest in seeing this program forced onto another state. The science and policy must be sound. We believe that the policy of deep geologic storage is sound and is appropriate. We also think that the science is progressing very well. On this point, we need the Board's help.

As the geologic repository was beset with legal, technical and management problems in the Eighties and Nineties, not only was the 1998 mandated opening date of the repository in jeopardy, but the funds from the Nuclear Waste Fund were in jeopardy too. It seems Congress couldn't resist devoting the under-expended balances in the Nuclear Waste

Fund for other federal uses. In fact, one of the greatest threats to the proper use of the Nuclear Waste Fund is, in fact, Congress itself.

Public Service Commissions and NARUC became distressed when it became apparent that DOE would not meet its obligation to start taking waste in 1998.

In 1994, we, along with the group of utilities filed the first of a few lawsuits against the Department of Energy over this program. I don't have time to go into the details of those lawsuits, but I can summarize by saying we only filed that suit because we were compelled by DOE's statements that they were not obligated to take the waste from the plant sites under the terms of the Nuclear Waste Policy Act.

The status right now is that the series of lawsuits that ensued has resulted in something of a stalemate. The courts have ruled that DOE is obligated to take the waste, but the courts have also refused to compel performance.

So, really, where are we? Well, it's become something of a discretion of the Administration and Congress as to when this waste will begin to move.

NARUC is also actively involved in the review and comment on important federal documents related to the project, such as the EPA's proposed radiation standards, and the DOE's DEIS. In both the radiation standards and the DEIS review of such technical matters as repository design, we are not always in a position of technical expertise. Instead, wee look to the DOE and its technical support contractors and consultants, the Nuclear Waste Technical Review Board, and ultimately the NRC to each provide a form of defense in depth in designing and eventually building the project and ensuring the best near and long-term public safety that is practically achievable.

I'd like to give you very briefly a few comments on our impressions on Yucca Mountain. As Chairman Cohon indicated, we did tour the mountain yesterday. I'd like to first say thank you to Dr. Itkin, Dr. Dyer, Alan Benson, and in particular, Dr. Michael Voegele, who provided the tour for us. It was an excellent tour and we appreciate it very much.

Having been to the Yucca Mountain site in 1994, it appears that the repository program is making real progress at last. It certainly is an isolated location, far more so than the 77 locations around the country where nuclear waste is stored awaiting safe, permanent disposal. The team of professionals focused on the site characterization work are well qualified and dedicated to their task.

We are very concerned about the M&O situation and the Yucca Mountain project. And I'm not referring to the old "who's in charge" problem that existed in the Eighties and early Nineties. Rather, we are distressed that at this critical juncture in the program, a decision has been made to recompete for the M&O. We're possibly changing the M&O now, only two years from the site suitability assessment and recommendation.

In closing, let me conclude by leaving you with the following thoughts. In 1982, the Nuclear Waste Policy Act was to be the final solution. Yet today, what we have is uncertainty. We have uncertainty over the availability of the Nuclear Waste Fund. We have uncertainty over the budget appropriations. We have uncertain radiation standards. We have uncertainty in the licensing process. We certainly have uncertainty in the courts.

In Congress, the debate seems to be digressing. We don't see the focus in Congress right now so much as how do we solve the problem, but how do we find ways not to take the waste.

In some of the bills that Congress has been considering and that are being debated in Washington, the objectives seem to be how do we limit the federal government's liability for its failure, and also implementation of the take title. Take title, and I have a number of reasons why we oppose take title, is not supported by a single state that holds a commercial nuclear power plant.

As I indicated, there is one certainty in the program, and that is litigation. Every conceivable lawsuit will be filed that will serve to delay this program. I may be so bold as to say that today in the year 2000, we may be further from removing waste from the plant sites than we were in 1982 when we passed the Nuclear Waste Policy Act, which was intended to be the final solution.

We believe that perhaps the best thing going for this program right now is the science in Yucca Mountain. There is progress being made out there. On this point, we need the support and the help of the Technical Review Board to keep that project moving forward.

That concludes my comments, and I would be glad to answer any questions that you may have.

COHON: Thank you, Mr. White. Any questions from Board members? Paul Craig?

CRAIG: Craig, Board. You made it very clear that NARUC would like the fuel to be moved from the present sites. On the other hand, when you move it, you can move it to a temporary location, which might or might not be in Nevada, and you can move it underground. Could you explore with us a little bit the NARUC viewpoints on moving it to temporary locations, and the NARUC viewpoint on moving it underground? Does NARUC believe it's important that it be moved underground rapidly?

WHITE: Well, we believe, first of all, that the Act as amended does authorize the Department of Energy to move the waste to an interim storage facility.

As I indicated, we have never said that we believe that that should be to the Yucca Mountain site, although obviously the bills that have been before Congress suggest that that may be appropriate.

We have concerns that the 77 sites that currently have waste were never intended for long-term storage. And we understand that this Board and others have indicated that it is safe to store the waste at those sites until a permanent repository is available. However, by doing so, that exposes the ratepayers to additional storage costs, and increases the environmental risk.

The ratepayers of this program have paid for the original design storage at the plants. We also are paying very regularly in the Nuclear Waste Fund. We have now had to pay a third time to expand the storage at the sites, and in some cases move to the dry cask storage. This is no small cost.

Because of the uncertainty in the program, we have real concerns that we may run into a situation where the waste will be at the plant sites, there will be no money for the program, and this program will not be in a position to move the waste to Yucca Mountain.

Under that scenario, we believe that it makes more sense to have one well designed, well regulated facility operated by the federal government, rather than the situation that we have now, leaving the waste at 77 sites around the country.

COHON: Dan Bullen?

BULLEN: Bullen, Board. There's an initiative in the nuclear industry for private storage, but you didn't mention that. Does NARUC have an opinion on the efforts by the industry to develop private storage?

WHITE: Yes, we do. We are supportive of those. We don't think that they should be discouraged in any way. We actually have been working initially to follow, for example, the Mescalero effort. We have brought in speakers and talked regularly with folks from the Owl Creek project in Wyoming, and also the Skull Valley in Utah.

We would like to encourage those projects to the extent that they can help alleviate some of the concerns that we have. We would certainly be supportive of those.

COHON: Thank you. Richard Parizek?

PARIZEK: Parizek, Board. Were you promoting lawsuits? You say one thing you could guarantee is there will be lawsuits. But then you said this puts us further away from actually implementing a waste isolation program by deep geologic disposal. So it seems like if you push the one and it delays the program, that's counterproductive. On the other hand, it's forcing decisions. I see two stories here.

WHITE: Well, I appreciate the opportunity to clarify that. As I indicated, we didn't want to file lawsuits in the first place, but we felt compelled to do so. We requested in 1993, we sent a letter to Secretary of Energy Hazel O'Leary. We asked the question when can we expect the waste to be moved. The response didn't come back with in the year 2010 or anything like that, but rather, the response came back that we don't feel we're obligated to remove the waste absent a permanent repository. We felt at that point that our rights needed to be protected in court, so we filed that lawsuit reluctantly. Subsequent lawsuits were to try to seek performance.

No, I see us, the states and NARUC as having run the course in litigation. What we see when I saw there will be lawsuits is I fully expect the State of Nevada, other parties who are opponents to this project will use ever legal means necessary to try to delay the program. That's what I was referring to when I saw that we see certainty that there will be lawsuits. They won't come from us, but we feel they'll come from opponents to the program.

COHON: John Arendt?

ARENDT: Arendt, Board. In your second viewgraph, you indicate that you're not an advocate for nuclear power. Is that a unanimous decision, or is it a consensus?

WHITE: It's a consensus. Certainly there are commissioners, we have many, many commissioners representing the 50 states. Some commissioners would strongly advocate for nuclear power. Some commissioners are strongly opposed.

What we've tried to do is remain neutral on that subject and focus instead on our responsibility to the ratepayers to see that the waste be removed as we have paid for.

COHON: Thank you very much, Mr. White. We appreciate your being with us.

Our final presentation for this overview session will be an overview of the second phase of Nye County's early warning drilling program, which as we heard before from Russ recently got underway. Nick Stellavato, who directs the program will tell us about the plans for Phase II, and we hope some results if you have them, Nick. Welcome back.

COHON: Nick, just let me remind you we didn't leave you very much time for this, only 15 minutes. So--

STELLAVATO: I've only got an hour.

COHON: You have an hour? We do have to keep the schedule. So thanks, Nick.

STELLAVATO: I'll keep the schedule.

I just want to hit on three different things real quick, and I want everybody to look on the wall, because I've got a lot of detail on the walls of this. But as our aeromagnetic initiative, I have to mention this because it's helped design and locate our wells, and you can read the detail, but this was a cooperative effort of Nye, Inyo and Clark County, and with the USGS out at Mineral Park, Rick Blakely.

We finished 14,500 line miles of aeromagnetic survey, and we will have the final report done in the next week or two, Rick Blakely will. But one of the big points of this is we wanted to thank the Nevada Test Site for--they let us fly with the Canadian contractor over the Nevada Test Site, which was a big kudo, we thought, and gave us some datasets that we hadn't had before. And we used this in designing the Phase II and Phase III EWDP.

You have this, but if you look on the wall, you can look at it in bigger detail, but this is pretty spectacular data, I think, and when we get the final analysis, this is looking at the magnetic profile survey of the entire area down to Sandy Valley, down past Pahrump, up to Calico Hills and up past Beatty. And as you can see, there's some pretty striking subsurface features showing up due to the magnetic anomalies. And you can see we're drilling in this area right here. We do have some buried volcanic cones that popped out that we're going to be looking at in the future.

And this is closer up of the Yucca Mountain area, and you can see Yucca Mountain is this area right in here.

We see some major anomalies. This east/west structure, you can see truncating at the southern end of Forty Mile Wash right along the Highway 95, which corresponds to the Carrara Fault or the 95 fault that people have talked about.

We have the north/south structure through here which corresponds to the old Ike Winograd's gravity fault system, and then the Rock Valley system coming in from the Nevada Test Site, which all terminate right here where there's a big buried volcanic cone we see.

So we have a well located right in this because we wanted to see how much water we could produce now. We don't know if this has filled that where those two faults intersected, or if it's resolved with those two faults. So we'll be looking at that in the future.

But the important area is right in here, down Forty Mile Wash, and as you can see, I'll show the next initiative, we're going to locate some holes right in here, as we've already done, and then you can see the--we're going to investigate the major flow paths off of Yucca Mountain.

Now, this is the latest version of the map. It seems to change daily. The blue are the wells that we're going to be trying to do this year. The red wells we finished last year. That was Phase I. And we got a good picture and we know we have to go deeper to get to the carbonate, so we've come back in and you can see 3DB right here at the three wells. That's going to be our carbonate test well. We had to come in and put in a bigger hole so we could go deeper, and we have a rig coming in that will go to 6,000 feet. So we're getting preparations for that.

We also put another well right here at 2DB because we want to take that down to the Paleozoic carbonate also. We finished this hole to 500 feet. We finished this hole as of last night to 500 feet, and we cemented the casing in and we've logged both this hole, and we will be cementing the casing in that hole and we'll be ready to drill into the carbonates.

We finished this. On yours, I think it says 4S1 and 4S2. We dropped those wells, 4PA and 4PB, and those are piezometer holes. We finished those two holes. We logged that and we're completing 4PB, and we're looking at the clay bed in Forty Mile Wash, and we have this gravel and sand channel. We've got two waters in 4PB, and 4PA is only 500 feet, so we're going to look at the one water zone and two completions in 4PB so we can look at the impact of pumping on the sand, those channels across the clay and see if they're--we know that the clay is confining in Forty Mile there, and the water, wee hit it at about 460, and it comes up to 350 after we hit the water. So we know the clay is acting as a confining bed, and we know that all the production down in here and over in this area, they go down to 800, 900 feet, and then the water comes up and that's where they have to pump, the water comes up to 350.

And I just talked about this one. You can read that. You can read those and it will give you a little more detail. Although we did finish the second hold, that 2DB hole, it's ready to set on with the big rig and start drilling.

And one other initiative, we felt, Parvis and Tom Buco and Dave Cox, the transport in the alluvium has been a big concern, and I know I've talked to the NRC about it, and so as part of our cooperative agreement, we're right now in the process of modifying the cooperative agreement to put in an alluvial tracer complex so we can, in cooperation with the DOE and all the labs, the national labs, the USGS, the M&O, and then the Harry Reed Center and Nye County, and what we decided upon in working with Russ Patterson with the Department of Energy is our first test location we're going to put is right in that square on the south, just off the southwest tip of the Nevada Test Site.

We picked that site, for one, it's going to be in one of the main flow paths off Yucca Mountain. It's right in the Forty Mile Wash, right off the edge of Forty Mile Wash, and we're going to orient this parallel with the Forty Mile Wash so we can pick up, probably be a worse case scenario for transport, and instead of putting it over in this area where you're mainly in clay. And one of the requirements, they want a thousand feet of saturated, so we're looking at 1,500 feet for the depth of the holes, and since we hit the water at about 350 feet, we'll have water, so at 1,500 feet, we should have a thousand feet of saturated alluvium.

So we're going to drill in the second phase of the EWDP, we're going to drill 19D, which would turn out, if the well is good, to be the pump well for the tracer complex. And we're going to also put in a 19P, which is a piezometer well that we get our samples, because we have to make a bigger hole for the pump, and we want to make sure we get samples down through the first 500 feet, so we'll put that in and that will give us another well for monitoring also.

So we'll finish up the 19D hole this year, and the 19P hole, and then do some single hole tests and possible single hole injection pump-back tests. The USGS and Los Alamos will be doing that work. Nye County, as part of the cooperative agreement, we're--that was one of our holes anyhow, and so we're going to use it as a long-term monitor when they're done with the testing.

So where we are on that is we've done the equipment specs. We've got all that. We're working on the modification to the cooperative agreement, getting the program approved. Since 19D and 19P were part of the EWDP Phase II, we'll have those wells in and we'll have the hydraulics and the stratigraphy and everything on those done.

What has to be done, the UIC permit has to be modified. It's DOE's permit. They're going to do the injection. We're going to do the drilling and reap the rewards of the data, but we're not going to do the injection part of it. DOE and their contractors will.

We've initiated the BLM right-of-way, and we've had problems. We're right now struggling with the EWDP Phase II because we haven't got the right-of-way yet for our new wells because of the UIC. So we've done the initiative. We've pulled the UIC permit off of our right-of-way permit so we can go ahead and drill our wells, or if not, I'll have to shut down if I can't.

Then we've identified the logistical requirements, and then piezometer hole by the end of February, but probably it would be sooner if we get the right-of-way by the end of this week, and then we'd have 19D done probably a little sooner than the end of April, too, if we get our right-of-way and get going.

So that's about it, but I've got to show you one slide, since I have a little bit of time. We did set sort of a record this year this last week with the 4PB hole. With the hammer rig that we've been using, it's a reverse circulation hammer rig, we set a record for this hammer rig. It's never been down--we took it to 900 feet, and it's a dual wall reverse hammer, and I think that's a remarkable achievement for this type of rig, and it actually hammers the dual wall into the ground, and it's perfect for drilling Forty Mile Wash with all the alluvial valley fill material. And it makes a wonderful hole for a piezometer hole, and for our completions.

So I'll answer any questions if you have any. I went real fast, but I want everybody to come out and take a look at what we're doing, especially when we get our new big rig in in the spring. It's a 6,000 foot top head drive, dual wall rig. So we'll go down and we'll get the carbonate somewhere this year.

COHON: The last time we were out there, it was, I think, 116 degrees. Can you promise that again, Nick?

STELLAVATO: It will be a little cooler, probably about 110.

COHON: Thanks for keeping it within time. I appreciate that. Richard Parizek?

PARIZEK: On your new drilling capability, how badly disturbed are your samples? I mean, obviously, this all comes up in a mix, and then you have your physics to kind of characterize what it might have been like in place. But in terms of understanding just the sedimentation patterns in the alluvial fan environment, you lose a lot of that just by the drilling technology and the way in which you have to get the holes down. And so the program has to deal with this question of how variable are alluvial fans, and at what depth and spatially as you go down the wash, and the drilling program sort of causes difficulty with that characterization.

STELLAVATO: Yes and no. And if you take a look at our stratigraphy section, I don't know if anybody has seen those, this is what our geologists are putting together for every hole we do. And I think we're getting closer to understanding, you know, what we're losing in the sample and why, and I think with the system we've got set up right now, we can pick up the fines also. With this rig we've got working right now, we can pick up the fines.

We know we lose some fines, but we think we can characterize the clays in the valley fill material with the system that we're doing. We think we can do a good job on it with the logging, and then some of the other tools we're looking at, the down hole digital camera we're going to be running this year, in any hole that will stand up over ten minutes, we'll try to run that. So that will be another tool to help us.

PARIZEK: Right. Are some more samples being taken for KD purposes from the current drilling, or is that program--

STELLAVATO: No, Los Alamos has done a lot of work, and I think they've got some posters up here on some of the work they've done with the cuttings, and with some very interesting results. So, you know, we get plenty of cuttings, and maybe lose some of the fines, but I don't think that has affected their KD studies. So they've done some in lab studies, and I think Harry Reed may be doing some work on it, too, on the cuttings.

COHON: Don Runnells has the last question.

RUNNELLS: Nick, last time, everybody was sort of excited about the elevated temperatures of the groundwater. Can you give us a quick update on temperatures of groundwater?

STELLAVATO: Well, we really haven't drilled down into them again. I assume we're going to go down to 3,000 or 4,000, 5,000 feet at the 3D location, which is--let me put that map back up real quick--that 3D location was where we really get the hot water. 15D on that map will also be a hot well. It's closer to the Lathrop wells cone than 3D is. So we expect to get elevated temperatures in 15D. I don't know if they're good enough for Secretary Richardson's geothermal initiative for Nevada, but we'll keep an eye on that.

I know it was hot enough that we couldn't keep our O-rings in our dual wall. It blew them out all deformed. But we'll look at that in 15D.

COHON: We have another question from David Diodato.

DIODATO: Dave Diodato, Board Staff. I was just wondering in the course of your drilling, is there an opportunity to take some water quality samples along the way as you encounter the saturated sediments, and that way to gain maybe some understanding of natural geochemical evolution and residence times for these groundwaters?

STELLAVATO: Yeah, we do take a lot of water quality samples, and I think you'll see some USGS, some water quality hydrochemistry. We've done two complete samplings. We use the West Pace System, so we can isolate specific zones, and that's where we pull our samples out of those specific zones, and it's worked very well so far. We pull through sleeves that we open in those zones, and we don't look at combining composite chemistries. We look at individual chemistries from specific zones.

DIODATO: Well, then to kind of follow up on that, with maybe the isotope data you've got, what kind of ages are you getting for the groundwater there then?

STELLAVATO: Zell? What kind of ages are you getting on the isotope?

PETERMAN: Zell Peterman, USGS. We've collected samples for both dissolved ion chemistry and isotope stable and radiogenic isotopes and radiocarbon. I don't think we have any radiocarbon analyses back from the Nye County samples yet. We have analysis back from our more southerly collection from the Amargosa, but I don't think we have any data from the Nye County samples yet. But we will have.

STELLAVATO: We do have some samples, but I can't tell you what the numbers are. I've been worrying about budgets and not sample numbers. Oh, there's Don Shettel, he's here.

SHETTEL: I'm Don Shettel with Nye County. Radiocarbonates so far indicate they're uncorrected in appearance so far, but 10,000 to 40,000 years.

PETERMAN: Thank you. 10,000 to 40,000, okay. Thank you.

COHON: Nick, just one last question. It seems like you're getting good cooperation from DOE; is that the case still?

STELLAVATO: Yes. I think this has been a cooperative effort with everybody, and everybody is sharing in the data. I know Linda with the State has used a lot, taken our data. We try to get her data, and the DOE has been very cooperative, and the labs and Harry Reed Center, you know, they just leave me alone and let me work.

COHON: Well, we congratulate you on the creativity and the intelligence that this program shows. It really is very nice stuff, and we thank you for being with us and keeping your remarks within time.

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

(Whereupon, a break was taken.)

COHON: Thank you. The second session focuses on the question of uncertainty, an issue that came up in the first session, and one that's very important to the Board and to the program.

As Russ Dyer observed, and Dr. Itkin did as well, the uncertainties associated with the Yucca Mountain site is unavoidable. No matter how long we study this site, no matter how much information we get, no matter how smart we become in our modeling, uncertainty will remain. That means that the program needs to deal with it, as Russ observed. It needs to figure out how to make decisions in the face of that uncertainty, and how to communicate that uncertainty to the public and to other interested parties.

For the Board, uncertainty is a central issue. For us, it is inseparable from the definition of suitability, one, we believe one cannot determine the suitability of Yucca Mountain without dealing explicitly and head-on with the issue of uncertainty.

That's why we put together this session, and why we're very excited to hear from our consultants and from the program and from NRC, and from the panel discussion that we'll have this afternoon.

Let me introduce them to you again. I mentioned our two consultants this morning, but let me tell you a little bit more about them.

Daniele Veneziano will be our first presenter. He's professor of Civil and Environmental Engineering at MIT.

His research interests include engineering application of probability and statistics, risk analysis of structural and geotechnical systems, and experimental design and data analysis. His presentation today will be entitled "Uncertainty Types, Their Assessment, and Decision."

Warner North will be our second speaker, and I know he's familiar to many of the people associated with the program because he's a former member of our Board. He's been a practitioner of decision analysis and risk analysis for more than three decades, and has carried our applications of decision analysis and risk analysis for electric utilities, the petroleum and chemical industries, and a variety of government agencies.

Dr. North's past membership on this Board and his more recent activities with the National Research Council's Board on Radioactive Waste Management give him a unique perspective from which to view the Yucca Mountain project. Today, however, in his prepared remarks, we have asked him to speak more generally about "Decision-Making Under Uncertainty." And later this afternoon, we hope he will be able to give us more specific views on the Yucca Mountain project during the panel discussion.

Budhi Sagar is the Technical Director of the Center for Nuclear Waste Regulatory Analyses, a federally funded research and development center sponsored by the NRC, that is, the Nuclear Regulatory Commission. Dr. Sagar is responsible for managing the technical work that supports the NRC's oversight of the DOE's Office of Civilian Radioactive Waste Management, especially the Yucca Mountain project.

Dr. Sagar's presentation is titled "Regulatory Views on Uncertainty in Licensing at the Yucca Mountain Repository."

Before Dr. Sagar makes his presentation, Joe Holonich of the NRC staff will make some introductory remarks.

Following the NRC presentation, we'll hear from DOE and from Abe van Luik, who in his position as policy advisor for performance assessment, Abe is responsible for helping determine and integrate the scope of, and approach for, analyses of geologic disposal system performance. Today, Dr. van Luik will tell us how the Yucca Mountain project is addressing the uncertainties associated with a potential repository at the site.

With that, it's my please now to call on our first speaker, Dr. Daniele Veneziano.

VENEZIANO: Thank you very much.

I'm going to talk about three topics. One is uncertainty types, different types of uncertainty, the quantification of these uncertainties, and how you use these uncertainties for decision. And that's a rather formidable task given the time that I have. But I'll try to at least point out some important issues related to these areas.

First of all, uncertainty types. There are many types of uncertainty, but for the purpose of this presentation, I thought that using the coarsest possible classification of uncertainty types would suffice. It's a classification that considers just two different types of uncertainty, and many different names are being tagged on these two different types. In order not to use jargon, I thought of calling them just Type I and Type II.

Type I uncertainty is an uncertainty that reflects the variability in the outcome of a repeatable experiment. This has been also called frequently aleatory uncertainty. I'll call it Type I uncertainty. An example are the kinds of games that you can play in this town are of this type, also, if you measure, say, daily temperatures or if you measure the maximal annual wind speed at a certain location over different years. In all those cases, you have a repeatable experiment, and each time you perform the experiment, you have a possibly different outcome, and uncertainty reflects this variability of the outcome.

What are the main characteristics or attributes of this Type I uncertainty? The objective, as it has a relative frequency interpretation, everybody cannot agree to it, how to measure it and how to define it. It is also independent of time. If you come back to Las Vegas next year, you'll have the same chances of winning or losing your favorite game. It doesn't vary with time.

It can be quantified, but not reduced by gathering information. Okay? And, finally, we know that probability theory applies to it. In fact, probability theory has been designed, constructed exactly to deal with this type of uncertainty. So we are on surer ground, in a way, on objective grounds with this type of uncertainty.

Unfortunately, it doesn't cover very many situations.

Most of the uncertainty we have to deal with is of a different type, which I call Type II. And Type II uncertainty is uncertainty from ignorance, sometimes call epistemic uncertainty. We'll call it Type II uncertainty.

We are ignorant about certain things and, therefore, we are uncertainty about them.

And here, examples abound. You can make an enormous list of examples, again, because this is the typical uncertainty that you encounter. And here are some examples. Does God exist, or is the accused innocent or guilty? When did the French revolution start, and so on. What is the conductivity of a given aquifer? Is a fault seismically active, form and parameters of probability distributions.

I had listed these examples, and in fact divided them into three different groups. At the top, you'll find examples of cases of one of a kind situations, one of a kind events. And in this case, uncertainty is very subjective, is very personal. It depends from individual to individual, because the state of information, or if you wish, the system of beliefs, like purposes, is different from individual to individual. We are in a world of very great subjectivity here.

On the other hand, if you go to the second group of examples, like the conductivity of an aquifer, is a fault seismically active, here you may at least think that there is a population of aquifers that are similar in some respects to the one you are interested in. You had experience with some other aquifer of a similar nature, and you can use that experience in order to at least quantify at least in your mind, and maybe communicate, and objectively assess uncertainty.

The same is true for faults, to the degree that you can refer to a population of seismic faults. But there is also a certain degree of subjectivity, as not everybody may agree with your definition of this population of the difference between different specific faults, different specific aquifers.

Finally, as you go to, say, uncertainty on the parameters of a probability distribution, this is fairly objective, and there are very well established methods to assess uncertainty on distribution parameters. This is the subject method of most statistic theory, in fact. And without getting into details, we can call this quite objective, an objective Type II uncertainty.

So what are the main attributes of this Type II uncertainty? First of all, it depends on the amount of available information. That's very important. In fact, as a consequence of that, it means that it can be reduced by collecting more information. And also, it seems this information varies over time, because new theories, new models, new computation, new data, et cetera, are acquired over time, this state of uncertainty, uncertainty of Type II, evolves over time.

This is a very important point, and I'll come back to it when we talk about decision in the context of uncertainty of Type II. This uncertainty evolves over time. We can talk about uncertainty today. We know that tomorrow, we'll have a different state of uncertainty.

Also, I have already pointed out that it is often, but not always, subjective, and these examples, I guess, illustrate that point.

All right, so we have these two major types of uncertainty. How do we use these uncertainties in making decisions? That's the second point that I would like to make.

I have sketched here a relatively idealized decision process about some uncertainty, some matter of which we are uncertain. And I have distinguished two phases of decision. The first phase is the one enclosed in this box, and you may call it due diligence. During this phase, you are collecting information. You are wanting models, you are involving experts, you're at least getting experts' opinions, in order to reduce as much as possible Type II uncertainty in order to quantify both Type I and Type II uncertainty.

So this is the phase of research, if you wish, of science or information collection, and so on. And you exercise this possibly repeatedly in a neat fashion until you are ready to make the final decision to license the process, to develop the project, to accept it or reject it. That's the final decision.

Now, I would like to say something about the quantification of uncertainty during this phase, and the use of uncertainty in this last phase, and I'll start with the last phase, because we have to learn what kind of uncertainty we need to make this final decision, so that we have tried to make that characterization during the first phase. So I'll start from the end point, and see what it is that we need to make the decision, and then we'll see how we can get what we need in the first sort of fact-finding phase.

So let me start from the final decision, and the question, the first question that I want to address is it doesn't matter if uncertainty is of Type I or is of Type II. It doesn't matter to a decision-maker. And I start here with a very simple example, it is not really general enough to cover the issues that are in front of this group, but I'll go to a more general set of theory in a moment. But let me start with an example.

And the example is this simple problem. Consider tossing a coin, you have a coin, you consider tossing a coin, and you compare two betting situations. One is before tossing. I haven't tossed the coin yet. It's a fair coin. It has even probabilities of being tails or heads. In this case, we want to see what is in fact our sort of betting odds in this case. And the other case is I toss the coin, so I toss the coin, here it is, but I don't show you the outcome. I say now bet.

What is the difference between these two situations? Well, you might say I have the same probability that it is heads, in fact, in one or the other, 0.5. In fact, my betting attitude is the same, and that's correct. That is true.

Let's examine the uncertainties you have before I toss, and after I toss. Before I toss, all uncertainty is of Type I. Okay? You are repeating the experiment many times, et cetera, and in fact, we know that the probability or the relative frequency of heads is 0.5. We know it with certainty, with probability one. We know that it's 0.5. It's a fair coin, we can argue, or we can make many statements to demonstrate that. We have all uncertainty of Type I.

In the second case, I have flipped the coin and I say the outcome is either heads or tails, one or the other. It's like what is the geologic profile here? The process has in fact generated a geologic profile. If I don't see it, I'm uncertain about it in the same way as I'm uncertain about the heads or tails. But in reality, there will be a single geologic profile that will be either heads or tails. So the true state of nature will be either that there is heads here or there is tails, or in the balance of relative frequency, the amount of frequency of heads is either one or is zero. It's either one or is zero, but I don't know what it is.

And so I place probability 0.5 on the fact that I have heads, and probability 0.5 on the fact that I have tails. But the uncertainty in this second case is of Type II, is due to my ignorance. If I could believe the hand, I would know. The answer is there. But it is due to ignorance. So all uncertainty in the first case is of Type I. All uncertainty in case two is of Type II. In the second case, it's Type II. And yet our betting situation is identical.

These two uncertainty situations, here we know something about the relative frequency. Here we are very uncertain about the relative frequency. They're as different as they could be from a distribution point of view. Here is very narrow; here is very broad. But these two distributions share a single characteristic. They had the same mean value. They had the same mean value, the mean frequency here is 0.5. The mean frequency here, of course, is 0.5. And this illustrates a fact that in making decisions, all that matters about your Type II uncertainty is the mean value.

So you are correct in placing your bets in the same way in the two situations, because the mean is the same. The spread here, this uncertainty does not matter, at least in this particular problem.

Actually, though, I have two reasons why it doesn't matter here in making decisions before or after tossing. One is that the mean value is the same, and the other is that it is impossible, due to the rules of the game that I have described, to change the state of uncertainty by maybe making an x-ray or peeking or trying to find out what really the outcome of this particular toss was. These are two very important conditions under which the mean in fact is the only thing that you have to care about. This second condition is very important.

Now, let me generalize from this simple example to more general situations that would be of greater interest here. Suppose that the final decision depends on the relative frequency of an event, like the release of a hazardous substance. Okay? The relative frequency here is F[A] of that event. And due to ignorance, we are uncertain about F of A. This is the type of problems that you are dealing with here.

What does the decision theory say? It says two things. If uncertainty on F[A] cannot change during the lifetime of the project, if it cannot change, we do not then gain new information, new aspects, new models, new hypothesis, new anything. If it cannot change, then all that matters is the mean value of F[A]. All that matters is the mean value of F[A]. So just reporting, just using the mean is sufficient.

However, if the uncertainty can vary, if uncertainty can change during the lifetime of the project, then the temporal variability of the mean should bee considered.

I have tried to illustrate here why this second statement is true with sort of a cartoon. Let's go here to the bottom picture. This is the present time. We have some uncertainty about the true value, this relative frequency of event A of this radioactive release. We have uncertainty and we have a mean value, the current mean value.

Now, as we look forward in time, or if we could walk along the time axis, we would see this mean value change, because there are many things on which we do not know due to ignorance today, and as time evolves, we'll get more information and we'll see this mean value evolve, and there are two possible trajectories just to say that we don't know really how this mean will evolve.

Now, suppose that you had regulatory threshold that says this project is acceptable if the mean value of the risk, the relative frequency of the risk, is acceptable if you are below a certain threshold. Then if this mean risk at a certain point in time in the future will exceed that regulatory threshold, you'll have to take some corrective action, let's call it retrofit, possibly very costly.

Therefore, if you are in a situation like this when the mean can evolve and can exceed in fact a threshold that you don't want to exceed, you'll have to design conservatively today. You cannot go with the present mean. You have to go with something higher than the mean. And we can talk about how to formulate this problem correctly in a decision framework, but I'm not getting into the technical decisions here, but rather, I want to emphasize the concepts.

I don't think that this problem here of the future evolution has been adequately thought of in the context of the type of things that you are deliberating. But I have not been involved, frankly, in the Yucca Mountain project to say for sure. But what I'm saying is that this has to be addressed. It's a fundamental issue of how to deal with Type II uncertainties, which are the pervasive uncertainties in this type of project.

You have to say over the period of time of this project, which as I understand may be very long, 1,000, 10,000 years, how much will this risk, mean risk, evolve during this period of time. This is a very important point.

In fact, as time evolves, present uncertainty, which is presented here by this distribution, will be hard to decide, will be hard to explain, because certainly you'll learn more about the physical and chemical processes, and so on and so forth. So this fluctuation in the mean is accompanied by a reduction in Type II uncertainty as you move over time.

Let me now move to the third and final point of this presentation, which is going now to the first phase of decision, how do we quantify the mean value of F[A], which as I told you, is what we need, and it's possible future evolution, or in general, how do we quantify the uncertainty on F[A], of which this is the mean value.

Now, first of all, if I have convinced myself that all that matters is F[A] expected value and possibly its future temporary evolution, why should we look at the distribution of F[A], which is more information that we need. And here is at least a possible reason for actually assessing Type II uncertainty on F[A], and there are these reasons that would be used by people many times in many different ways.

In my own opinion, the main reason for assessing uncertainty on F[A] is to estimate the mean value. Because not until you have characterized that uncertainty, you can calculate the mean value. So I believe that this is the fundamental reason.

Then there are other reasons which I don't read to you here, but you can look at yourself, which are sort of similar reasons, but these objectives, like compare, communicate, document expert opinions, et cetera, could be done also in other ways other than really showing these distributions. It could be conveyed in other ways. But in order to estimate the mean value, we need that distribution.

How do we get the distribution which represents uncertainty of Type II on this relative frequency? There are many methods, some are formal, other methods are informal, and I would like to definitely mention some of then.

Of course if Type II uncertainty were of that objective type like we are uncertain on the mean of the distribution, or on the variance of a distribution, then we could use standard statistical methods. Perhaps we have a statistical sample we use, for example, like you might say the earthquake risk K, is the earthquake recurrence, you have a historical sample, you could use standard statistical techniques to assess Type II uncertainty on the recurrence rate.

However, this is by far a case that is sort of more the exception than the rule. The rule is that you don't have uncertainty in this nice form. You don't have a population, you don't have a statistical sample. And then you have to resort to methods that are based on expert opinion and mathematical model runs.

So let me focus on these matters. Now, the method that you use to, for example, combine expert opinion, combine the results of different models that use this input from experts, use the results of models to assess uncertainty about F[A] depends on how you view models, how you view the information you get from experts, how you view the information you get from running different models. So unfortunately here, we have a little bit.

And I want to talk about two different interpretations of model and expert input, which result in different ways of estimating uncertainties, and is pretty important. Now, let me actually start with B, instead of with A, and I'll come back to A here for the interchange. So let me start with B, because B essentially represents the sort of classical way of dealing with this problem.

That classical way is to view models as hypotheses about nature. So we have Model A, which corresponds to a certain hypothesis that nature conforms to that model. Nature maybe corresponds to Model B or Model C. So different models represent different hypotheses about how nature behaves.

In that case, and this has been done over and over again, you assign probabilities to different models, and then you combine the model estimates, if I had the estimate from Model I of F[A]. So you run this model and say, okay, in the model, there is this, this relative frequency is this value, which I'm denoting here. Okay? That's the estimate of F[A] from Model I.

So we then assign that same probability, and you calculate the mean value, say, current mean value, as this weighted average. Essentially, this is the mean rule, and this has been used over and over again. So you see many occurrences, for example, with probabilities attached, and then you take the average and you get these. That's what this is, the mean rule.

Now, this is only one way to view, however, models, and it's not necessarily correct. In fact, in many cases, it is incorrect. It's a rather narrow view of what models give you. Or you can apply this to experts as well, experts' models. I don't make actually a distinction here.

So let me go through the alternative way, which is in fact more general and more appropriate, to view models as a way to estimate a quantity. So if we use a certain model, mathematical model, to come up with an estimate of F[A], we do not say we don't trust you that nature behaves this way. No, this is what we can do currently with our numerical methods, because we haven't developed better models.

For example, we make an assumption that earthquakes occur in a poisson manner. It doesn't mean that we really believe that earthquakes in nature behave in a poisson way. No. So what models do is answer questions like what if. What if nature were to be poisson, then what would be F[A] in that case? What if nature were behaving different? What if? We're taking some snapshots of nature, given our current probabilities and conceptualization of possibilities, et cetera.

So if Fi had an estimate, are they answering questions what if? And we can make models that product best estimates. We can use models, bounding models, making conservative assumptions, and so on.

How do we then combine or use this information, these estimates, to obtain a probability distribution of F[A] and eventually mean values? Again, two ways. You can proceed formally through probabilistic analysis. The tools are there. It's called Bayesian theory essentially. There is a certain procedure which formally takes your estimates and produces uncertainty on F[A].

So you might say all right, then we can do it. In theory you can do it. In practice, you know this Bayesian approach, you need what is called, in jargon, the likelihood function. What is this likelihood function? I'm not going to explain it in detail. But basically, you have to be able to say how probable it is to obtain this result if the true value is a certain F[A]. And if you think about it, this is a very difficult thing to assess. Very difficult. Conceptually it's the right thing, and unless you have this likelihood function, you cannot use this approach. You cannot. And this is why this approach is not commonly used, although everybody I think agrees that this is the way one should go. This is the correct way of combining these results.

Before I go to A2, let me give you two examples, because these are rather revealing about this formal Bayesian approach. I say that it's very difficult to come up with the correct likelihood function for a given problem, very difficult. But let me make some make believe assumptions. So I say let me take some hypothetical and let me see what I get.

And I don't want to go into the details of these assumptions, but if I make a certain assumption about the likelihood, and I run through the Bayesian machinery, I get this result, that the mean hazard is just the average of these values model results. Again, a mean rule. Great. That sort of is the mean rule.

But if I take a different likelihood function, what I get is another combination rule. I should say first to get the probabilities, and then you calculate the mean value, and the final result is the expected value of this F[A] is the median value of the Fi, median, not the mean. You can find the mean and the median, may be off by big factors.

This is very important. When I say for a decision, you need the mean value of the relative frequency, mean value of the risk, that mean value I say can be obtained under different assumptions, either as the average of the estimates, or as the median of the estimates. So I need the mean hazard, but the mean hazard doesn't mean that I have to take the central average--I mean, the average, the average of the model results. The mean hazard may be the median of those numbers. And these are just two examples. If I change again the likelihood function, I can produce other means of compilation, that given these results for models, given these elicitations from experts, give the mean value of F[A].

So this combination rule need not be the average, a weighted average. It can be something else. It depends on the problem. And this I'm not sure is well sort of understood usually in the decision making arena.

Now, just to complete--actually, there is another way you might go. I told you that Bayesian theory is the way to go from a theoretical point of view. Here, we are not talking theory. We have to solve an actual problem. If we cannot use Bayesian theory because we don't have methods to assess the likelihood function that we need for doing that, then what can we do? The alternative is to use judgmental approaches, and here there are a lot of them, formal, informal, with expert--this and that and et cetera. But let's put them in a single box. They are judgmental approaches rather than mathematical approaches. All right?

Now, these methods in my opinion, can be actually pretty good, because through judgment we can usually account, although again in an objective way, that's a limitation on these approaches, we can account for a lot of things. For example, for the tendencies and biases of expert opinions, for the information that they may or may not have from the school they come from, et cetera, et cetera, all things that yes, in theory, you can deal with with this approach, but in practice, would be extremely difficult to do. So these methods are often the only way you can practically get to the answer.

So let me just summarize my main points here.

Sorry for the handwriting, which was a last minute addition. I thought that the summary might be useful on two issues. One, decision, and the other the assessment of uncertainty.

Regarding decision, the first point is what matters is the mean hazard and its future evolution. That's the only thing that matters for decision making. Usually, one stops at the mean hazard, and then people say I'm not comfortable just using the mean of the hazard, and they're right. They are right. For that particular problem, there is the possibility of future evolution of that mean, and that's the correct way of framing the problem. That's a correct way of changing the rule that states just the mean hazard and go with it. What is it we should do? We should account for possible future events.

In a case like the Yucca Mountain project where it seems to me there are many fundamental laws, sort of physical laws, states of nature, et cetera, et cetera, and the time span of the project is so long, it seems to me that neglecting this component is really not right. It's very important that one explicitly considers this feature.

The other thing that I hope I have sort of elaborated on is that the aleatory and epistemic distinction or Type I and Type II distinction is not important. It is not important because the Type II here is responsible for at least future evolution. So to that extent, it is important to recognize that there is this exception.

Also, let me add my own philosophical point of view. In a problem like the one we're dealing with, 99 per cent, if my philosophical point is 100 per cent of the uncertainties of Type II, there is essentially no uncertainty of Type I, or very minimal uncertainty of Type I. Most of the uncertainty is because you don't know what is there. You don't know exactly certain physical laws. You don't know. Ignorance is the driving cause for uncertainty, mostly uncertainty of Type II. And, therefore, it is subjected to future evolution because you can improve a model, so on and so forth.

Moving onto the assessment of uncertainty. One is this is usually the interpretation of models. Models as an alternative hypotheses is often incorrect. They are just practical views. They aren't the views that we have now. Nobody would swear on any of them as being the correct one. We know they are all limited in their capabilities. They're all approximate, and so on. So that hypothesis is incorrect and may produce erroneous estimates of the mean value of the distribution of uncertainty. But that result may be what is more important for a decision maker.

In fact, I talked about the median rule, for example, and so on. Those may be more appropriate rules than the mean that is produced by this interpretation of models.

Bayesian methods are to assess Type II uncertainties are theoretically exact, theoretically correct, but they are often impractical. In most cases they are impractical. And judgmental methods are less objective. That's the problem in the context, and one has to deal with it. But I think there is no way to get out of it. You have to deal with it. They are less objective, but they are simpler and often, in my opinion, they're more accurate.

Thank you very much.

COHON: Thank you, Dr. Veneziano. That was very stimulating, outstanding.

I had made a promise to my colleagues, but I'm going to suggest that since we do have a panel discussion, you'll have another chance that we can address questions, and move right on to Dr. North. Thank you.

We had to do a little exchange here, since there was one microphone.

NORTH: Let me start out by saying I'm really pleased to be here, very grateful for this invitation. It's really a pleasure for me to see a lot of old friends, not just on the Board, but in this audience, and reflect that I really enjoyed a great deal my five years on the Board, my involvement in the problems of high-level nuclear waste in general, and Yucca Mountain in particular.

During my five years on the Board, I was never asked to give a half hour lecture on decision-making under uncertainty, and I'm delighted to have the privilege today.

I'm really not going to say much about Yucca Mountain or high-level waste. I am going to quote one individual from the Irvine conference later in my remarks, and I want to say as the Chair of the Steering Committee that organized the workshop in Irvine and is responsible for writing the report, that we who were there can all draw our own conclusions on the consensus, or lack thereof. There are no recommendation or conclusions or findings that have been endorsed by the National Academies at this point.

It was a public meeting. I expressed myself at the end as to what I thought went on, but there is no formal set of recommendations or conclusions. Stay tuned, read our report, which we expect will be out this fall.

While I go to my remarks about decision-making under uncertainty, what I want to do is provide a quick tour on concepts of what I will call decision analysis, a formalism for decision making under uncertainty, and do so from the practitioner's point of view, this has been my day job for about three decades now, and in particular, I'm going to talk a little bit about approximations, following on Dr. Veneziano's talk, you know, how do wee do this. How much detail is enough? I'm not really going to get into the fine points, but I have some general principles I want to leave with you.

And following my colleague, I'm going to start with a very simple example involving the tossing of a coin. We're going to take a coin picked at random, which we might expect to be a fair coin, probability of heads one-half, and flip it three times. What is the probability of getting three heads?

Everybody understands that problem, and most of you think you can calculate the answer. I suspect the answer for most of you is yes, that's relatively straightforward, I know how to do it.

Well, let me make it more complicated. We'll give you some new information. We'll tell you there is at least one head among the outcomes of these three flips. Now what's the probability of three heads?

I first encountered this problem as I was nearing the stage of taking my PhD examinations at Stanford. It was in the PhD qualifying exam for the year before mine. And out of 20 or so people taking this exam, I think only one person got this simple problem right.

So I'd suspect for most of you, unless you've had an unusual course in probability and statistics, you might have a little difficulty with it. You might find this is something I'm not sure I can do. How do I take into account this new information in answering this very simple assessment of probability in a, I will call, almost simple as possible situation?

What's in this case, and I will submit it's a very good way of attacking complicated probability problems in general, is let us lay out the set of events determining what happens, call it the outcome space. And in this case, we can diagram it in the form of a simple tree, first flip, second flip, third flip, heads versus tails, and we get a sequence. And if we know that the probability of the head is 50 per cent, wee can just go through this and figure out what's the probability for each of these end point. It turns out to be 1/8th or 1/2 cubed.

Okay, now we are in a position to ask what happens when we bring in the new information. Wee have eight sequences here. What does the information tell us about those sequences. It's actually very simple. We've been told there was at least one head. That means that these seven are still in, and this one down here, all tails, is out.

So let us simply cross that one off. Have we determined anything about the change in the likelihoods of the seven that are left? No, we haven't. Are they all equal and likely? Yes. Now, what's the answer? Well, here's the case we're interest in, three heads, we have seven equally likely cases, probability is now 1/7th.

Okay, folks, you've just learned Bayes' rule. That's what it is. You get information that changes your description of the probabilities on all the possible outcomes. Typically, that's in the form of some sequence of final results, and data has been ruled out. Now we renormalize, because the probabilities have to sum to one, and we find our probabilities are different. We've gone from one chance in eight to one chance in seven.

So I would submit that for practical as opposed to theoretical purposes, we ought to view probabilities as being conditional on what it is we know. Probabilities reflect a state of information. They are not a characterization of nature, but rather, what we know about nature.

So if you flip a coin, for the person sitting in the audience who hasn't seen it, the probability of a head may be 50 per cent, but I'm sitting up here, and I can look at it, and for me, the probability of a head is one or zero. Same coin, but different information.

So probabilities reflect information, and as information changes, we need to be able to reflect that in changes in the probabilities.

So here's a very simple example with a coin. The real world is much more complicated, and we have all this literature with respect to how do we use probability. I'm not going to try to summarize that. There's a tutorial introduction to decision theory that I wrote more than 30 years ago, which has a good list of references on the philosophical foundations of these various approaches to probability.

What I'd like you to take away from it is there are basically three ways you can do it. You can develop probabilities from data, statistics. You can use probability as a way of summarizing subjective judgment, such as at what odds are you willing to bet, and measure probabilities that way, or you can view probability as an inductive logic where you can build up from a series of assumptions how you ought to calculate a probability. And there's a large literature, over 200 years, on how people have done that.

I'd like to note that any use of probability involves a certain set of axioms, which may or may not describe very well how people might choose to place bets. In my tutorial, there are some references on this, and there's a lot more literature subsequently. I don't want to get into it, other than to say that human judgment about uncertainty is quite fallible. So if it's important, you might want to do that logic explicitly as opposed to guessing. The issue is how much detail do we want to get into.

Now, I'm going to say briefly that decision analysis, a formal theory for a decision under uncertainty is putting together decision theory, how do wee deal with uncertainty in simple situations, with a whole set of technology that has been evolved in most fields of science and engineering that are quantitative of how do we deal with complex systems. And it's really putting these things together that gives you the ability to deal with complex decisions under uncertainty of the kind we're dealing with with high-level nuclear waste.

And there are two outputs we might want to look at. There is the local and quantitative procedure for making the calculations. But perhaps much more important is a language and philosophy for dealing with uncertainty and complexity. And in my judgment, we really ought to see all this technology as leading to enhanced communication. Just as science for years has used mathematics of which probability theory is a subset to communicate among the members of the scientific community, we can use quantitative methods as a means of summarizing what we know about complicated decision situations, and sharing that with the interested public.

Now let me go on to some examples, and what I want to do is give you whirlwind tours of several from my consulting experience, and then talk about one that we all have more or less in common, and then go to some overall conclusions to leave with you.

In each of these cases, I am going to make available to you a technical paper. In each case, these papers were written for generalists in the scientific community, not specialists in this particular area.

The first of these applications was done about 1970 and is available in a Science Magazine article from 1972. The issue involved whether the U. S. Government should do something that it had never done before, and that is to seed a hurricane that's off shore that might impact a coastal area of the United States, Miami for example.

There was a very interesting new theory with a simulation in a computer of how a hurricane worked, and an experimental seeding that had been carried out in 1969 on Hurricane Debbie that tended to indicate that the theory looked good. The theory predicted that if you put silver iodide in the eye wall clouds around the hurricane, that you would make the eye larger, and this would slow down the Hurricane, reduce the maximum wind, and that would reduce the property damage from the wind and the storm surge.

We had an Assistant Secretary of Commerce that came in that was very much interested in decision analysis, and looked at this problem and said if hurricane seeding is very likely to reduce the damage and won't change the hurricane if this theory turns out not to be true, let's go ahead and seed hurricanes.

The National Weather Service scientists said no, no, no, you don't understand. Hurricanes are highly variable. The U. S. Government goes out and seeds one and it gets worse, we're all going to lose our jobs.

So there are two parts of this, and I'm only going to talk about the first one, which is characterizing the uncertainty. And, really, it is the second part of the problem, the political context or the value judgments, that was the main focus of our analysis in convincing the Assistant Secretary that there really was a legal and institutional problem, not just getting the probabilities right. But I haven't got time to tell you the story there. I'm just going to show you how we did the probabilities.

The issue was you have a hurricane that's twelve hours away from projected landfall, and what is going to happen with and without seeding. We were able to get data on what happens to hurricanes over a twelve hour period, and then ask what can we now say about the knowledge on this hypothesis that seeding will change the hurricane and make it less damaging, so that we can make that quantitative.

So here's a picture of what the frequency data looked like with regard to changes in hurricane intensity measured off barometric pressure and projected essentially through regression analysis on the changes in the wind. And that is reasonably well established in the community.

What we did in doing the analysis was combine that source of uncertainty with others in terms of how much additional change would the seeding occur--or would that introduce. And, yes, it introduced some additional uncertainty. But the main uncertainty was how about this hypothesis, was it right or not? I've shown that with the green arrow. That was on the average seeding a hurricane makes it less intense. And that hypothesis was there's no change. Seeding doesn't really do anything. And at our request, a third one was added, namely seeding could make it worse. It could change the hurricane to make it higher winds, more property damage.

We were able to get a consensus within the community of scientists working on the problem on how they saw the probabilities before and after the Debbie seeding experiment with some very simple statements at the level of before we had the experimental seeding, we believed fairly strongly that seeding the hurricane was much more likely to reduce the winds than to make the hurricane more intense.

After the Debbie, we think it's about equal probability for seeding makes it better, or seeding has no effect.

Well, in a little more complicated version of Bayes' rule than what I showed you with coins, you could work that into a set of probabilities for these three cases, three equations and three unknowns. And with that, and the frequency data, we were able to develop a probability distribution on wind speed with and without the seeding.

Now, is this exact in any form? No. Basically, what it is giving you is a crude sketch of what some combination of data and judgment might give you as an attempt to characterize quantitatively what will happen with and without seeding a hurricane. Much of the focus of this was the value of additional experiments. Again, I won't go into that. Read the paper. And by making discrete outcomes here, we could explain to decision makers who couldn't read the graph that if we were worried about, for example, sample an upper 5 per cent event that the hurricane got much worse, the chances changed from about 5 1/2 per cent to a little less than 4 per cent with the seeding as opposed to no seeding. In other words, a favorable change, but not a big one.

So what this allowed us to do was to point out what the scientists were telling us, that a seeded hurricane might get a good deal worse after it was seeded. And we could highlight that issue for the decision makers, and point out the political context, and change a problem which initially had a big debate about the probabilities and how much data did we need, into an issue for lawyers to describe the legal basis for the U. S. Government taking action. So in other words, we're able to change the debate onto another set of issues in terms of what was important.

Let me go to whirlwind number two. This is the issue of contaminating Mars with the Viking landing that occurred in 1976. I was brought in as a consultant to review what NASA was doing on estimating the probability that we would introduce microbes from the earth onto Mars, and they would be able to replicate up there. That's called contamination.

The United States and Russia made an agreement that had the force of a treaty that both nations, in conducting their space programs, would not violate a constraint that the probability of this kind of contamination would be below one chance in a thousand.

The Russians showed they didn't have microbes on their spacecraft. They actually ground one up and cultured it, got a negative. We knew we had about 20,000 on ours, mostly spores encased in the plastic that we used to protect our transistorized electronics. The Russians were using vacuum tubes. That's why they were able to sterilize theirs.

So we had 20,000 microbes, and we had some very concerned scientists led by a man who became famous from his television programs, Carl Sagan, and the issue before NASA was to convince this community that it was really safe to fly the mission, that it was below the probability constraint.

So I was asked to do that, and I want to give you a quick tour of how those probabilities were calculated. There's a diagram that shows the load of microbes on the spacecraft, the way they might be released, transport into a favorable micro-environment where they could get nutrients and something like water, and then finally the probability that in that environment, they might be able to grow.

So you've got the hazardous material, containment, release mechanisms, all the way to the probability that something bad happens.

We were able to work with a series of scientists who were expert in various pieces of this problem in order to go step by step through this process, and develop a numerical description of what happened in that box, none of this very precise, all of it essentially making a quantitative sketch of judgment. Again, I won't go through the numbers. I can show you a page full of sensitivity analysis. The answer turned out to be six changes in a million, and varying of the assumptions didn't violate one in a thousand. You had a factor of 16. And as uncertain as these judgments were, it would take two or three big changes before you'd go over that line.

Now, is the number the answer? Hell, no. What we learned in this analysis was there was some physics that determined why it came out that way. That physics was the Martian atmosphere is thin. There's a lot of ultraviolet light coming in. And if you think about those microbes encased in plastic being released by wind driven sand on Mars, if the microbe is in a particle that is large enough to protect it from the ultraviolet light, it is too heavy to be suspended in the Martian atmosphere and it's going to drop right under the spacecraft, which is very unlikely to be a favorable micro-environment. That's why it comes out that way.

That insight sold to Professor Sagan and the others on the advisory committee, and we were able to get a consensus that flying this mission was safe.

So I don't think it's the number. I think it's the insight and the state of information that we have, and when we do this kind of analysis, often it helps you to focus on what's really important, and get the insight so you can make the case without having to use the numbers.

The third example on the importance of thinking about probabilities as being conditional, safety of flying on airplanes. Today, my son is supposed to fly from San Francisco to Baltimore, and there's a huge snow storm in the east. So should I be worried about that? You know, this is a flight in bad weather as opposed to a normal flight.

We can look at statistics, and we have very good statistics on airplane accidents, is the basis for this probability. But I would assert that for most of our decision making we want to think not just about that frequency data, but what do we know about the causes of airplane accidents. Bad weather, mechanical failure. Did they really stop the plan and not fly if there is any indication of a mechanical problem. I think most of us have concluded from our experience that they're very conservative about that.

Well, what about Tom's comment to me over the coffee break, human nature? Supposing you find out the pilot is suicidal and the co-pilot has to go to the lavatory. Wow, are we in unchartered territory there. We might like to know can you put this plane into a dive or a spin that's so bad that nobody can get the plane out of it. It would probably be a good idea not to have planes that have that characteristic. It would probably be a very good idea to do psychological testing to make sure that pilots with that kind of adverse human nature don't fly planes. And I would argue that the statistics of the past may be largely irrelevant in terms of dealing with specific situations of deranged pilots.

Well, my sense is that the airlines have compiled a very admirable safety record. In 1998, there were 600 million of us flying on commercial flights in the United States, and no fatalities. But I would argue that in dealing with airline safety, we don't want to rely just on the statistics. We want to be out there pushing the frontiers of our knowledge and understanding of weather, mechanical systems, human nature as far as we can push it to get as much safety as we can.

And I would argue in that framework that what we're really interested in are the unusual bad outcomes that might occur, and how can we eliminate those and how can we reflect on making air travel safety better by using that information.

Now let me go to my conclusions. First, quoting Bob Bernero, who many of us in the room know from his previous work with NRC at our Irvine conference, and I thought he put it very nicely and very succinctly, we want to make our judgments and our decisions based on the body of knowledge, not the equation. I think that's relatively consistent with what the speaker from NRC who follows me has said in the article that he handed out.

Here's one from me which I find being quoted by my friend from New Zealand who does analysis of problems involving diseases from imported or exported agricultural products, read New Zealand Land, and is one of the world experts in this community. I started quoting him. He's now quoting me back, and this is what he quotes.

We want to develop those insights, and we want to avoid too much reliance on high precision in the calculations. I've got three significant figures written on some of those slides I showed. That's so I can check it. I don't pretend that the results are accurate to that level. In fact, I describe it as a sketch. But what we get is an ability to sharpen our thinking about what's important in a complex problem, and we have an ability to explain our reasoning to other people. We want to watch out for numerical results being misinterpreted by decision makers and stakeholders.

So I want to come back and summarize with a couple of bits of advice in conclusion on what it is we do. In building large complex models in decision situations, we want to include the detail that's important for the decision, not everything we know how to model. We can use sensitivity analysis and value of information calculations--I'll refer to my paper for those of you who don't know what that is--to determine where is more detail useful.

If the detail is unimportant, we might use a fixed value. We don't care about temperature fluctuations in some contexts. If wee have something that is modestly important, we might get away with a simple probability distribution of the kind they teach in the first year class, Gaussian or normal or poisson or something like that, simple assumptions.

If, on the other hand, this uncertainty is crucial to the decision, maybe we want to invest a lot of time and build up a detailed model that incorporates the details of what we know, because that's an area where we need to concentrate. So I don't think there's any fixed rule, but I think we need to adopt the analysis to the problem.

So I'll conclude by making the point again that probabilities depend on information, that there is a formal way to revise probabilities as we get more information, and that we want to remember that probability represents what we know about something. It's a state of our mind. It's not a state of things. As we get more information, probabilities can change.

Now, for a lot of people who believe that a probability was a frequency based on statistics, this is something they're not used to. If probability represents judgment and probabilities change as we get more information, then it really is very important to think about what information do we have now, what information can we get later, and how does that allow us to make the decision in a better way.

Thank you very much.

COHON: Thank you, Warner. That was very good.

To show you what a generous chairman I can be, we're going to break now for lunch. We'll reconvene at 1 o'clock for other speakers. Thank you very much to all of our speakers.

(Whereupon, the lunch break was taken.)

AFTERNOON SESSION

COHON: We continue with our session on uncertainty with two presentations, followed by a panel meeting. We'll hear now from the Nuclear Regulatory Commission, starting with Joseph Holonich, Deputy Director of the Division of Waste Management, followed by Budhi Sagar. Mr. Holonich?

While he's getting together, I need to admonish all speakers please to speak directly into the microphones, especially those speaking from the audience and my colleagues on the Board, please speak direct into the mike so that our recorder can hear you and everybody else can as well.

Thanks.

HOLONICH: As Dr. Cohon said, my name is Joe Holonich. I'm the Deputy Director of NRC's Division of Waste Management. Budhi Sagar and I are doing a two-part presentation on uncertainties in the licensing process, and the way we've broken it up is that I will start off giving some general discussion of how the NRC treats uncertainties, focusing in on some particulars, transitioning into the high-level waste, and then Budhi is going to get into the technical discussion, in particular, of how uncertainties are treated in the performance assessment process for high-level waste. He's also prepared to answer all the hard questions. He and I had this discussion at lunch.

Starting off with the general overview, basically NRC has a goal, and the goal is to set regulatory requirements that are protective of public health and safety, the environment and the common defense. When you do this and you implement a regulatory program, you of course have uncertainties. There's uncertainties in everything, and NRC essentially has come to two means of addressing uncertainties as it does its regulatory responsibilities, as it implements its regulatory responsibilities.

Number one is to compensate for the uncertainties through conservatism. The less you know about the uncertainty, the more conservatism NRC looks for in the design. The more hazard there is, the more uncertainty, the more conservatism. The less hazard, the less uncertainty, the less conservatism you need. And also to work in defense in depth, have multiple systems there that offer protection so that you can make sure that if you don't understand the system and you've got uncertainties in it, you're able to compensate for it by additional barriers, additional means of protection.

As I put the presentation together, I tried to think of something that was a good tangible example of how NRC has handled uncertainties in the past. And given my roots are in the reactor side of the house, I tried to come up with an example from the reactor side of the house that I had lived through, and how NRC had changed in terms of addressing uncertainties, moving from a conservative approach to a more statistical approach. And essentially what I picked was what's called the departure from nuclear building ratio. And what that ratio is is it's an indication of how effective your heat transfer is in the reactor core, and theoretically, a value of one is where you don't want to be, and when you get below one, you start to get heat transfer problems.

And in the past, as people designed reactor cores, especially the earlier generations, they established a limit for the CNBR, and that limit was pretty much based on the correlation that was there.

As they did the analysis, they set the parameters at their most extreme values, whatever they were, lowest pressure, highest heat, lowest flow. They then ran calculations and designs to see how the reactor behaved, both in steady state and in transients, and what they ultimately showed was that the reactor met its limit during steady state operations and transient. And pretty much the uncertainties that were in these parameters, the uncertainties that were in codes, in measurement techniques, were all handled deterministically, and the Agency's view was everything was in such a conservative limit, that the uncertainties were compensated for by being at the extreme conservative limit.

Subsequently, Westinghouse, who was the designer of the reactors, came in with an improved methodology, what's called the improved formal design procedure. They came in to show that basically, they could change the design parameters, that the design parameters were somewhat mutually exclusive.

They also established a new correlation to set a different limit, and then they ran the reactor design and the reactor core analysis to show what the difference was in the design. And pretty much what they did was used statistical analysis, use uncertainties and account for those uncertainties as they did it.

And what I've got on the next slide is a little bit of a table comparing two reactor design. The first one, Watts Bar, was done with the conservative design approach, and the second one, D.C. Cook, was done with the statistical approach, accounting for the uncertainties, and a couple of things happened. Number one, you can see the power level for D.C. Cook, the nominal power level drops, which means you don't have the heat at its highest level.

The flow rates are a little different. D.C. Cook actually is moving in a conservative direction. The pressure is higher in D.C. Cook. The higher pressure, the less likely you'll get transfer problems in the heat. So that's in a non-conservative direction.

The limits that were used by Westinghouse, one was called the W-3 correlation. If one was theoretically at a value where you wanted to be, Westinghouse accounted for the uncertainty by picking the highest value that they ever got on that correlation, which was 1.3, using that as the design limit.

They then developed the second correlation called the WRB-1, where they statistically analyzed the data, and came up with a number that's closer to one. So instead of having 30 per cent margin in their limit, they had 17 per cent margin in their limit.

They ran their calculations, and pretty much what you see here are the ratios that start at the reactor at normal operation. And what happens is you have a transient, and that number goes down, and then the system recovers and comes back up. They start at about 2 for the normal design, and then their transient condition, they get down pretty close to their limit, 1.39 and 1.38, depending on the type of cell they analyzed in the reactor core.

What happens when you do the statistical work, you account for the uncertainties, they were able to go to lower power in their analysis, starting at higher conditions for their operating parameters. When they went through their transient and hit the bottom, they were at 1.77 versus a limit of 1.17.

So what this shows is, you know, early in the design process, you don't have a lot of knowledge and people are setting things at their most conservative. That's the way the Agency looks to account for uncertainties. As you're getting operating data, you're getting able to show how these numbers behave, how these reactors behave. You can start to account for the uncertainties, change your design methods and back off from the conservatism to come to more nominal.

I thought that was a good example to show kind of how the Agency has flexed, and really it's not the Agency that flexes, it's the applicants and the reactor owners who have come up with different methods to gain more margin in their design.

The second thing was defense-in-depth. I talked about that. And if you look at this graph, what this graph shows is essentially whether you need defense-in-depth, depending on where you are. You've got a lot of smoke detectors out there, fairly low hazard, you've got a lot of data. You don't need a lot of defense-in-depth for smoke detectors.

You've got reactor systems over here. You've got a lot of data, but a fairly high hazard, so the Agency is looking to compensate for that hazard by having defense-in-depth systems there that help you respond to transients, help you respond to accidents. And then in between here you've got different kind of things, like independent spent fuel storage facilities. You don't have as much data on those as you do the reactors, but the hazard is not as great at the reactors.

And so the different kinds and different levels of defense-in-depth the Agency looks for to address uncertainties depends on the hazard you've got, and the amount of data you've got.

Now, moving into how we look at things in a repository, essentially we're looking for DOE, the applicant, to treat uncertainties for a couple of things. Number one, the parameters that they use, the scenarios, and I'm not going to go through a lot of depth in these slides. Budhi really has a lot of the technical meat on these, and so I'm going to walk through them rather quickly.

You've got to look at the uncertainties through a number of means, doing sensitivity studies, doing uncertainty studies, an importance analysis where you can take away modeling-wise a barrier and see what the results are, and that tells you how important that barrier is to meeting the final performance standard. If that barrier is not that important, you can say maybe I don't need that much more data in terms of characterizing this barrier.

You know, you don't remove the barrier from the mountain. We have a struggle sometimes with the technical staff. They keep saying, well, it's going to be there. Yeah, it's going to be there just for modeling purposes, though. You remove it, see how well the system performs without that barrier, and if it performs just as well, that says to you this barrier is not that important, or this piece of the barrier is not that important.

Pretty much what the Commission is looking for is for DOE to give us the technical rationale for the models that it's put together. The Commission has said that it's looking for a credible representation of Yucca Mountain, no more than that, and no more than that is needed in terms of the Commission making a decision.

So it's up to DOE to be able to put together the rationale for its choices and the technical basis for those choices. And also for any models, alternative scenarios, alternative models that weren't considered, because you can get data and you can come up with multiple models using the same data. It's hard to say which one is really correct, so it's up to DOE to say this is the one we've chosen, and these are the ones we've rejected and here's why we've rejected those.

And all of this needs to be based on data that DOE collects, field data, laboratory data, analogs where appropriate, and detailed process models, which give you an idea of how well you've abstracted things into your overall performance assessment.

This data collection, the first three bullets really start with what DOE is doing today, site characterization, and the way the Commission set up the rule, it recognizes that it's going to have to make a decision with some degree of reasonable assurance, not absolute assurance, but it's also set up a system where you make the decision, you construct the repository, if the application is found acceptable, and you begin to collect performance confirmation data. Actually, performance confirmation starts with site characterization, and then as you operate the repository, you place the waste in the repository, we're looking for DOE to continue to collect data to confirm the analysis that it had on which we've made a determination of acceptability of that license.

So through the operating life of that repository, the Commission has a system of checks and balances where DOE is to continue to collect data to confirm its analysis, and the check in that is the confirmatory data. The balance is you've got to be able to take the waste out of that repository if you're finding the actual data as you operate the repository is not confirming your model.

So there's a check and balance system built in, and we're not going to know everything at the time of licensing. We're not going to have as much data in the repository as we do on reactors, we've got a hundred of them operating out there, who've got years of data. So we're going to have to make a decision on reasonable assurance and continue to collect data with this check and balance of checking what we're doing, and as a balance, being able to get rid of the fuel, pull it out of the repository if we're finding that it's not performing the way we expected as we did the analysis on licensing.

This slide just talks a little bit about we're looking for both quantified and unquantified uncertainties to be addressed. The overall standard that NRC currently has in its draft rule is 25 millirem. That may or may not be the ultimate standard, depending on what the Environmental Protection Agency does. We will have to conform our regulations to whatever standard EPA puts together. And we will look to use the mean, and we think the mean is the appropriate value to use, in that we've looked at uncertainties throughout the process and the models and the data and the codes, and we've incorporated those into the final number. So we think as you use the determination of dose, that the mean of the dose is the appropriate value to use.

And then, you know, when you look at unquantified uncertainties, you've got to look at how they're affected by models, parameters, scenarios, and the choices among the alternative conceptual models. If you pick one model versus the other, what does that do in terms of the uncertainty space?

So to kind of summarize it and pull it all together, I guess two things, number one, NRC deals with uncertainties in two ways. First off, we look for people to, in conservatism, will be able to quantify and address uncertainties. And I probably should have said performance analysis. I was using the reactor example and I left design analysis up there. We really ought to say performance assessment, performance analysis. And, number two, through defense-in-depth, which is currently in the rule, in our proposed rule, and which is a concept that the Commission I'm sure will keep in the final rule. And then we look for people to make sure, licensees to make sure they've quantified the data, quantified the uncertainties, and if they haven't, that they're able to compensate for that.

So that's kind of a general overview. What I'd like to do now is let Budhi come up and talk about some of the technical detail, and then we'll both be prepared to answer any questions.

Is that all right?

SAGAR: Thank you, Joe, and thank you, Mr. Chairman.

I'll add a little bit to what Joe just said, and as a matter of fact, I don't have any equations, so I don't know how much technical content you were looking for. By the way, I do love equations, and I could have made it pretty mathematical, but--and I think since we will have the panel after this, so questions could be probably deferred to that at that point.

I'll talk about three things. I will talk about the treatment of uncertainties as they appear in the proposed regulation, proposed NRC regulation applicable to the high-level waste repository, which is 10 CFR Part 63. I'll talk about the other major important documents that NRC will produce, and that's known as the Yucca Mountain Review Plan, which is guidance to the NRC staff how to review DOE's license application, what to look for, what would be the acceptance criteria, what method the NRC staff would follow to review and, therefore, the uncertainties, how to review the work on uncertainties that DOE would have put into its license application. And then I'll talk a bit about technical issues related to the various kinds of uncertainties.

As was said this morning, the public comment period for Part 63 is now over, and the NRC staff is busy responding to the public comments. There would be some changes as a response to the public comments that would be incorporated in the rule. But one thing that you might, those of you who have read the statement of considerations, will notice that the discussion of uncertainties, various types and at various points and various times as the licensing process would play is central to the statement of considerations.

So it's pretty well recognized that the uncertainties will persist throughout the process, starting from the construction authorization to the repository closure, and thereafter. So that's pretty well accepted.

The post-closure performance criteria will be stated in terms of the statistical average in the sense of probability weighted average, the maximum within 10,000 years, mean or expected value dose not to exceed 25 millirem, and as Joe said, that may change, depending on what the EPA's final standard would turn out to be.

The primary focus, however, as several speakers said this morning, is not the numbers per se, not one single equation per se, but all the evidence that goes into getting this estimate of 25 millirem. If it is 26, or if it is--26 doesn't necessarily mean the license application is rejected. So the multiple line of evidence that would be brought forth would all have to be considered.

And I think there is a part which NRC has used since its beginning, reasonable assurance, which is subjective. In the end, it's recognized that after looking at all the data, all the calculations, all the numerics models, the ultimate judgment has to be made, and there would be quite a bit of subjectivity in that judgment. And, therefore, essentially reasonable assurance means, because it will not be defined numerically. So that would remain a subjective judgment in that sense.

And the draft Part 63 also has two or three clauses in Section 114, which indicates what NRC expect with respect to the uncertainties in the license application. For example, it requires that the license application, or the safety case in the license application account for uncertainties and variabilities in parameter values, and provide the technical basis whether you are assuming deterministic values, probability distributions, bounds, and we know that there will be a mixture of all these in the eventual calculation. But so long as there is a technical basis that we could see, I think we could review, that's what we expect.

Secondly, Part 63 requires a consideration of alternative models. Now, whether you consider alternative models as alternative hypothesis, as one speaker said this morning, or you consider this as estimators, we believe that you have alternative models because the data that you have in your hand cannot rule out all but one. That's why you carry alternative models in your analysis. And so long as that is true, the NRC staff would expect to see the discussion of those alternative models, and calculations using those alternative models.

Whether you assign probability distributions to them to combine into a single probability curve in the end, the application should contain a separate discussion of each alternative model. So that's what we expect.

And, again, the disruptive scenarios or the event classes that you have to consider during the 10,000 year compliance period of the repository are defined in terms of the probability of those event classes. So the probability factors into, or the uncertainty factors into almost all steps of the building of the safety case.

Even in the preclosure safety considerations, the design basis events, Class 1 and Class 2, are defined in terms of the probability with which, or the frequency with which they might occur during the preclosure period.

In the Yucca Mountain review plan, which I said earlier was another major document in addition to the regulation itself, we talk about--you know, it's a complex project. There are all kinds of disciplines involved, hydrology, geology, geochemistry, and so on and so forth. But what we did was we decided we can write some generic, what we call generic acceptance criteria, and then as we go from one discipline to another, one part of the repository to another part, we can customize them to that particular part.

The two generic criteria that I have indicated on this viewgraph are related to the data uncertainty and verification, which is the generic criteria Number 2. The "T" here stands for technical, technical criteria Number 2. There are a couple of criteria that would be pragmatic criteria, quality assurance, expert elicitation, et cetera, et cetera, but these are technical.

And the technical criteria Number 3, relates to model uncertainty. And, again, the language here is very flexible, very general. It simply says that the parameter values assumed, the ranges of those parameters, the probability distribution, bounding assumptions, et cetera, are technical defensible. The reviewer is supposed to check that these assumptions are technically defensible, which means the DOE would provide the technical basis indicating why certain assumptions or certain probability distributions are okay, based on data, based on theory, based on whatever.

And it's recognized that, of course, the data would be used both for model development, and also for the parameter estimation of the same model. Therefore, the model uncertainty is again talking about the alternative conceptual models that may fit in the existing data that you have in hand, and that you can't rule out.

The alternative hypothesis of a model should not be rejected out of hand because there is one preferred model, unless there is a technical basis to say that's the only model that really honors all the data. If there are other models that honor other data, well then you have to carry through the analysis.

Going into a little bit of the technical content of the review process regarding the uncertainties, the sources of uncertainties, whether you call them Type 1 or Type 2, I think most of the uncertainties in this process would be a mixture of those two. There would be some data, and there would be lack of knowledge, and so on and so forth.

And I agree with the first speaker here that the distinction between those two doesn't necessarily add to the decision making in the end; that the uncertainties should be identified as such to make sure people understand, that it should not be a black box is important to understand, but to necessarily treat them in a different manner may or may not help.

And wee feel strongly that many times, the spatial variations, the heterogeneities and the temporal variations are lumped, and they are treated as uncertainties. It's okay to do so, so long as it's clearly explained how that is done. But as far as possible, if you can keep them separate, if you can propagate the spatial variability and temporal variability through a model, it's better. But if you can't, if you have to lump them as an uncertainty, so be it. But it ought to be explained.

And Joe made this point, there are uncertainties you can quantify, and there are uncertainties you can't. It's sort of an unknown/unknown kind of thing, and we all know that science has developed over the past hundred years, it will develop in the next hundred years. There's always lack of knowledge, and the idea is that if this is a hazardous possibility, then you should consider the possible effect of the unquantified uncertainties. This could be done qualitatively through defense-in-depth, through other evidence that one might bring forth, natural analogs, and so on and so forth, but that evidence would be required to assure that unquantifiable or unquantified uncertainties have been considered.

And then, of course, you have to propagate all these uncertainties through the analysis properly, correctly. That's probably the least of the problems, because the matter of propagation is pretty well known. Monte Carlo is one simple one through sampling processes, and so on, and there's not a whole lot of uncertainty about using those methods. So that is probably the least critical to this discussion.

The appropriateness of probability distributions, again, you know, it's easy to say, well, we know nothing about it, let's assume it to be uniformly distributed from zero to infinity, or whatever. That will not work. But you do need some justification of why a particular probability distribution is assumed in the safety case.

And rather than calling them Type 1 and Type 2, I think we look at sources in the sense is the uncertainty in the conceptual model itself, how much simplification have you introduced, that is, in the sense of the model detail, how many stratigraphic layers, how many have you lumped together. Are the faults represented discretely, or are they all bunched together as a continuum, et cetera, et cetera, those uncertainties, and what kind of constitutive equations. Is it, for example, a function of temperature, if not, why not, or what effect does it make if you omit that. Those are the conceptual model uncertainties.

The mathematical model, again, the translation of all these concepts into equations that you can solve on a computer, the numerical errors you might introduce, et cetera, et cetera. Again, in my mind, the second one is much less serious, not that you can neglect it, but it's much less serious than the first one where you first formulate what concepts should go into the safety analysis.

I won't go into alternative models again. This is re-emphasizing the same thing again. But the parameter identification of models, most of the models that we use in performance assessment have a set of parameters. For example, the one we developed has as many as 700 parameters. Now, you can call them 700--which means you can fit almost anything with that model.

Now, it gives you flexibility, but on the other hand, it puts on you the responsibility to show that with such a large number of parameters in a model, it still makes physical sense. So it has to be compared to some maybe module by module you have to compare this model to a more detailed model, or data or natural analogs, and so on and so forth.

And the disruptive scenarios, again, there's not a single way of defining them. We think that you have to define classes of events, volcanism, for example, all volcanic events as a class of events, assign a probability to that, and then define the uncertainty within that class, depending upon the particular event, and so on and so forth, in a probabilistic manner.

But there are other ways of doing it. I don't think NRC staff would say this is the only way you can do it. I mean, the DOE is free to do whatever method is acceptable to them, but it has to, again, be clear, the probability distribution ought to be clear, or it ought to be clear how the probability distribution was determined or assigned.

And completeness of scenario classes is the other issue. The laws of probability have to be followed in the sense in the end, they all should add up to one. So you can indicate that the entire universe, so to speak, of disruptive scenarios have been identified.

And distinction between variability and uncertainty, again, it's important, if for nothing else than to make or box the complicated PA model, not completely a black box, at least a gray box, if not entirely a white box is needed to be explained. It needs to be indicated. If DOE wants to use six or seven columns, for example, in the transport model, we want to know how did you come with six or seven columns, why not 15, for example, or why not two. So some sort of analysis indicating, look, if we did use more columns in our transport model, it doesn't make a lot of difference. I think that's what the staff would be looking for.

A clear characterization of the variability, and temporal variability, if that's applicable, needs to be documented. And the model uncertainty needs to be clearly described. And by that, again, the best we can say is that if you do have alternate models, you should present the results separately, not a single curve in the end. Even if you present a single curve in the end, as intermediate results, the alternate models should be treated one by one just to show what the effect of those models are on the outcome.

And, again, there are various ways that are used to assign probability distributions to parameters. Data-based empirical distributions of course is the statistical standard method. But then you can drive them theoretically in the sense based on some physical or chemical principles. Or you can do expert elicitation, that's of course a possibility. The NRC view is that if you can collect data, that data should not be replaced by expert elicitation, that you should have some reasoning indicating why certain data cannot be obtained, and then go to the expert elicitation.

Then correlations between data is of course important because as you propagate uncertainties, if you neglect correlations between data, the end result may be quite different from what it ought to be. And, therefore, if you ignore correlations, it needs to have a technical basis why those can be neglected.

And then unquantified uncertainties, again, there's a discussion that ought to be there indicating, you know, Darcies apply for a fracture, for example, or whatever other considerations you have built into the model that finally gets used in the safety analysis.

Again, a few things that staff would look at for the propagation of uncertainties through your analyses are that the entire range, the uncertainty range, gets propagated, that you don't ignore low probability values in the propagation, and that the model uncertainty again, along with the associated parameter uncertainty is propagated through the entire analysis.

The appropriateness of probability distributions, again, I think one of the speakers in the morning, I think it was Dr. North here who presented a sensitivity analysis with respect to one of his examples on using different probability assumptions, and so on. It's very helpful because most of the time, the probability distributions are not really objectively known, so you end up making assumptions. And whether one type of distribution produces end results which are more conservative than the other distribution, I think it's worthwhile exploring through sensitivity analyses what kind of distributions are most appropriate for the safety case.

And, again, I think the curve point is important because many times in the absence of knowledge, it's generally assumed that if we assume the range to be wider than it actually is, or if wee assume that the uncertainty is larger than it actually is, that it's a conservative assumption. That's not true at all times. It may be true for some cases; it may not be true for some other cases. It's very easy to show an example indicating that greater uncertainty, for example, can lead to a smaller mean dose, which means it's not a conservative assumption. And, therefore, the staff would certainly look at those kind of scenarios if they are built in.

And by the way, that the mean is enough, I think Joe suggested that, and the mean is specified as the criteria for post-closure performance in Part 63, but the time dependence of that mean, just like I think it was said this morning that the likelihood function is very difficult to define in a practical sense in the application of Bayes' statistics, I think the idea that I can show how the mean would change with time is very difficult really, because then you need to again foresee the future, which is something that is not easy to do.

But I think in Part 63, the various stages of the process would require that the data be updated, or the analyses be updated, that the mean be calculated at different times of the repository development process, and that would tell us whether new knowledge changes the mean that we calculate.

In the end, to close my presentation here, the staff at NRC recognizes the importance of the uncertainties throughout the licensing process, and it is included in Part 63, and it will be discussed in quite a bit of detail in the Yucca Mountain Review Plan.

We understand, I think all of the audience here understands that the incorporation of uncertainties into an already complex modeling exercise makes it even more complex. And for one, I'm not quite sure how this would be explained to the public, but I think an effort needs to be made to present as many of the intermediate steps as you possibly can to make clear how you went from Point A to Point Z in the end. I think a simple black box is just not--should not just be done.

And, again, I think the model uncertainty and parameter uncertainties should be clearly identified, indicate which one is which, and the effect of each individually shown.

I thank you for your time.

COHON: Thank you. Our thanks to both of the speakers from NRC. We'll defer questions again, and move right to Abe van Luik from the Yucca Mountain Project. And if Abe sticks to time, we might be able to sneak in some questions before we break before the panel discussion.

VAN LUIK: Well, after the first two speakers in this session, I was quite elated. After the last two speakers, I'm somewhat burdened, and I think I've discovered a new disease. It's like manic depressiveness, you know, it's pre-lunch euphoria and post-lunch depression.

I'm Abe van Luik. I'm with the U.S. Department of

Energy, and what I want to do today is talk about decision-making in the face of uncertainty, and I want to make a couple of things clear right at the outset. What I have here in these illustrations is cartoons, or sketches, of a process. I'm not outlining a structure that we man with people. I'm telling you of how we go about making decisions in the face of uncertainty.

And an illustrated point here is this circle that says technical analyses, analyze quantified uncertainties. Of course, there are 15 or 16 other bullets here of all the other things that we analyze. But one of the things that's always part of a technical analysis is to look at the uncertainties. But then we do a larger technical assessment after the calculation is done and say does this make sense, what does it mean, and then we have to look at all the other uncertainties that could not be quantified into the technical analysis itself, and come out with the numbers.

And then on top of that, when we go to making a decision, there are policy and other technical considerations, and we have to manage uncertainties. We have to live with uncertainties, and so we do some of the things that some of the speakers referred to. We say how important is this issue? Is it important enough to go more conservative? Is it important enough to change the design, et cetera. And then the final thing is we have to communicate the uncertainties to an audience such as this, and the more difficult part is to communicate uncertainties to the public.

I think the other speakers have covered this admirably. Uncertainties arise from complexity, variability, unanticipated failure mechanisms, unknown unknowns, and the potential system evolution. As Budhi said, it's difficult to predict the future.

DOE must identify sources of uncertainty, I mean, you can almost just say this is a summary of your talk, reduce or mitigate critical uncertainties, and assess the effects of residual uncertainties. We understand that.

The purpose here is to describe our approach to uncertainty, and show how it involves not only evaluating expected performance, but also explaining the uncertainties and their meaning.

Again, these boxes here, we could have drawn a circle and just had little labels on a circle. These arrows back and forth indicate that this is a process that you go through iteratively over and over.

When you make decisions, you have to communicate, even internally communicate, assess, analyze and manage. And the point to be made here is we started this already with the Yucca Mountain Site Characterization Plan back in the 1980s. It had an issue resolution strategy as the first go-around in this type of a loop. We'll talk a little bit about each one of these boxes.

But to analyze quantified uncertainties, this is everyone's favorite part because we know how to do this, analyses provide input to general assessment of uncertainties. Through iteration, analyses are modified as a result of changes in strategy, feedback, design, et cetera. Analyses include explicit treatment of quantified uncertainties, like in a total system performance assessment, and sensitivity and importance analyses.

Uncertainties quantified and treated in PA. Nice list here; process model complexity, conceptual model uncertainties. It's been covered by several people. There's also mathematical model uncertainty, variability and parameter uncertainty, you know, we know these things, unanticipated failure mechanisms, potentially disruptive events, and the uncertainty in the future states.

This approach captures what is known and recognizes there are limits to the analyses. And I think this is another reason to do this type of analysis, so you can stipulate what the limits of the analysis are.

Now, how do you go about treating conceptual and mathematical model uncertainty? You can test the consistency of a mathematical model by looking at trends observed in process models, in other words, the abstractions will be tested against the outputs of similar things from process level models. You can test alternative models against additional data, not the same data you used to calibrate, but additional data. You can conduct analyses for alternative models to provide perspective on any choice of a preferred model.

In other words, when we have a preferred model out of a selection of models, we will go through all of this analysis and try to make it clear why we chose the one that we did, or if we couldn't choose, why we didn't.

This is a rather busy slide, but it illustrates that, you know, this is a case where a model is applied to six different--or six different models were applied to some test data, and then we looked at which model was the best predictor overall of performance. And these things on the left are rather meaningless, except to just show that we have practiced what I'm preaching here.

Treating input parameter uncertainty and variability. We like this. We know how to do this. We represent uncertainty in parameters through probability distributions. We propagate them through Monte Carlo techniques. And if you look at TSPA/VA, there is a plethora of examples of that. And we look at the impact of parameter uncertainty on the performance measures in terms of expected mean and range variance of values.

Here is a sampling, in fact, this is an illustration, there is one like it in the viability assessment. Here is the parameter inputs. Here's the outputs. We intend to show the mean as it changes over time. We also intend to show the variance as it changes over time. That's our intention. That's what we did in the VA. We intend to keep doing that.

Disruptive events and future states uncertainty. Now we're getting into something that's a bit more difficult. We intend to, and we're busily doing this, identify relevant features, events and processes, or FEPs for short, as the shorthand developed in the international community, screen them and develop them and combine them into scenarios, formulate a nominal scenario and identify the associated models, and then estimate that scenario's probability. And then formulate disruptive event scenarios, volcanism, et cetera, using expert elicitation in that case, and associated models--there will be different models to describe the state of the system as one of those events occurs--and estimate the scenario probability for those scenarios.

Then we do Monte Carlo simulations for the individual scenarios, and combine the results into overall probability distribution. This is the classic total system performance assessment approach that we've been using for some time.

However, it should be noted scenarios are not constructed to represent all possibilities. They are constructed to evaluate significance and to be representative of the scenario classes that we generate. In other word, Budhi was saying, you know, everything has to add up to one, ensure that you're comprehending the universe. We agree with that in principle, but in practice, what we will show is that the scenarios that we will show you are either bounding or representative of the class of scenarios, you know, within which we could evaluate thousands of areas.

This is just a cartoon, something similar belongs in the viability assessment. We have conditional performance, multiply it for a scenario, the nominal and the igneous activity scenario in this case, multiply it by a probability, get a weighted performance, and then give overall performance. And every time we show the mean as it varies over time, we show the variances that varies over time.

Sensitivity and importance analysis. Now we get into something a little bit different. In addition to incorporating uncertainties and propagating them through a total system performance assessment, we get insight and perspective through additional analyses. And these analyses are not always realistic. They're not always meaningful in terms of the height of the curves or the shape of the curves, but they are done for insight.

We look at analyses of variance in estimates of post-closure performance. We look at parameter sensitivity analyses to examine the effects of specific values. We do trend analyses to look where uncertainties accounted for in TSPA are important. And, you know, we are glad that the NRC is fully on board with the idea that you put your effort in those things that are important to the outcome of your analysis. And then we also do barrier importance analysis, which are somewhat controversial even within DOE, because they are unrealistic, but we do them for insight to examine specific contributions of individual barriers.

And here's an example for illustrative purposes only. The shapes of the curves, the heights, the times are all quite meaningless. What I'm trying to show here is that we do analyses that span the range of uncertainty from the first percentile to the 99th percentile, showing the evolution of the mean. And we review a whole range of calculations, and compare that range against standards.

For illustrative purposes, we put a little box in there at the 25 millirem point, and what we are doing is saying, you know, the factors that we're looking at here, even if we go to the edge of the envelope where we think we're being reasonable, it's still orders of magnitude below, meaning that the factors we're looking at here are not that important to showing compliance.

So this is only for illustrative purposes. Don't read this as if it means anything. It's just to show this is the type of analysis that we're doing.

This is actually out of the VA, so you can take this somewhat more seriously. And we only showed mean values here. We didn't show the variances. But the idea is that we had different levels of CCDFs for the source term. This is the amount of material coming out of a given amount of waste, and we had a lower estimate, a best estimate, and an upper limit, and what we did is plot the sensitivity of total dose to that source term to see what the importance of that source term is. And the idea is to consider a possible range of conditions, to evaluate a set of specific conditions within that range, and then to go to "what if" calculations and evaluate very specific cases. So that these are inside calculations to give us a handle on when and why some things are important at different times.

And here's a trend analysis, and again this is for illustrative purposes. Don't pay too much attention to it. But if the red line is general corrosion rate and the black line is infiltration rate, it's pretty clear that if we can bound infiltration to a value less than this one, the results of the our calculations are pretty insensitive to that particular parameter.

So whereas the general corrosion rate, if that really is the curve, which this is hypothetical at this point, at all values is important to the outcome. So it's one way to consider the range of uncertainty and compare it to trends, identify ranges where certain uncertainties may be important, and consider variations for parameters that are not at their mean values. So it's a combination of all these types of analyses that we need to do in order to get a handle on understanding the system.

Barriers importance analysis. Again, these are unrealistic, but we do them to gain insight into how the system works. If we look at neutralizing a waste package, meaning that the waste package is physically there, but it serves no purpose, water falls through the waste package as if it wasn't there, and water bearing radionuclides comes out of the waste package as if it wasn't there, if we look at neutralizing that, we have the blue curve. If we look at neutralizing the unsaturated zone, meaning it serves no function, everything falls through, very unrealistic, and then we look at the base case, which does have characteristics for those, you can see that both the unsaturated zone and the waste package are important to performance. That's basically what this whole graph says.

Now, it's for illustrative purposes. We don't pay too much never mind on where those curves are, or exactly how high or when they start, because the point here is to evaluate whether or not something contributes to the total system's performance.

Next, we go to the thing that's a little bit more subjective, and that's to look at other uncertainties. The objective is to provide information to support uncertainty management. Now, uncertainty management strategy means, okay, here we have irreducible uncertainty, or uncertainty that we cannot reduce, you know, within the next two or three years, so we have to deal with it.

One way to deal with it is to go to a bounding analysis. Another way to deal with it may be to go to a more conservative design.

Now, the inputs that are very important come from what I've just shown you, which is the total system performance assessment and its feedback loops and its calculations. But it also evaluates and takes into consideration the limits to the total system performance assessment analysis, and it also looks at the analyses which are more subjective, unquantified uncertainties. It's a "what if," what if you're wrong about this type of argument.

We have to do this kind of thing in order to make decisions on how to proceed. We synthesize and assess the results of performance analyses. We look at the limits in the analyses, and that includes the limits in the models, the limits in the probability estimates, how sure are we of those, the limits in the scenario representations, could we have missed something important, and then we look at the confidence that we have in the models used in the analysis, and the importance of the uncertainties with respect to the conclusions.

So it's a way to synthesize and assess everything that we've talked about before. All of that needs to go into making a decision that costs maybe nothing, or maybe billions, on how to manage this particular source of uncertainty.

We know of some uncertainties, but have not incorporated them. For example, centimeter-scale heterogeneity, the burden is on us to show that even though we are aware that there is heterogeneity at the very small scale, that its a gross scale, and over the large time scales that we model flow and transport, for example, this may not be important. We have to make that case.

Non-linear friction forces in flow and transport, a favorite subject of some scientists, however, we are making an assumption that we can safely neglect those kinds of forces, because in the large scale experiments and large scale that we're looking at, these things have all been homogenized into the results that we see.

The potential for unknown unknowns, what if there's a failure mode for Alloy 22 that both our expertise and our experiments do not uncover? The future evolution of the system, you know, we presume that the system is pretty stable for a million years, like the National Academy said, and we presume that the only thing that can really perturb it is earthquakes and volcanos, and so we factor those in. But there may be things that we have not looked at.

And then surprises, what if we keep doing this performance confirmation testing and we find that the performance is not confirmed by that testing? You know, these kinds of things are unknown unknowns, very difficult to quantity, but yet we need to show by managing uncertainties that our system is robust enough to withstand even some of these surprises.

And then the objective of managing uncertainties is to look at the strategy for addressing uncertainties, not just addressing them quantitatively, but saying okay, here we have uncertainties, what do we do about them. It relies on the result of everything you've seen before, and it focuses on the factors that are principally involved in determining the importance of those uncertainties, and then focuses on the approach to reducing or mitigating those uncertainties.

And here again, you identify areas where the uncertainties are not critical, and take them off the list. You identify options for reducing or mitigating uncertainties that are critical. The word critical here means importance. We evaluate combinations of such options to address all critical uncertainties against such factors as the magnitude and importance, the introduction of new problems, feasibility of the options.

Here's another thing. You can come up with a solution that may not be feasible for either cost or other reasons. The effectiveness in addressing the uncertainties. You know, you can come up with a good scheme, and then later on, you find out you've just introduced a whole new bound of uncertainties that's harder to live with than the other. Demonstrability. We have to convince the licensing board that this is the right way to go. And then the cost for each option is important, but it comes last.

Now, the options for reducing or mitigating uncertainties, I've kind of hinted at these already, but basically, it's either you go out and get more information, or you go more conservative in the analysis, or you enhance the design to make that particular uncertainty less important.

You can also do other things to build confidence that you're on the right track. You can have an explicit discussion, you have to anyway, of key disruptive events. You can go to natural analogs and make arguments that argue that what you know pretty well bounds what nature sees. And, for example, if you look at the suite of secondary minerals in the uranium ore body at Pena Blanca, they're almost exactly the same as what we see in our experiments of UO2 dissolution. So we have a pretty good feeling that we're carrying those experiments long enough, because we see the same suite of secondary minerals that nature sees after tens of thousands of years, hundreds of thousands of years of its own experimentation.

And then we make a commitment in licensing, we will no doubt have to do this, to specific future confirmation tests where we have not convinced the regulator that we're done, and then select a set of options to identify principal factors for each one of these uncertainties.

Here's an example, and we picked examples that are really irrelevant, just like the pictures I showed you were irrelevant, because actually the idea whether you take cladding credit or no cladding credits has probably 15 or 17 perturbations that I know of. Here's two of them. You can take cladding with the waste package only, and retardation in the valley fill alluvium, and not look at dilution in the UZ, the SZ, and not look at the waste package and drip shield's role.

You know, those are just two of the 17-some options for whether or not you want to take credit, because everything that you decide in the workings of this system has implications both before it and after it in the way that the system works.

But some of the considerations that have to be made are the benefits of cladding versus uncertainties, cost to acquire additional needed data. That's a great argument for not taking credit for cladding in performance. You can just do a qualitative defense-in-depth benefit argument, and you can say that if you, instead, put your effort into looking at dilution, retardation, et cetera, you probably have a more cost effective solution to showing that things come out of the system quite slowly.

And so these are some of the things that are ongoing discussions within the project, and there's not just two, there's many perturbations and combinations of things that are being looked at. And, of course, the project has a procedure for when we make these types of decisions to fully document the basis for that decision. This is a decision still under consideration.

Here is another example of analyses that were done to gain insight, not to be indicators of performance. But if we make very pessimistic assumptions about a lot of things, and look on the VA design and the EDA II design and say that the waste package doesn't function anymore, everything else is quite pessimistic, you see that there's a big difference between the two, and that difference is largely because of the drip shield.

Now, when I say conservative assumptions, the reason that this is here at all within the 100,000 year time frame is because one of the assumptions is that the very first failure in the drip shield is co-located with a pre-failed waste package, so that you immediately thereafter start to get releases.

So, you know, we can do analyses that basically show nothing for both, because in the VA, if you remember, there were some 100,000 year calculations that showed no release whatsoever. So the reason that we make all these pessimistic assumptions and do these calculations is to gain insight into what is and what is not important. It's not to give us an indication of future performance.

Making these same assumptions as in the EDA II case right there, quite conservative, if we look at the difference between cladding and no cladding, it makes a pretty good argument that for a little while, it's quite important.

But as you come out and more of the cladding fails over time, and more of your contribution comes from other waste forms like waste packages begin to degrade generally and you begin to lose material from high-level waste, then you can see that it's not that big an issue anymore.

So, you know, these are the kinds of things, and this is the reason we want to look at time histories, these are all mean curves, you want to look at time histories and trends in those curves to see, well, how important is it to me to have this big of a gap temporarily. And that gap could be here, or it can be here, depending on other assumptions that you made. But it just shows that saying that cladding credit is very important is dependent on the time frame in which you're talking, and dependent on the model in which you're implanting it.

Okay, now comes the thing where we could use some help. We have an objective to communicate the results of all our analyses, including the uncertainty assessments, the approach to uncertainty management. We have to explain this to decision makers. We have to explain it to the TRB, the NRC. Congress needs to know, et cetera, and they need to have some degree of why, you know, some feeling of why they can have confidence in what we do.

Your objective in communicating--communication is a two-way street--is also to get feedback. We hope to communicate in a variety of ways to a variety of audiences.

We need to, in our communication, identify the sources of uncertainty, the magnitudes, potential impacts on post-closure performance. I have been in meetings where scientists basically drowned in their own uncertainties, and came away wringing their hands saying this is impossible. You know, it's kind of interesting that this was a meeting with hydrologists that I was at where they said, well, this is an intractable problem, and that some of the gentlemen there said wait, you know, we predict where to drill the next oil well, and we're pretty good at it, so don't discount it all, just quit taking your numbers so serious. And I thought that was very good insight, as qualitatively you could do a lot with this; quantitatively, it leaves a lot to be desired. And one thing that I have insisted on over the years is we quit calling our forward projections predictions, because you're not predicting the future. You're assessing a range of likely futures.

Provide information regarding credit and conservatism, and I think these are very important because to communicate to the public the mathematics and the charts that show uncertainties, variabilities, mean values, et cetera, is not enough. That's not going to convince anybody on the outside, only insiders.

You have to make arguments of why you're conservative, why it's probably not near as bad as the way you've modelled it. You have to show that you have defense-in-depth, that you're not overly dependent on any one functioning element within your system, that you have safety margins, that even your envelope, your envelope is well below where you're supposed to be to project health and safety. You have to explicitly and not hide the treatment of disruptive processes and events.

You have to go to natural analogs to explain that, you know, the reason that ore bodies still exist is because nature is rather conservative about moving things around, and you have to be able to show that you have a credible path forward to say we recognize there are uncertainties, and we will continue to work those.

Formal documentation for communicating uncertainty. We have AMRs, which are certainly not going to be sold at the local drug store, but they will discuss uncertainties in individual models. We have PMRs, project model reports--the other one is analysis of model reports--which will roll up the uncertainties into process models from the AMRs. We will have the total system performance assessment/site recommendation report which will quantify uncertainty at the system level. And we have the RSS, which you will hear about tomorrow morning, the repository safety strategy, discussing uncertainty assessment and the uncertainty strategy.

Specific plans for providing information to decision-makers, as well as other interested parties. We have those plans in formulation and, you know, basically it's a difficult problem to communicate confidence and uncertainty at the same time.

We communicate with you, with review groups. We have field trips. We talk to people all the time in public meetings, Appendix 7 meetings with the NRC staff, et cetera. And we continue to explore means, including the set-up in the back there, to simplify TSPA to try to take some of the mystery out of the black box of TSPA, total system performance assessment. We continue to look at means of communicating with technical and non-technical audiences.

Summary. We're evaluating uncertainties. We're getting more comprehensive about it over time. I think if you looked at products from '91, '89, '93, '95, and recently, you can see that there is an increasing sophistication over time, and we realize that we have to make a large leap in greater comprehensiveness and sophistication in order to pass the big hurdle of the license application especially, but also to convince Congress and the United States public at large that this is a safe undertaking in the site recommendation.

We will evaluate, and this is a promise, not only expected performance, but also uncertainties, including quantified and unquantified uncertainties. We will explain the uncertainties and what we're doing about them to audiences at many levels.

We recognize that the approach to uncertainty must be adequate to build confidence that the system will protect public health and safety, despite that uncertainty. And that's a heck of a challenge, and that's why I feel somewhat burdened.

Thank you.

COHON: Thank you, Abe. That was exactly a half hour. I appreciate it. Good presentation. It deserve applause.

I have great hesitation in doing this because I feel like I'm about to open the flood gates, but we do have ten minutes or so to start questions, and then we'll continue with the panel. Questions? Dan Bullen?

BULLEN: Bullen, Board, and it's for Abe, so you might want to stand up.

I was looking at your Figure Number 19. Well, Number 19 says assess confidence in models used in the analyses and importance of the uncertainties with respect to conclusions. How do you assess confidence? What are the criteria? How do you do that? I mean, is it a good feeling or is it--

VAN LUIK: No, that's a separate talk, and you can invite me back to give that.

BULLEN: But can I have like the Cliff Notes version of it?

VAN LUIK: The Cliff Notes? We assess confidence in the models by rigorously testing them, challenging the assumptions, et cetera, et cetera, bringing in outside experts to see if we are capturing the processes properly, and also in then laying out a program for looking at what else could we learn that could steer us. You know, it's a process rather than just a simple assessment, and I think you will see in the analysis and modeling reports, you will see our attempts at each model level to make a statement about confidence and where we go forward to build more confidence in the modeling. And sometimes building confidence means changing the model, too, when you see that it's wrong.

BULLEN: Thank you. I just have one more quick question for you on Number 25.

VAN LUIK: 25 said nothing, so how can you have a question?

BULLEN: No, 25 had pictures.

VAN LUIK: Oh, that one. Okay.

BULLEN: Yeah. And I guess the key here is that as you showed this example, you said that you're showing us EDA II versus the VA design, and then you're showing us clad credit versus no clad credit. But aren't you showing us EDA II has clad credit; right? I mean, these are the same curves?

VAN LUIK: No, no, no. On the right, I'm showing EDA II with and without clad credit. I should have made that more clear.

BULLEN: Right.

COHON: Furthermore, the one on the left assumes cladding.

VAN LUIK: Oh, yes, yes.

BULLEN: Okay. And so is clad credit part of EDA II?

VAN LUIK: Clad credit is part of this particular analysis. The argument, which was on the previous page, of whether we go forward taking explicit credit for the function of cladding in the SR and the LA is still an open discussion and dialogue.

BULLEN: Thank you.

COHON: Priscilla Nelson?

NELSON: Abe, stay. Nelson, Board. I have a question which I know it will be discussed more tomorrow, but it has to do with analogs, and the use of analogs in managing uncertainty, and in taking analog information and seeing it affect PA.

So I guess I'd be happy to have any comments from anyone about how analogs could be used in the model that is going on here, or understanding the uncertainty in the model. But for the specific case that you cited about Pena Blanca, how was the knowledge gained implemented or used to change conservatism or some aspects of the PA, if at all?

VAN LUIK: The specific example I gave is from observations by the NRC in looking at the uranium secondary minerals, and when we did testing in the laboratory at Livermore and at Argonne, they saw basically the same suite of minerals, one following the other, that we saw at Pena Blanca, and that gave us the indicating that we had probably reached the end point. The last phase that we saw is the last phase that nature also saw and, therefore, probably the last phase that we would expect at Yucca Mountain.

That was a qualitative helper to say that our length of experimentation was adequate. There are other things about Pena Blanca that tomorrow you will hear, you know, about some of our plans. But one of the things that we've already done is I have misspoken over the years, believe it or not, and said that the NRC's analyses are showing that we're very conservative, for example, on the transport rate of uranium through the system.

We surreptitiously did a calculation applying basically our TSPA tools, our total system performance assessment models, this last year to what we know about Pena Blanca, and I was totally surprised that what we estimated should be, you know, the output from Pena Blanca is not that different from what the NRC observed in their sampling.

So when we do that in a more stylized and a more controlled way, it was just a quick, you know, let's look and see how this looks, because I was expecting we would be orders of magnitude conservative, it seems to be right in the same order of magnitude, the same ballpark. So this may be something that confirms that our modeling is about on the right track, but it's not quite the result that I was hoping for, of course. But that's the kind of thing that we hope to get from doing something more quantitative at Pena Blanca by taking more sampling, et cetera, which you'll hear about tomorrow.

COHON: Paul Craig and Bill Barnard have questions, but I'm going to use the prerogative of the Chair here to ask my own.

I'm troubled and somewhat surprised by the lack of enthusiasm for quantifying uncertainty as a component of decision making. And let me elaborate.

There was a focus in your talk on the mean, and it's the mean that matters, recognizing of course that the mean incorporates the uncertainty to some extent, and NRC has always had this view in this project. But there's a lot going on here. Let me start with a specific question.

In your focus on the mean, is there an implicit assumption about attitudes towards risk, that is, that a decision-maker is risk neutral?

VENEZIANO: Yes, essentially there is. But let me answer the question. What I'm talking about is mean risk developed into what I was calling Type II uncertainty. That has to be differentiated from taking the mean of the dose, for example, dose curve over time. In fact, I had a question on my own, which is I do not understand--acceptance criteria on the mean dose, unless one can show that in fact the dose has a linear effect of whatever consequences one is interested in.

Given the inferences that there is uncertainty of risk and its quantification, I would applaud what I understand DOE is prepared to do, which is at least to provide some indication of uncertainty around the mean dose in the form of a standard deviation or whatever. But I do not understand well why one does not go into a quantification of the probability of, say, exceeding different levels of dose exposure. Maybe some people can clarify that, why in fact the acceptance criteria should be in terms of the mean dose.

COHON: Excellent. Thank you so much for that. That crystallizes I think the central issue. And let me suggest that we just discuss that at the panel when we get to it, because I'm sure every one of our participants will have something to say about that. That's an excellent response.

Bill Barnard?

BARNARD: Bill Barnard, Board Staff.

I have a question for Joe, and if you could put up your fifth slide? Where would you put Yucca Mountain on that diagram?

HOLONICH: It sits right about there, not a lot of data, never been built, analyzed, but not a high risk, a medium type of risk. So right here on the border between the brick and the--

COHON: Presuming you're doing some kind of multiplication of dose times population effect in that hazard?

HOLONICH: Yes, we're looking at--yes.

COHON: Okay.

HOLONICH: The question was where would we put Yucca Mountain on the graph, and I said it was right about here on the interface.

CRAIG: What does that mean, the incidence of concern have occurred rarely?

HOLONICH: It's never been analyzed, tested or operated, but we're collecting data and we're getting an understanding of it, and the risk is a medium hazard in terms of the risk from the type of facilities we regulate.

COHON: Well, his answers are consistent with what you just said; right? He's got it up high on--

CRAIG: He put it right at the borderline between never analyzed and incidents of concern have occurred rarely.

COHON: You could see why he didn't draw a dot on there.

Paul wants it at the top of your scale.

HOLONICH: He wants it higher up? Up here?

COHON: Yes.

HOLONICH: We've got some data. We're getting an understanding of the system, how it works. It's right there on that interface between never been tested and built, and getting a better understanding of it, being able to analyze it. That's our view. You can give us a different view.

CRAIG: I'm just trying to understand when it's at a borderline, with the thing that says incidents of concern have occurred rarely, and I simply don't know what that statement means. Maybe my problem is with your--but I remain mystified.

HOLONICH: Okay.

COHON: Okay, that's fine. This is a great advertisement for our panel discussion. I think it should be very interesting. Alberto gets the last question of this session.

SAGÜÉS: This is an observation for Dr. Sagar, but it could apply also to any of the other participants. It seems to me that the uncertainties are not only at the estimation end where we're trying to find out how likely an event will be and trying to assign a number to it, and so on, but it looks to me also like the uncertainties are also the specifications in maybe the goals that we're trying to reach. In transparency Number 4 of Dr. Sagar's presentation, we're talking about, for example, consider events that have at least one chance in 10,000 of occurring over 10,000 years. And, of course, why not one chance of 1,000 over 1,000 years, or one chance in 100,000 in 100,000 years. That right there puts us on a four orders of magnitude type of uncertainty of the specification, let alone the calculation end. And I think that that is as much of a problem as what we're trying to deal with at the other end, and I would like to know what you think about that.

COHON: You don't have to answer unless you want to.

Okay, I'm glad you can take advice. We're going to break now. I will leave a question on the table to be answered later. Mr. McGowan asks when and where is the next earthquake. And if you can't answer that, how can you answer--how can you claim that there won't be one. Something to ponder during the break.

We'll reconvene at 2:45.

(Whereupon, a break was taken.)

COHON: We've been joined by several additional people coming up here with the speakers that we've had up to now in this uncertainty session. They are Mal Murphy from Nye County. Mal, would you raise your hand? Thank you.

Engelbrecht von Tiesenhausen from Clark County, Abby Johnson from Eureka County, Steve Frishman from the State of Nevada, Judy Treichel from the Nevada Nuclear Waste Task Force, Rod McCullum from the Nuclear Energy Institute.

Now, I'll call on each of them to make brief comments of whatever sort they would like, and when they're completed, then we'll have a free-for-all, which I will try to referee. And why don't we just start at this end, and

Engelbrecht, if you'll go first, and we'll just move right along the table.

VON TIESENHAUSEN: First of all, let me state that I'm uncertain what I'm doing here.

COHON: Okay, hang on one second. To the speakers at the table, you've got to put the mike really close to your mouth. Otherwise, you can't be heard, and we wouldn't want that.

VON TIESENHAUSEN: All right. Well, I appreciate the opportunity to be here, and the issue of uncertainty is very critical, I feel, to especially the citizens who will be involved in this program in the future. And like Abe, I have a very difficult time on how to communicate this issue in an understandable and reasonable manner. I feel that in this case, I'm an engineer by training, and I can understand reactors and uncertainty concerning reactors. I have a real difficult time projecting that 10,000 years into the future, and making much sense out of that.

So I look forward to being enlightened by the rest of the group.

COHON: Thank you, Engelbrecht. Abby?

JOHNSON: I'm Abby Johnson. I'm the nuclear waste advisor for Eureka County. I'm a last minute addition to this panel, so my thought process is a little slower than everybody else who's had days to think about it.

I don't have any particular insight or wisdom. I bring to the panel the sort of common sense, I'm a citizen in Nevada, tell me what to make of this point of view.

The one thing that I've always thought is that if we had a major earthquake at Yucca Mountain tomorrow, that the result would be that the Department of Energy would say well, we've gotten our 10,000 year event over with, let's move on. And so to a certain extent, that kind of additional information, that Type II information, then just makes us more certain of the course we're going in, even if the information on the face of it to the average citizen seems, in fact, to say gee, we're going in the wrong direction.

That's what I'd throw out as an initial reaction to what I've heard so far today.

COHON: Thank you. Rod?

MC CULLUM: Yeah, on behalf of the Nuclear Energy industry, I'm very glad to have been given this opportunity to be on this panel. Also, I was very encouraged to hear the remarks of Dr. Itkin and Dr. Dyer earlier today about the Department's commitment to presenting and clearly communicating uncertainties. I agree with Mr. von Tiesenhausen that this is very important. We're entering a critical window of decision making opportunity here that started with the release of the draft environment impact statement, and will continue through the site recommendation consideration report, and up to a Secretarial recommendation and Presidential decision.

One of the things that will weigh the most heavily on these decision makers is uncertainty. It will be a critical component of the decision. And we have 20 years more good science that has gone into this, and one of the reasons we know it's good science is because one of the hallmarks of good science is that every answer produces still more questions.

These questions manifest themselves in uncertainties, and it's no surprise with something like this that we do have a lot of uncertainties.

The good news is that we are equipped with a decision-making process which is good at making decisions in the face of uncertainty. I would submit that the reason the United States has gotten to be the nation that it is today is because our democratic process facilitates our leaders making decisions in the face of uncertainty. This Board, and all the steps that we're about to go through over the next 18 months are functions of that process.

As we go through that process, I think there's three things that the decision makers can do with all this uncertainty. They can choose not to accept it, in which case it's either a no decision or a decision that more science or more design changes are needed. They can choose to accept it based upon what is known today in terms of how important is the uncertainty, or what pessimistic assumptions or countervailing conservatisms exist that they can account for a design margin.

And something that this process gives us that I don't think has been explored enough, and would hope would be encouraged to be explored more is the notion of accepting uncertainty based on what we expect to learn in the remainder of the process.

We have a four step process here, a site recommendation, a license to operate--or excuse me--a license to construct a repository, a license to operate a repository, and then finally, a license to close a repository.

We have an opportunity for those areas of uncertainty that are weighing heavily on the decision makers to lay out dedicated research programs as we move through performance confirmation and to license application. We'll address those things, and I look forward to DOE packaging this and telling us what those things are and what those plans might be, and any discussions we'll continue to have on this panel, because I do certainly agree this is a very important issue.

COHON: Thank you. Judy?

TREICHEL: One of the things that struck me while I was listening was that for years and years and years, we've heard about something called acceptable risk, and that gets determined generally by whoever sets the standard decides what the acceptable risk is, and then they put some numbers to that. And I suppose at some point, there's going to have to be a decision about what an acceptable level of uncertainty is.

And I feel as a representative of people, a public advocate, that we're going to be in the same box we are with the acceptable risk idea, and there are a lot of kind of, oh, sort of difficult to define words that get thrown around. Acceptable risk, reasonable assurance, and on each of those, you can say acceptable to who, reasonable to who. And who is going to determine what the acceptable level of uncertainty is? And if you don't agree, what do you do about it?

And I guess that's where the battle lines have been drawn, because as the project marches along, Dr. Itkin mentioned to us that if nothing else stays in place, by George, that schedule is going to stay in place, whether they have even a shuffle of contractors, or whatever, nothing gets in the way of the schedule. And he tried to claim that that led to public confidence, and in fact, that's just the opposite.

What we worry about when uncertainty is discovered is that it becomes reduced by sort of relaxing something else, and it doesn't become reduced because it runs up against the schedule. And the things that are very important to the public usually fall victim to the schedule.

So I suppose that's where the frustration and the anger and the battle comes in. But once again, as I've said many times, Nevada is a very difficult place to make this argument, because the Department of Energy doesn't have a good record here. There's been problems with testing that went on for so many years, and there are a lot of people who were hurt or seriously injured by that, and what we heard was, well, we just didn't know at that time what we know today.

Well, I have the feeling we just don't know today what we will know tomorrow and the next day when it comes to nuclear waste management, disposal, whatever, and I don't know if I was the only one that was shocked when Joe Holonich put up his graph with the brick wall and placed the repository where he did. I don't think most of the public would agree with that. I think they would see it right on the upper right-hand corner of the thing. And so his confidence level is probably far higher than most of the public, and I don't know how we compete with that.

Thank you.

COHON: Steve?

FRISHMAN: Let me just start with the idea of acceptable risk. I remember quite a few years ago, a discussion in a meeting of this Board where people were somewhat taken aback by looking at the graphs of performance and uncertainty, and seeing four to five orders of magnitude uncertainty. And someone on the Board asked at that time, well, what's acceptable uncertainty, and I remember someone else saying, well, it's not five or six, but is it two and a half?

So I think the question is a legitimate one. It's not answered so simply. But at some point we're going to have to find a method to deal with it. And if you recall in the preamble to the proposed EPA rule, there's a little discussion of that, and says that the level of uncertainty is expected to be relatively high, and they didn't use reasonable assurance, they used reasonable expectation, that for some reason is a little bit different.

But anyway, that's something that is going to surface, and I don't know how it's going to be resolved.

Now, to move on to just a couple other things, the two very sensitive performance components, as we all know, are the engineered barrier and seeps. These have extremely high uncertainty associated with them now. I don't know that in the time between now and licensing, if there is a licensing proceeding, that they can be reduced, and I'm speaking in terms of long-term performance. I don't know that they can be reduced.

I don't know how they're going to be dealt with in a licensing decision. I don't know how they can possibly be dealt with in a site suitability decision, because you look at the performance assessment, and it's a case of, in the range of the analysis, the repository either vastly exceeds any reasonable standard, or provides a minimal dose. You can't have the coin flipping in this case. It just doesn't work to have a performance assessment coming out saying, well, maybe it exceeds it and maybe it doesn't.

Now, if you sort of extend those two most sensitive components to the latest greatest design idea, which is the idea of keeping the repository open for about 125 years in order to keep the wall temperature below boiling, what is performance confirmation going to do? It's not going to be providing you data that has anything to do with the performance that you've proposed, because you're keeping the repository in a condition in which you won't be able to take data on what matters in terms of whether you're possibly right or wrong about the engineered barrier and seeps. You're not going to allow any test. I don't think in 125 years you're going to be able to tell anything anyway. But this latest design idea precludes any value of performance confirmation in this area.

COHON: Thank you, Steve. Mal?

MURPHY: Thank you, Dr. Cohon. I, too, am glad to be here. I certainly, on behalf of Nye County, appreciate the opportunity to take part in this discussion. Most of the points that I was thinking about making have already been raised by other panelists. I just want to say a couple of things very briefly.

I'm sure, as almost everybody in the room knows, the level of uncertainty in this program has been of great concern to Nye County for a long time. That uncertainty--and by uncertainty, I mean the data sort of uncertainty, and that concern is one of the reasons for the Nye County Scientific Investigations Program and the Early Warning Drilling Program, Phase II, that Nick Stellavato discussed with you this morning.

We have always insisted, if you will, that more of these decisions be based on hard data than on conceptual models, mathematical models, and expert judgment. We think the program has been moving recently in that direction, and that's one of the reasons why we have our own EWDP to collect that data.

And in that vein, I guess, if you wanted to sum up in one sentence, you know, Nye County's views on uncertainty, it would be Warner North's quote from Bob Bernero, that is, to judge on a body of knowledge and not on an equation. And we think expanding that body of knowledge with hard scientific data conducted under a good quality assurance program is the way to go.

Another position of Nye County historically in this program is one that Joe Holonich articulated on behalf of the NRC, and that is the more uncertainty you have in the program, the more conservatism you also have to have in the program and in the decision, and we're happy to hear that kind of language coming out of the NRC, and from Abe as well, to give DOE credit in that respect.

There is one other problem, though, I think that I'd like the panel to discuss that has been alluded to and discussed somewhat, and that is the very, very difficult issue of communicating this uncertainty and communicating why it is, how you're making the decision, whatever decision you're making, and why you're making the decision in the face of whatever degree of uncertainty remains at the time of suitability determination and licensing, and communicating that in an understandable way to the public.

I think everybody understands that that's a ticklish problem, and I hope we can bounce some ideas back and forth in that regard.

And finally, I want to just touch upon, this is not the forum to discuss it, I realize, but I want everybody in the room to appreciate that there is another overwhelming uncertainty in this program, which has been a great and continues to be of great concern to Nye County, and that is the funding uncertainty in the long term future.

Bill Barnard and I were discussing it just during the break. At some point in time, whether it's 50 years, 125 years, but at some point in time, the government, assuming again suitability, assuming licensing, et cetera, at some point in time, the government will say well, we're done now. It looks fine to us. It seems to be working. And so we're going to clean up the site and restore it and put whatever markers and monuments are required, and we're out of here. We're comfortable with it.

And the folks who are going to be left to watch Yucca Mountain and monitor it, worry about it for the long, long, long-term future, the state of Nevada, and more directly and specifically, Nye County and the program, and I understand that this is not Russ Dyer's problem; it's Congress's problem, but the program right now makes absolutely no--does not take that into account and makes no provision for how the state and how Nye County is going to be funded to continue that very, very long-term monitoring.

That's an uncertainty which has to be addressed at some point in time, it seems to me. It's one of great concern to Nye County. I just throw it out there on the table, because I understand this is not the forum with which to deal with it, but I hope everybody appreciates it.

COHON: Thank you. Ground rules here are if you all want to say something, just raise your hand and I'll call on you. Board members and staff are encouraged to ask questions. And we'll see how we go for a while.

Does anybody want to say something in response to anything you've heard?

MURPHY: If I could, I'd like to start with a question to Joe Holonich.

COHON: Go ahead.

MURPHY: With your little chart, or whatever that's called. Two questions, I guess, Joe. Number one, whose thought is that when you put the dot up there? Is that your dot or is that the official Commission dot?

And secondly, if that dot stays right where it is, if it doesn't move at all, is the placement of that dot satisfactory for construction authorization and/or license to receive and possess?

HOLONICH: First off, it's kind of the staff's dot, my discussion with the technical staff. It's not a Commission dot. The Commission hasn't said that's where we think the dot goes. In fact, the whole graph there is the staff's presentation. So it is the staff's dot, me and the technical staff sitting down and kind of talking about where we thought it fit.

In terms of--ask your second question again, Mal.

MURPHY: If the dot doesn't move, will you, if you're the staff czar at the time, would you grant a construction authorization?

HOLONICH: Well, I don't think the staff would grant the authorization. It will be the Commission that will grant the authorization. But at that point, you know, the Commission has said in the statement of considerations for Part 63, that it sees defense-in-depth as a mechanism to be able to treat the uncertainties in the program, and to make sure that there are adequate protection measures in place.

And so the commission has put what it thinks are necessary defense-in-depth provisions in the rule. So I think we've got laid out in the rule what we would need to in terms of dealing with defense-in-depth, which is what the graph was trying to show, what level you needed.

COHON: Let's not get hung up on that diagram, though. I mean, you have a right to question it and maybe even be concerned about it, but as I understood, it was simply a characterization of your understanding of the nature of the uncertainty and the hazard. It's not literally a quantification of what that uncertainty is, or how it's going to be dealt with.

HOLONICH: Right. It was just an attempt to show how you need additional measures of defense-in-depth, the more hazards you've got in the system or the more uncertainty you've got in the system. If you've got a system like smoke detectors where you've got lots of data and little hazard, you really don't need defense-in-depth. So it was just trying to pictorially show how you would incorporate or include consider defense-in-depth, depending on the hazard and the amount of data and understanding you had.

COHON: I'd like to go back to the question that I posed during the session before the break, that is, the issue of quantification of uncertainty, and by extension, the presentation of that uncertainty to decision makers and to stakeholders. And Daniele responded to that. I don't know if you have more to say. I know Warner has something to say about this.

Daniele, do you have more to talk about at this time?

VENEZIANO: I might again pose the question as to why was it being regulated as a mean dose rather than a full characterization of risk, meaning probability of exceeding different levels of dose.

COHON: So for DOE and NRC, why no quantification of uncertainty? Why the focus on mean? We grant you you've got all sorts of characterization of uncertainty, but the question is why not quantification, a number?

SAGAR: Well, if I might?

COHON: By the way, for the recorder's sake, I forgot to mention this, please identify yourself every time you speak, because he'll go crazy otherwise.

SAGAR: This is Budhi Sagar from CNWRA.

I think there were two reasons why the mean was selected. First of all, the relationship between the mean dose and the cancer risk is assumed. Therefore, one was assumed more or less equivalent to the other. And the risk is always a mean. It's an expected value, a probability rated average anyway.

The second reason we found that most of the analyses we had seen done for Yucca Mountain, and the analyses that were done at NRC, indicated that the mean dose actually had a probability of 90 per cent. The probability distribution of the mean dose, peak mean dose, was cued towards the right, so that the mean had a really high probability in the sense specifying another limit, for example, for 95th percentile, or some such number, seemed not to add to the safety issue that we were trying to regulate.

Those were the two questions. Those are the two reasons underlying the specification of the mean dose.

VENEZIANO: So you say that the mean corresponds roughly to an 85 percentile? So the value exceeded the probability 15 per cent?

SAGAR: That's correct.

COHON: Warner North?

NORTH: I have a couple of points I'd like to make on this. I'd like to start with how do you explain it to the public. And when people use the technical term "mean," it seems to me there might be an advantage in explaining that this is an average over something. And what it is we're averaging over becomes very important information.

For example, are we averaging over space measured in feet, measured in miles? Are we averaging over time measured in years, millennia, or whatever? Are wee averaging over variabilities, such as climatic fluctuations, day to day, week to week, ice age to ice age? Or are we averaging over our judgment about which model may be right, epistemic uncertainty?

It seems to me really critical to disclose that, and maybe illustrate it by showing the calculation. If you have probabilities and you have scenario outcomes, or models, or ice age dates, it might be very useful to take people who don't think intuitively about a mean of a distribution, and show them, well, we've got this possibility here, and we've got a probability assigned to that. Now, let's think about the case.

Let me give you an illustration. I think this is in the area of standard setting by EPA, but I think it's an important issue when we talk about 25 millirem versus 15 millirem. I'm thinking at the level of what is the diet of an individual that is using water that is contaminated by radionuclides from the repository sometime in the far future.

Let me suppose there is a vegetable that concentrates the lead radionuclide and this individual far in the future happens to be a vegetarian that loves to eat large quantities of this particular vegetable. I don't even have a good candidate. Brussel sprouts, artichokes, something like that. Anyway, this person eats a very unusual amount of that food, and as a result, has an anomalously high dose relative to our standard. Well, are we averaging over the population's dietary habits? Are we protecting this individual, or are we simply averaging across lots of different dietary habits on the basis of a year 2000 plus X projection of what is a normal diet?

I think if we worked on it, we could think of about 50 questions like that in terms of exactly what is the scenario. And it seems to me there might be a lot of value to disaggregating so we show what is it we're averaging over, and how the calculation is being made, and get away from I'll call it relatively arcane language in terms of the way the regulation is written and the way the performance assessment is carried out.

Maybe we might all agree that we are not going to go to enormous lengths to protect people who have very unusual diets. But at least it seems to me that's an issue the public needs to understand.

COHON: Go ahead, Judy.

TREICHEL: I guess this comes back to the thing that you're going to hear from people all the time on this, is it's more important to me, as John Q Public, that you find a better site than that you reduce uncertainty or that you play numbers games with Yucca Mountain. Because if you had a site where you were confident that you could have zero release at the door forever, as some countries are looking at, you wouldn't have to worry about that.

And the one thing that I worry about is what do you mean or what others mean, and a lot of people talk about we need to educate the public, wee need to figure out a way to tell them right now. What do you see as the test for when you've done that right? Is that when they say it's okay and they accept the answer, or are they allowed to say I understand this, but I still don't go along with it? Or does that indicate they need more education?

COHON: Go ahead, Warner?

NORTH: I'd like to try a response of that of let's consider the decision to get on an airplane. I think that's one where the public has been educated over a long period of time, and there's still a lot of people, I know some well who are very competent analysts and as familiar with risk numerology as I am, and they still don't fly in airplanes.

On the other hand, an awful lot of us do, and an awful lot of us decide I will get on this airplane under these conditions, and I'm not going to fly on that airplane under those conditions. There are certain countries on the other side of the world where I really don't want to fly on their airplane.

I also consider a situation I had recently where there is a young man who has just received his pilot's license, and there is a relative of his age 80 who is a very experienced bush pilot in Alaska. And my personal decision was going to be I won't fly with the bush pilot because I'm worried about a health problem. I won't fly with the young man because he doesn't have enough experience. But if they're both in the plane together and if something happens to the old bush pilot, the young man can probably take the airplane back and land it at an airport. I'm comfortable. I'll get on the plane.

I think people have a great deal of ability to think through what affects them, and we need to be able to present them with the information so they can make informed decisions.

I think it would be wonderful if we could propose that. We are so secure in this one site that there is no possibility of any release of any radioactivity and, therefore, we're going to go there. With the experience I have looking at a number of national programs, there are lots of ways things could go wrong, and it's very hard to sit there and say we have a site that's so good and a program that's so secure that we're sure nothing can possibly go wrong, no chance of any release.

I think we're going to have to make judgments about the uncertainties, and we're going to have to make a lot of comparisons. I certainly don't feel I've got any revealed truth in exactly how you go about doing it. I tend to agree with the reply to Mr. Sagar's article on the opposite page by Konokoff and Ewing saying the devil is in the details. The devil really is in the details, and I think we have to iterate to get those details right.

COHON: Abe?

VAN LUIK: I'd like to agree with you. The idea that there are repository programs that are looking at no release forever I think is a myth. The expectation was that this was going to be true for Crystalline Rock, for example, but Switzerland has all but abandoned Crystalline Rock because of the uncertainties in the future state of Crystalline Rock in an active uplift environment. And so now they're looking at clays more actively. They haven't abandoned Crystalline, but they're moving in that direction.

So the reason that the Swedes and the Finns have a marvelous no release for a million years repository is because of their total reliance on the waste package and the engineered barrier system around it. And so I don't see that much difference in the approaches or in the outcomes. If you look at our expected case, you know, even for VA, we have many realizations up to over 100,000 years with no releases.

And so I think the point is that we are informing, like through the DEIS process that, yes, there is some risk associated with this repository. Society, make a decision. Is this an acceptable risk.

Now, the point is well taken. We're not explaining to people how they should judge this acceptable risk. And, for example, the calculation of dose to an individual, I know how we're doing that calculation. I know that there's a million ways to do that calculation, and frankly, we are looking for guidance from the regulator to tell us what the path is through that quagmire that would be representative of a reasonable path, and that would be acceptable to society at large. And that's why the rule-making processes are in place.

We are looking at annual doses over 100 year spans, averaged over those 100 year spans at the same location for the same hypothetical individual forever. It's a hypothetical individual. It's not a real individual. And that's how we're calculating that dose. But even that, even though to me it's a great simplification of something that could be real complex, is somewhat questionable.

And then the other point about seepage, yes, seepage has great uncertainty. We have secondary evidence from the ages of groundwater in the mountain that we're probably being extremely conservative. But there is uncertainty and we recognize it, and that's why some of us who kind of doubt the seepage are calling the drip shield an uncertainty shield, which is exactly what it is. It's a guard against the uncertainty in the seepage rate.

And so there's all these factors that if we put them together in a communications package, might sell well. But if you take each individual one apart, you see that there's a facile way to tell this story, but that facile way at the hands of an expert can always be challenged. And so it's real difficult to communicate at different levels to different people.

I can spin a yarn that will make you feel real secure about the site. I feel pretty secure about the site. But then I would have to simplify to the point of absurdity all of the uncertainties that we're dealing with. So, frankly, I need help.

COHON: Russ, before you go, let me just follow up with what Abe just said, and keeping on this issue of quantification.

I feel the outcome that I anyhow would anticipate is at the point of a decision when the program recommends to the secretary a course of action that there will be a base case, which will be a curve like the one we just saw, presumably showing that it does not exceed the standard, and then a volume like that, which is the sensitivity analyses which is your characterization of uncertainty. And let me acknowledge this is a tough problem. This is not easy to deal with. There's a lot of uncertainty, a lot of complexity and interlocking effects. But clearly we would all agree that the result I anticipate is not acceptable.

Now, let me just point out another thing related to this. NRC in its decision making is one thing. But suitability is an old horse that I keep whipping, is something else, and we've got to get through that before you get to NRC. Undoubtedly, unless Nevada blesses the repository, that means you're going to have to convince Congress that this site is suitable. That's 535 people who will need much more than a base case and a volume like that. So that you've got to come to grips with this. There's one more thread here to tie back to something.

It was said before, Daniele said it, that he had that decision diagram which ended in a final action, and he said well, let's see the final action, because you have to-- the final decision, because you have to know what it is you're going to decide and what the criteria are for that decision in order to do all the stuff that comes before. I come back to this issue of what are the decision criteria when it comes to uncertainty.

Thanks for letting me make this speech. Russ? Russ, Rod, and then Steve.

DYER: Actually, I think you started my little dissertation for me here.

We've talked quite a bit about details of uncertainty and how you quantify it. But what I'd like to address is I don't know if it was designed or not, but there is built into the nuclear waste program, not just for this country, but I think for every country, there is an inherent, almost an inefficient process for dealing with uncertainty, and that is that there are a series of small non-irreversible steps that one takes. So one takes one step, observes what's happening. Then moves on to the next step. And I think certainly what is facing us for the site recommendation is what is the level of uncertainty that you need to address and be comfortable with to make that next step. Because we're not talking about all in one fell sweep, constructing, building and closing a repository. It is just the next step on this long process.

COHON: Good point. Rod?

MC CULLUM: Yeah, I want to thank Dr. Dyer for those remarks. I think that is very important to realize that we do have an approach that allows us to address uncertainty as we move to closing this repository.

And I'd like to thank the architects of this process, the Congress and all the input they had. We're smart enough to realize that in fact it is by a design. And I want to get back to what I originally was intending to say, which was built on something that Abe had said about the myth of zero risk.

I once saw a sign on a building somewhere, and I forget where, that the greatest risk of all is zero risk. There is not zero sewage in this glass of water. There is some quantity of sewage here. But we all accept that it's below some level that we have defined, and we routinely drink the water that comes out of our tap.

Indeed, the risks of trying to have water with zero sewage would require us to turn off so many things that we do, that a lot of bad things would happen. And when decision makers are looking at these balancing of risk questions, there are a lot of uncertainties out there that don't pertain specifically to Yucca Mountain that will weigh on their decision, just like there are a lot of things that affect this glass of water.

There is the uncertainty on America's electric power supply of not having a repository. There is the uncertainty that's placed on our children of this generation not managing the nuclear waste issue effectively and in a reasonable period of time. I think that's why the schedule is important.

So there are all these things that have to be considered and weighed, and it is a vast political undertaking, and that's why it goes to our President and to our Congress. It is a very important national decision.

Now, in order that they make the right decision, and if you look at the history of our country, I think this political system has a pretty good track record, they do need ways of taking the uncertainties that are specific to Yucca Mountain, knowing what they are, knowing what knowledge we have now that speaks to those uncertainties, and knowing what they can do throughout the rest of the process, I go back to what type of program we're laying out as we--if we would move towards license application or performance confirmation, and the decision makers need to be aware of that as they go through so that all the risks on all sides can be balanced, and they can indeed choose what's best for the country.

COHON: Thank you. Steve, and then Mal and then Engelbrecht.

FRISHMAN: There's one thing that I guess has bothered me for quite a while, and that's that the greatest uncertainty in the whole system seems to be related to the 10,000 year regulatory cut-off. Because there's an uncertainty--well, the real uncertainty is not in performance. The real uncertainty is in the performance assessment, because you can turn just one dial in the performance assessment, and you can have unacceptable doses inside of 10,000 years, and that's instead of assuming one juvenile failure, you assume a hundred juvenile failures out of 11,000 packages.

That one assumption in the performance assessment I think is the biggest uncertainty, and I think it needs to be dealt with. And I don't know whether Abe wants to deal with it, but I recall how difficult it was for them to even accept the notion that there would be juvenile failure. And almost every system, and I think, Dan, you can probably speak to this better than anybody in the room, almost every system can expect juvenile failure.

COHON: Any desire to respond to this, or should we move on? Abe?

VAN LUIK: The desire is to respond in two ways. One is that one of the reasons to put the uncertainty or drip shield on is to make sure that the uncertainty in the juvenile failure factor is not going to be a controlling factor.

FRISHMAN: That's the most enormously expensive bandaid I ever heard of.

VAN LUIK: And the second is that we are putting a lot of effort into, one, establishing a basis for the distribution of failures at receipt and then after emplacement and, two, putting in place whatever we can to assure that these things are going to be controlled and not have any. But I grant you this is a large uncertainty in the whole undertaking.

FRISHMAN: Can I just follow up on that?

COHON: Sure.

FRISHMAN: With a wise remark? How many people believe that at the end of some period that could be as much as 100 to 125 or more years, that Congress, with no money from the Waste Fund, is going to spend billions on titanium drip shields?

MC CULLUM: Just a very quick response. What makes you think there would be no money for the Nuclear Waste Fund at that time? That's just a rhetorical question.

COHON: Okay. Mal?

MURPHY: Mal Murphy, Nye County. I just wanted to point out I liked Warner North's octogenarian bush pilot and novice analogy. You know, that's a very simplified explanation of defense-in-depth, for example, but I just wanted to point out that with respect to that analogy as well as drinking the water that contains some sewage, and incidentally, I have a better example than that, both of those pertain to voluntary risk. You voluntarily get in that airplane with the 80 year old bush pilot, and the kid who just got licensed last week, and you voluntarily took a drink of that water.

The people of Nye County are not going to be given the opportunity to voluntarily or involuntarily accept the risk of Yucca Mountain, assuming that it is declared suitable and licensed. For some people, that risk, no matter how low we get it, for some people, the risk will never be acceptable. That's going to be involuntarily imposed upon them, Nye County, all of Southern Nevada, and for folks along the transportation corridors as well. And it seems to me that dealing with and addressing and disclosing and making transparent uncertainties which people may voluntarily avoid is a lot different than dealing with and disclosing uncertainties which people cannot avoid, or can avoid only by uprooting themselves and giving up their farm which has been in their family for four generations, and moving somewhere where they don't want to live. That's an entirely different set of issues, it seems to me.

The better example is I wonder how many people in the country realize that the U. S. Department of Agriculture, when you talk about voluntary risk, the U. S. Department of Agriculture has, by regulation, acceptable levels of rat droppings in wheat, and how many people, if they knew that there was a legally okay number, expressed I suppose in parts per million number of rat droppings in their bread, how many people would voluntarily decide not to eat bread. But, you know, so we should disclose the number of rat droppings that are allowed. There again, that's a voluntary risk when I have my hamburger with a roll.

COHON: Thank you for that, Mal. Engelbrecht von Tiesenhausen?

VON TIESENHAUSEN: I'd just like to change the subject from rat droppings to something else. We've discussed many kinds of risk, and one thing that I haven't heard mentioned, and maybe I missed it, is human factors. And the people that are doing the analyses, engineers, scientists, we all tend to make mistakes. Some of those mistakes are critical, some are not. And I just wonder what kind of thoughts Abe has on this issue, and will this be addressed in any way, shape or form?

COHON: Abe?

VAN LUIK: In fact, I take great comfort in the fact that our analyses are independently--not our analyses, but independent analyses are being done and have been done by the Nuclear Regulatory Commission, by the EPRI folks, the Energy--the Electric Power Research Institute, by the MTS organization, who is, as you can see in the rear, gearing up to basically help themselves review the work of the M&O by redoing it, and by having the Technical Review Board look over in very great detail pieces of the puzzle.

I basically agree with you. This is an issue, and without that kind of oversight, we can't be sure, we'll never be sure that this is the absolute truth in a calculation, but we will be sure that the best science and the most rigorous thought has gone into the process I think through that type of review.

So even though we bear a great burden through these reviews, and they're not pleasant, they are absolutely necessary to assure that the best work is being done for society.

COHON: Paul Craig?

CRAIG: Paul Craig, Board. This is a question which is really I think addressed mostly to Daniele Veneziano and Warner North, but anybody else--a Daniele and Warner type question. And it has to do with the aspect of decision making that you almost always, maybe you really do always have to say what would I do instead. You can't just say make a decision in a vacuum, but you also have to say what happens if the decision is negative.

Now, the Congress, with respect to Yucca Mountain, hasn't provided any alternatives, and so in some sense, that's not on the agenda, but on the other hand, on the famous brick diagram that Joe Holonich showed us, he ranked the public health hazard of this, independent spent fuel storage, below the little place where he put Yucca Mountain, which leads one to suggest that at least somebody thinks that maybe the risk of Yucca Mountain is higher or, alternatively expressed, maybe the risk of ISFS isn't so great.

And so I'd be interested in asking you to help us out to think about the time urgency of the viability decision, which is, after all, the one that we're most concerned about, it's a go, no go decision, in the context of alternatives, and where we might be if there were a little bit more delay introduced so that more information might be collected.

COHON: Go right ahead if you want.

NORTH: Warner North. Yes, the framing of the problem is very important. And there are a lot of ways this problem can be framed, and I think there's been a great deal of discussion. Perhaps one extreme, this is a "not in my back yard" problem, and maybe at the other extreme, it has to do with the future of nuclear power, and then a lot in between.

I'm not sure in this meeting it's really useful for us to get into that debate beyond acknowledging there is a much larger public policy context into which what do we do about site suitability for Yucca Mountain fits. And I think I'd rather not talk about it, given my role on the Academy Committee following the workshop. I hope you will find our report very illuminating on this particular subject.

COHON: Daniele did you want to respond to that?

VENEZIANO: It seems to me that many of the concerns about the acceptability or not of a certain risk or level of uncertainty would probably be put to rest or mitigated by explicit consideration of alternatives to a certain decision.

It is very much possible that in fact alternatives would be worse than any of--our acceptance of a large range of uncertainties has been pointed out before, and in fact possibly they are not better alternatives. I don't know that. But certainly to cast the problem in a relative sense rather than absolute would greatly facilitate any decision, at least at the conceptual level, although it may be very difficult to do, to make analysis of many alternatives, and so on.

And that probably also goes to the issue of delaying the decision, which might be formulated as alternative decisions. Do we decide now or do we decide later, and so on. So, yes, I do see benefits from that kind of exercise to make sure that one is not boxing one's self into a single decision and not considering alternatives. I do not know the degree to which one can do that, one can implement that.

Much has been said on a slightly different issue, much has been said, it seems to me, around this table about the resolution of some of the uncertainties over time, and I'd like to reiterate something that I said in my own presentation. It seems to me that one has indeed to structure the decision process in the context of information acquisition, so that one makes a decision thinking that the current level of risk, or average risk, as I put it, but the current assessment of risk is subject to evolution, and in the face of that, one has to exercise conservatism. And it's certainly very difficult to quantify the future evolution of our risk assessment. There is no question about it. But I believe that an intellectually correct framing of the problem may help at least in saying are we including a reasonable amount of conservatism in our decision. What should that reasonable amount of conservatism be?

For example, about seepage, the amount of seepage, there is much uncertainty, as I understand, in this parameter. How much of that uncertainty will be reasonably reduced over a period of 50 or 100 years? If the uncertainty will be reduced in terms, say, of standard deviation by half, then that would give us a reason to build in that sufficient consideration that let's say is a small likelihood, this level that we are assuming today for our decision will be exceeded over this intervening period of time before closure.

And I think this kind of reasoning would be very helpful in addressing some of the issues of a very large uncertainty today, that today exists. So I think that in fact one can make one additional step probably in addressing these issues.

COHON: Abby, and then Joe.

JOHNSON: With a program that's so terribly schedule driven, that makes it very difficult to give the uncertainty the time that it needs. Similarly, Rod had mentioned the responsibility of this generation solving this problem, and in fact it's very possible that that's the irresponsible thing to do, given what you just said, that what we need to give it is time.

VENEZIANO: May I respond? The point that I would like to make is this. Suppose that you have to decide today rather than two years or in ten years, and today, your level of uncertainty will be greater because you haven't conducted those tests, you haven't collected that information, et cetera. But obviously, today you would have to decide more conservatively than you would in two or ten years or 100 years, and you would have to pick out what level of conservatism that gives you enough sort of confidence that it will not be exceeded in ten years and 100 years, et cetera. So, in fact, you conduct--the fact that you have less information with a higher level of conservatism, and I believe Abe in fact emphasized that in the face of a larger uncertainty, you have to be more conservative.

The only thing I was adding is that maybe one can structure that. One can make sort of some decision model in which the acquisition of information comes in explicitly, and although these models will be necessarily simplified, et cetera, but at least they will be--they will make explicit this added conservatism that one is using because we are in a state of large uncertainty.

COHON: Joe, and then Priscilla.

HOLONICH: Yeah, I hate to do this to you, Dr. Cohon, but I've got to clarify something on the graph.

COHON: We already burned ours.

HOLONICH: When I put the dot on the graph, I didn't say, nor did I imply--mean to imply that the risk from the repository was greater than the risk from spent fuel storage. In fact, if you look at the graph, the risk is the X axis, and the repository and the spent fuel storage both sit in the general risk area of a medium hazard.

What I was saying was because of the lack of data in terms of operational experience and in terms of the site knowledge that we've got, there was more need for defense-in-depth in a repository than there was in spent fuel storage. That's not to say that the risks are greater. The risks are both medium hazard in terms of the types of facilities we regulate. It's just that because we have less data in terms of operations of a repository versus the number of spent fuel storage facilities we've got out there, we have greater knowledge and, therefore, can understand better how much defense-in-depth we need. That's what I was trying to say. Not that there was a greater risk at a repository.

COHON: Priscilla, then Budhi, then Alberto.

NELSON: Nelson, Board. I hope this isn't too ignorant overall, but I've got a couple of questions dealing with two observations. One, you gave a plot, Daniele, about where you showed total uncertainty through time, and showed a rising curve, or plot, that separated a domain of unexplained from a domain of explained, and implying, the way the plot was, that it was a closed system with a fixed amount of uncertainty.

One point that the Board has made and I think observed in some cases is that as more information comes in, sometimes the uncertainty increases. And in such a case, what to do in terms of trying to kind of bound, constrain the acquisition of new information, and understand the uncertainty that's evolving.

And I also think from the standpoint of PA, as much as I understand it, there are some components of PA that are done in a full probabilistic framework where the uncertainty is assessed, and there's some places where perhaps there's a bounding, almost single point or deterministic component in some cases of it. And so we have a very complex model where we've got cases where some of those bounding models could actually be made to be probabilistic if it was chosen. So to what extent do we understand the uncertainty around what might be an expected value, or a mean calculation?

After this discussion, I'm not knowing what to do about new information and growing uncertainty, and I'm not sure that the PA represents the full uncertainty around what might be an expected value. So two linked observations.

VENEZIANO: First of all, let me correct two--let's say in those sketches that I presented, one of which is the one that you have picked up. That is correct, that in fact in making them, I was debating whether I should be more realistic, or present the picture as more idealized, and I opted for the latter.

You are correct, what that picture shows is, let's say, an expected behavior over time. And certainly in expected value sense, your uncertainty will be reduced over time in an expected value sense. In reality, there will be random fluctuations, et cetera, et cetera. So you may want to add some wiggling to my straight lines there.

What is important, however, is not so much the reduction of uncertainty, but the fluctuations in the mean value, which I tried to show are stochastic in nature and not predictable. What you may be able to reasonably predict, I'm not sure how much, is how much those fluctuations will be in terms of something like variance, so whether they will be large or small, whether on a certain issue you are expecting to acquire significant information so that you will be able to resolve that certain parameter, the seepage, et cetera.

And the other unrealistic aspect which has been I believe noticed by some other speaker there in that figure, is that the regulatory constraints are portrayed as fixed over time, and actually that's not a source of uncertainty, but over time, the regulatory limits may very well fluctuate. And, indeed, that was another simplifying decision that I made. I said let's not present also these acceptable limits as possibly evolving over time, as our society will sort of be more or less accepting risk. That should also actually be represented as possibly fluctuating over time, and will be another consideration to be conservative whenever one makes a decision that has to last over a long period of time.

NELSON: Can I just ask Abe to talk about PA?

COHON: Yes. Sure. Go ahead, Abe.

VAN LUIK: Yes, this is Abe van Luik. You hit on a point that, you know, one of the amazing things about total system performance assessment, it takes us a couple of months to do the assessment, and then about two to three times as long to do all these sensitivity cases and the uncertainty analyses, because they're so complicated.

When we put in a bounding value, it is going to be our burden to show that, one, that value is bounding, a reasonable bound, and we have to do that through ancillary arguments, and we have to show that either the value is not going to significantly perturb the dose, which is our final performance measure, if it were, you know, less than that bound. Or we have to make a case, or some other case, and if in our sensitivity cases we show that by varying that, you know, going lower than the bound that we picked, that we do perturb the dose, then we have to rerun the whole thing and do it right.

So you've hit on the crux of a very difficult problem and one of the reasons it takes so darned long to get these PAs right, because often we do find things that we have to go back and fix because of the sensitivity studies.

Now, another thing is that we are calculating, and this is a little bit further from the subject than perhaps the Chairman would like, we are calculating dose as a surrogate for risk, and I think something that Judy was hinting at and several others have hinted at is that risk is perceived differently by different people. And we look at the societal decision process, I'm very comfortable with looking at a risk number or dose and saying this is acceptable to me, and this is not acceptable to me.

Society as a whole has a lot of other baskets in the air that it's trying to weigh, value systems from different organizations when people come in, and I think, you know, Congress has a very different value system when it comes to this. They're looking at issues that probably a performance assessment person would never even think of, such as, you know, how long does this funding have to continue, et cetera, the kind of thing that Mal was hinting at.

So I think when we're looking at the risk basis, which is what we're focused on within the Yucca Mountain project, and when we make a recommendation to the Secretary, it will be to say we are confident that this risk meets the guidelines set forth by the regulator. The point is that the regulator is the guardian of society's safety and health in this whole structure.

Once it goes beyond the regulator to Congress and the President for final determinations, many other values will come into the equation, just like not all uncertainty is captured in the performance assessment. Those are some of the other values that have to be worked in. There's nothing simple about this process. And just because we come in with the right number doesn't guarantee the success of Yucca Mountain in becoming a repository, I guess is what it boils down to.

COHON: Budhi?

SAGAR: Budhi Sagar, CNWRA. I just wanted to come back to one of the questions that had been raised by several speakers here, what is the acceptable level of uncertainty has been asked several times. There is obviously no easy answer. The easiest answer to that question in my mind, and this is just free talk at this point, is that if I was comparing two designs, for example, or if I was comparing to sites, the answer is much easier because the one design or one site which has smaller uncertainty is preferable.

But if I have a single site, or a single design eventually, and I'm doing a performance assessment, what level of uncertainty is acceptable, how long should I wait and collect more data, until the uncertainty is reduced. I think the same way we make other acknowledged decisions, you can allocate a value to the reduction in uncertainty, and there comes a time when the marginal value of the reduction of uncertainty reduces as the uncertainty comes down.

And it's at that point you make a decision saying okay, delaying the schedule or spending more money or resources in trying to collect extra data does not give me a benefit in terms of reduction in uncertainty which is equal to or greater than the resources you are spending. And that's where you say this is the uncertainty under which I have to make a decision.

I mean, in the decision framework, in a logical framework, I think that's the one way you might try to decide what level of uncertainty is acceptable and when to go ahead for the next step.

COHON: Alberto and Leon, Joe and Steve. Alberto?

SAGÜÉS: Actually, what I was going to ask was touched upon indirectly a little bit already, but I might as well say, and that is that it's interesting that the uncertainty analysis and how much uncertainty--that issue seems to have been divorced from this discussion pretty much on one quantitative factor that may be determined, and that is what is the population of Nye County would increase by, say, two orders of magnitude, and we get into, you know, a seven figure kind of population. Will that change the way in which the analysis is made and the way in which the criteria are applied? And I guess that since I'm looking at Abe, I'd like to ask his opinion about that.

VAN LUIK: This is another reason why we look to the regulator for guidance on this issue. They need to define for us a biosphere that we can calculate these doses to, because to try to predict the future population of that area is not something that we want to get involved in defending, you know, in a licensing area.

At the same time, I think that the way that they are defining it will work no matter what the population is, because they're saying look at the critical group, look at an average member of the critical group with this particular lifestyle. The more people you pump into an area, the less likely it is they're going to grow their own vegetables, and that's a very large, you know, being a vegetarian I know, it's a very large contributor to your dose, and the less likely it is that they'll be pumping their own wells, and the less likely it is that they will not have a water purification system.

So we think that the NRC approach, and even the EPA approach, properly applied is a conservative way to go about judging a reasonable but cautious risk level that will apply to future populations in that area.

COHON: Judy, did you want to speak just to this point?

TREICHEL: No.

COHON: Okay. We'll come back to you then. Leo Reiter?

REITER: Leon Reiter, Staff. It's interesting to note that people are talking about the need to, or it would be nice to estimate how our estimates would change with time as we get more knowledge. I'd like to point out that 10 CFR 960, which I gather is the operative site suitability guidelines for all other repositories except Yucca Mountain, includes such a criteria in that. Although your calculations may show the site can meet the criteria, before you determine whether it's suitable or not, you have to be able to show with a high degree of confidence that future knowledge won't change that. But, of course, that's not for Yucca Mountain.

I have another point that I wanted to make, and Warner said this about risk analysis is best used to develop insights and not to develop results that might mistakenly be considered to be highly precise, and he quote Bob Bernero. And, in fact, I have not met anybody who works in, analyst, who hasn't repeated that same thought. It's such a powerful thought.

But on the other hand, when dealing with regulatory bodies, once you create a quantitative criteria, at least my observations in the past, those criteria take on a life of their own, and those numbers, the quantitative criteria tend to dominate anything else. So even though we may say we're interested primarily for insights, very often what gets used is just the numbers themselves. Is there any way to prevent that?

COHON: Warner, do you want to speak to that?

NORTH: Please. I think the path out of that problem, which I certainly would acknowledge occurs a lot, is good public discussion and transparency for the analysis.

To the extent that more people can understand what those numbers mean and where they come from, I think the dialogue can be improved.

If we are able to use the analysis to conclude that the crucial issues have to do with juvenile failures and seepage as opposed to a lot of other things, that may be a big step forward.

I would hope that as this issue moves toward a decision, it is not going to be a go by the numbers, 24.9 is acceptable and 26.1 is not. I think that would be a horrible failure in the process. And I acknowledge that occasionally things like that have happened. I really doubt it's going to happen here, because I think there's already too much dialogue and too much discussion to allow a decision to be made narrowly by the numbers. I just don't think it's going to happen.

COHON: Well, I'm not as confident as you, Warner. It seems to me that Leon's observation, with which you readily agree, is that the more complicated the problem is, the more weight we put on the number. And it's very easy to imagine a scenario where we've got the number and we've got the volume that explains uncertainty, or the characterization of uncertainty, and you could see someone, a stakeholder saying well, no, I see--of seepage. Doesn't that disqualify the site? Well, no, because this is one of a thousand items that go into that number. That's why I keep holding back on another number, which is an estimate of uncertainty.

Well, I'll leave it at that. Sorry to intervene.

HOLONICH: Joe Holonich with the NRC. I just want to comment on two things that Abe said. One, he talked about the safety of the repository resting with the regulator. And whenever I give a presentation on the NRC licensing process, I always start my presentation with a quote from the NRC's Information Digest, and that quote says basically NRC's regulations and requirements are an integral part of ensuring public health and safety. But the burden of safe operation of any nuclear facility rests with the licensee at that facility. So the safety of the repository is DOE's responsibility. NRC helps to oversee that, but the organization involved with the safety is DOE.

And if you carry that process out, the first organization that needs to determine if NRC's requirements are met is DOE before it submits an application. It should make the conclusion that the requirements are met before it provides us with the application. So the safety doesn't rest with NRC. The safety of the facility rests with DOE.

Now, the second thing I wanted to do was kind of amplify a little bit what Abe said. The Commission, in a statement of considerations for Part 63, did note that one of the things they wanted to do was use a critical group, and they believed if you used a critical group, that you would get the worst case dose scenarios that you could expect, and that a farming scenario was the worst case in terms of doses because you're going to be ingesting it, you're going to be pulling more contaminated water out of the aquifer. You're going to be getting it, breathing it, and direct exposure.

So the Commission said what we think the best way to look at it in terms of doing the performance assessment is using a critical group, and as Abe said, when you focus on that critical group, that's going to stay there no matter how big the population grows, and the Commission's view is that a farming critical group is probably the worst group in terms of the dose. So I just wanted to kind of amplify a little bit on what Abe answered when he answered Alberto's question.

COHON: There's a distinction, and I think it's an explicit one by EPA, that the non-critical group is getting a dose of zero; right? Everybody else is getting a dose of zero. The question was-- if the dose to the non-critical group is not zero, then the total risk to everybody is higher than the critical group. That must be true; right? Abe, talk into the mike.

VAN LUIK: The critical group definition is that it is those highest exposed, down to--from the highest in order of magnitude, down. So by definition, if you're outside the critical group, you're an order of magnitude below the highest in the critical group. And so there may be some dose, and it may be to a larger population, but it's going to be very minuscule compared to the critical group. And I think that's the philosophy, if you protect the critical group, if they are actually protected, everyone else is protected, too. Although if you do some gymnastics with population dose calculations, you could probably scare somebody if you wanted to.

COHON: So the answer is that those outside the critical group will get a dose small enough so that any reasonable population cannot be big enough to make the risk to the overall population larger than the risk to the critical group? Go ahead. Dan Bullen?

BULLEN: Bullen, Board. And actually this was a note given to me by a member of the public who came from behind, but it voices my opinion because we're talking about conservatisms here, and we're talking about estimation of conservatisms. The assumptions that we make when we set the regulations, whether it be 15 or 25 millirems, basically are predicated on the fact that there's a linear, no threshold kind of dose assessment. That's conservative.

The other problem is that there is a background dose that everybody gets of 300 to 400 millirems per year. So it's not like they're getting zero. They're getting 300 or they're getting 325. And the question is is the additional risk that's associated with it, whether it's their choice or not to get that dose, is the additional 25 millirems acceptable or unacceptable.

And so these are the kinds of issues that are brought up, you know, in effect it's not even the regulator that has to make the decision, it's Congress that has to say yes, the site is suitable. So Congress is going to say that indeed, whatever the EPA or the NRC regulations are, are acceptable risks to the public if you can meet those criteria.

And so you have to take a look at that in the broader scheme of things, and that's the one thing that when I teach my class in radioactive waste management, people don't understand that you're already getting irradiated.

Okay? And so it's not like you get zero. And if the dose that gives you 15 millirems is two additional cancer deaths per 100,000 people per year, then we decide whether or not that's acceptable. But I guess you don't want to say that there's never a zero risk, and that's the thing that always bothers me when I try to teach this to students, is that there's always a risk, and is the additional risk acceptable.

So the conservatism is already built in, and it's up to the regulator--it's not even up to the regulator--it's up to Congress to decide to tell the regulator that that's the way that they want it to be.

COHON: Steve?

FRISHMAN: I think DOE, NRC and EPA have all said at one time or another that the important part of the rationale for a regulatory period of only 10,000 years rather than out to peak dose, whenever that might be, is that beyond 10,000 years, the uncertainties begin to overwhelm.

Now, given this supposed new non-boiling approach, relative to Yucca Mountain, is that true? It looks to me from some of the performance curves, that once you start getting near peak dose, the uncertainties, regardless of when that is, even if it's inside 10,000 years, the range of uncertainty looks about the same for as far out as you go. And I guess I'd like to just raise that question, because I'm always looking for ways to attack the 10,000 years.

COHON: Abe will be happy to help you.

VAN LUIK: This is Abe van Luik. Perhaps what we are doing when we focus on those types of issues is getting mesmerized by the quantified uncertainties in a performance assessment, rather than seeing that the larger envelope of uncertainties, including those not addressed, which is the future states of geology and just the future state of the system, which are not that well quantified into a performance assessment, are not reflected in those calculations.

And so one of the limitations of performance assessment is that the farther you go away from what you know, the less certain you are of the future of that system. And since you are sufficiently uncertain that you can't specifically model it, you can guess at it, but you can't specifically model it, that's the type of uncertainty that drives us to distraction beyond 10,000 years, not that the values of peak dose are not useful in giving us just a general indicator of the type of risk that could be possible, it is not a projection of certainty, though. And there's a lot of uncertainty in those calculations that's not reflected in the width of the horse tail.

COHON: Judy, then Dan Bullen, and then Mal. Judy?

TREICHEL: As we've been talking about uncertainty, it occurred to me that the first time we started really intensely talking about it was when we were in scoping for the EIS, which has now ground its way all the way down to the hearings and the draft. And one of the things that people were saying, and I was one of them, was that this project is not EIS-able, and it always sounded like it was a little quip or a little joke, but it's really quite true.

When you look at the reason that you do an EIS, and what a project that gets acceptance and the decision is made to do it, you know, part of NEPA is that a project is supposed to either preserve, restore or enhance, and it's very difficult to see as a Nevadan how this one does that, but the levels of uncertainty make it so difficult to determine what is being built, and I think that's why we've been having a real battle with the EIS and everything else that's gone on. And one of the first questions that was posed to Dr. Itkin when we first met him, or one of the things we said was you are constantly going to be asked a question, and we'll ask it to you as well, because we'd like to know what is it that would make you say no to this project.

Because as you listen to people around the table, whether it's DOE or NRC or whoever it is, it's always kind of moving toward yes. We may have to do that. We may have to mitigate this. We may have to reduce uncertainty that much. But you never--and I'm just talking about the psychology of the thing for the benefit of people who oppose it, and it's always how would you get to yes. And one of the arguments that comes in, as Dan was saying, you know, everybody is getting nuked. There's sewage in the water. There's this, there's that. Well, if you're looking at the EIS and if you're used to dealing with that horrible monster a lot, that's cumulative dose.

If you've got dangerous trucks on the road, why would you then include trucks carrying high-level radioactive waste. If you smoke or you decide to hang out in smoky places so you have a health effect, why would you take on another one. So this is in addition to. None of those go away because you've agreed to take radiation. It becomes worse, and we're dealing with a lot of the cumulative stuff with the people in Nye County and Lincoln County and others.

COHON: Judy, you've I think crystallized the suitability decision. There is a value judgment that is going to be made by the Secretary, the President and the Congress between the mean and the various. Let me sort of do it shorthand that way. And that's why I personally, and this Board collectively, has focused so much on quantification of uncertainty. Because if it's a number versus a book, there's no basis to make that value judgment. And there is a value judgment to be made.

I left unsaid, but I'll say it now, is that it seems clear--this is one person's opinion, not the Board--

that it's highly unlikely that the program is going to discover a show-stopper, as it's been called, between now and their site recommendation. So the real question will be this trade-off between uncertainty and mean performance, I think. And that, you know, from an ideal view of our decision making political system, that's where the decision belongs. People that we elected ought to be making this trade-off.

TREICHEL: But that's why it's so lousy that people can't say no. They can't be like John Madden and not jump on the plane.

COHON: Okay. Well, here--

TREICHEL: When you don't have a place where you can say this is what you need to get to if you don't want the project, this is what you'd have to show, and there's nothing out there, it's always going to be fixed.

COHON: Yeah. I think that's in the nature of this problem, though. I mean, this is not a kind of problem where you can, I think, draw a very bright line and say, well, if you're over it, it's done, and if you're below it, it's okay.

TREICHEL: But, you see, when we started out with this, that's exactly what we were told. If groundwater moves 999 years, the thing is gone. And there were all of these absolute marks that had to be made, and they have all disappeared, and with the deal with the guidelines, which I think is criminal, but at any rate, the rest of them go out the window. And so it was deceit, and it's very frustrating.

COHON: Well, just to--I'm going to get the last word on this, Judy. Just to bring this to closure, the Board supports, overall supports the philosophy in the change in the guidelines, and that's because when you're talking about a big complicated system, to decide whether it's going to work or not based on sub-system requirements really is a flawed approach, in my view. And that's why the Board supports the philosophy.

Still, I mean, your point about having a way to say no, a basis for saying no, is very important, and it's only going to come if there's some clear quantification of what the trade-off is, and getting the people that have the power to make that decision, and should be making that decision, focused on it and understanding it.

Dan Bullen?

BULLEN: Bullen, Board. We learned this morning, or early this afternoon, about the reduction of Type 2 uncertainty because of ignorance if we gain more knowledge. And Steve Frishman brought up a point that actually maybe the Board has been responsible for exacerbating. If we do indeed want a cooler repository design and keep it open for a long time, then the confirmatory testing phase isn't going to test post-closure performance.

And so I guess both the question to the NRC and to the DOE is what do you envision the confirmatory testing phase to tell you, and how are you going to use that information in either improving your confidence that the reduction in uncertainty is real, or in deciding to do something else? So maybe I'll ask Joe first, because he's the first person, and then I'll ask maybe Russ to follow up on that.

HOLONICH: I think the NRC's vision is that DOE is to continue to monitor the site and collect data during the operations period. the expectation is the more data you get, the better you can see how well you've predicted what the repository is supposed to do.

What would happen with that data is if the repository is showing that it's not performing the way it was analyzed, the NRC then has that balance in there of being able to remove the waste, because that's obviously, the repository is not working the way you expected it to work.

Other things that can be done with that data is DOE collects that data, it might find that in fact it had a high level of conservatism in its design, and that, just a hypothetical or arbitrary example, you know, 50 feet between canisters is what was needed for a cool repository, now that data is showing that the repository is less conservative in terms of its performance, DOE could come back in with an amendment to us and say 20 feet between waste packages is all we need to operate a cool repository, and we would have to take a look on that and determine whether that was acceptable.

So there's two ways you could use the data. Number one, NRC is looking, from our perspective as a regulator, to continue to monitor that site, to collect data to make sure that the way it's performing is the way it was analyzed. DOE could use that data from our regulatory view to change its design to make it less conservative based on the data it's collecting.

BULLEN: Russ, before you jump in, this is Bullen, Board, I guess just a quick question is did you expect to see data that would be post-closure performance confirmation data, though? I mean, the kind of data you're talking about is operational data and you expect to see with a ventilated repository, these kinds of things, but you're not going to see that unventilated, this is a closed repository kind of data unless, of course, you allow them in some license modification to close off a couple drifts and look at that and say that's never going to be a sealed drift, you're going to do the experiments. I mean, would you expect to see those kinds of experiments, and is that the kind of question you're going to ask DOE when it comes with a license application?

HOLONICH: We are now. It is a very good question, and DOE always has the flexibility to come in to us and say we want to backfill these drifts for operational reasons or for performance confirmation reasons. As they submit their application, they may look at and say in our performance confirmation program, we plan to backfill drifts on this schedule, and continue to collect data so that we can see what a backfilled drift looks like, how it performs, how the heat transfer is behaving in those drifts. So that's part of what, yes, we'll be looking for in our review of the performance confirmation.

Our objective is DOE collect the data, to continue to show us that it's performing the way you analyzed. It's up to them to tell us how they're going to put that program together, including whether they would be backfilling drifts to show more closed or final repository conditions.

COHON: Russ, you wanted to comment on this?

DYER: Dyer, DOE. Dan, we started thinking about this a while ago when we were trying to decide what to do with the drift scale test. Do we want to continue it at the current essentially upper limit of thermal range kind of approach, or do wee want to say turn the rheostats down, lower the temperature of it, and make it something that was more reflective of the latest design approach, and we chose to leave it with the original design.

Now, one of the things that could be done in the future is to have other test facilities that look at various thermal envelopes. You could also, just like Joe said, as part of a performance confirmation program, and one thing we've talked about is to dedicate one or more drifts to look at some variance around your base case. And if you have decades of information that you can acquire, you've still got some period of time where you might want to change something later on, come in with an amendment for some better way of dealing with the repository.

COHON: Mal?

MURPHY: Mal Murphy, Nye County. I wanted to add one small point to what Abe was saying in the discussion about 10,000 versus 100,000 years, and that is that it's always been my understanding at least that one of the express reasons for not--by the regulating entities for choosing a 10,000 year regulatory period versus 100 or 200 or a million was not only geologic uncertainty, but uncertainty in defining that future biosphere, that it may be even more difficult to figure out how people are going to live in 10,000 years than whether or not the fault is going to let go in 10,000 years.

So, you know, one small point is that if there were some way to deal with that biosphere uncertainty issue, then it would become easier and less uncertain to have a 100,000 year regulatory period, for example, rather than 10,000.

On this latest point that people were discussing, you know, I guess we feel some sort of a proprietary, you know, that this is Nye County's property sort of, since it was our work and our encouragement which has prompted DOE to move toward a more ventilated repository, and consistent with the overall discussion this afternoon, it seems to me, my own personal view would be, it seems to me that it would be always preferable to choose to begin with a "safer" repository, even though one of the trade-offs for that would be less opportunities to provide post-closure performance confirmation than to begin with a repository design which produces greater degrees of uncertainty with respect to thermal effects, but allows you to do more post-closure studying.

We would, Nye County, or at least I think the Nye County position is that one of the reasons for moving toward a ventilated repository is to reduce uncertainties associated with thermal effects, and to, just as importantly, or more importantly, and to reduce the uncertainties with respect to cask degradation because you will keep the seepage away from the cask through the ventilation.

So even though that may cause some difficulties in post-closure performance, it seems to me the reduction in uncertainty on the other side would always be preferable from our point of view, at least, to going with higher uncertainty, but more ability to do post-closure performance confirmation.

COHON: Rod, did you have your hand up before? Rod, then Budhi, then Joe.

MC CULLUM: McCullum, Panel. I don't know if this remains relevant, but I wanted to address a couple points that are in all of this, and that being the subject of voluntary risk and the human factors, and coupling of risk and uncertainty.

Getting back to the point about the glass of water, I don't agree with Mal that it's a voluntary decision for me to drink this glass of water. Perhaps this glass it was, but if I don't drink water within the next several days, then it ceases to become voluntary anymore.

MURPHY: But you can go out to the gift shop and buy a bottle of water.

MC CULLUM: Sure. But how do I know where that's been? And that gets back to the point that this is an involuntary risk, and I think the airplane example, and I'm agreeing, was a good illustration of defense-in-depth. We do have to recognize that this is an involuntary risk, and that there are a lot of these in society, and we rely on our political decision making processes to assure us that these are taken care of so that we do not have to think about this glass of water, or the bottle in the gift shop, or the air we breathe, and these things do protect us.

In terms of perhaps human factors, that may indeed be the greatest uncertainty of all. I would agree with that and once again would point to that's why we have the process wee do, to allow us to uncover those things that the humans were wrong about.

There's a display in the back of the room there that has a "what if" button on it, and you can turn up the flow rate here, or you can turn down the absorption there if you want, and you can ask those questions, and I think it's important to ask them now, for the decision makers to ask them now, and for those answers to be considered on both sides. The "what if" questions have to be clearly defined to the decision makers so that they can lay all these things and assure that the levels of risk are acceptable, which gets down to the last point about the coupling of risk in uncertainty.

You know, we talk about taking these things apart as if they're separate, but they're not. It's because of the uncertainties that we have a linear no threshold dose model that Dr. Bullen talked about before, that we take that level of conservatism. It's because of the uncertainties, we're talking about 15 versus 25 millirems, and even smaller fractions of that if you look at the latest performance assessments, and we're actually debating the significance of those levels because of what we don't know. We are using lower and lower risk levels, far below anything any health effect has ever been shown, and we're debating those things because we know those uncertainties are out there. And we need to look at those uncertainties in that context, know what they are, know how they--you know, press those "what if" buttons and recognize that whatever generation of humans makes this decision, and I would hope it would be this one that would have the courage to do it, whatever that decision may be, that we do that on the best of today's knowledge, and we put in place the measures that if the humans were wrong, we have a period of time that we can compensate for that wrong, or at least confirm that we're still okay.

COHON: Thank you. Budhi, then Joe, and then I've got a couple of specific questions for our consultants, and then we're going to wrap up.

SAGAR: Budhi Sagar, CNWRA. My comment relates to performance confirmation, and Dan Bullen's comment on it. As I spend more time in this project, I find that the use of terminology and words is extremely important in this project. And I think performance confirmation perhaps can weigh an idea that by the time the repository is closed, the post-closure performance, 10,000 years, would be confirmed, and confirmed by some certainty attached to it.

Perhaps there's a wrong use of this word here. I think we do not--realistically, we do not expect waste packages to fail and flow and transport to occur. I think what we realistically expect is that there would be large scale controlled experiments simulating the repository conditions during the post-closure phase, and that we would be able to look at the rates of processes, the geochemical changes, the thermohydrology, the thermal mechanical processes, and so on. We would still have to extrapolate those to say yes, at the post-closure time, the expected performance for the next 10,000 years would be X, but I don't think the observations would directly lead you to make that conclusion. So I don't know if the use of the term is faulty here, or what people understand what is being said in that context.

COHON: Go ahead.

BULLEN: Bullen, Board. Actually, you're right, and we could argue semantics on whether or not it's actually confirmation or not. I guess the concern the Board has always raised is that this science always continues, and so we always want to make sure that you've got your eye on the ball long distances from here so that you can actually make sure that those kinds of things, even when you start emplacing waste, if in deed you get a license from the NRC to construct and operate, before you get the license to close, you're still going to have those kinds of scientific experiments going on, whether they be drift scale tests like

Russ talked about, of if they're just a bench scale test or anything else that provide you with a better understanding.

I mean, my guess is in 125 years, as computing power advances, Abe van Luik's great grandchildren are going to be able to tell us where every molecule goes, and so it might not be a problem. But I guess the key there is that we want to make sure that that same type of scientific undertaking is continued throughout the program, rather than just saying oh, now it's a construction project and we just have to finish it. We want to make sure that you keep taking the data.

COHON: Joe?

HOLONICH: Just two things. Number one, I went back and I looked at the Commission's requirements for performance confirmation, and at least in one paragraph for the waste package, it says consistent with safe operations at the repository. The environment of the waste package selected for waste package in the monitoring program shall be representative of the environment in which the waste will be emplaced. So I would interpret that to me that you need to look at it in terms of how the waste is supposed to sit over the designed life of the repository, over the 10,000 years.

Speaking of the 10,000 years, I wanted to kind of recite for folks the Commission's reasoning for why it chose 10,000 years, because it did lay out in the statements of consideration for the draft regulation the three reasons. The first was that if you look at the decay of the waste, the waste, by 10,000 years, decays, 99 per cent of it decays away in terms of short-term hazards, and what's left gives you a hazard that's equivalent to about .2 per cent uranium ore body. So the first reason was you get rid of the nasty stuff, and you're back to really what an ore body would be in the earth.

The second reason was that period gives you the ability to look at different geologic conditions and how they're going to impact the repository's performance. And then third was a policy consistency within the government. EPA had picked 10,000 years, and we were looking to pick the same performance period. And the Commission lays out in more detail why it picked those, and those reasons, but it does lay out those three reasons for the 10,000 year performance period.

COHON: And by implication then, you also reject the rationale that the National Research Council panel offered? Peak dose?

HOLONICH: Yeah, the Commission does discuss that also in the statement of considerations, and it says it thinks 10,000 years is the appropriate period.

COHON: Very diplomatic. Two questions for our consultants. In Abe van Luik's presentation, he showed one example of some sensitivity studies they do where they choose a barrier and sort of make it disappear, and in that way, get a sense of its contribution to performance. So, for example, he gave an example the waste package is there, but you assume it's completely porous and all water goes right through it.

Any comments on that approach as a method in general terms as a way to get a handle on uncertainty? I don't know if you've seen it before or you care to comment on it.

NORTH: Warner North. I'll take a shot at that as follows. I think "what if" questions are very useful. If they're not so realistic, maybe they're less useful. And I'm not sure I'm close enough to be able to judge whether some of the scenarios shown were good "what if" questions. I would encourage more of that rather than less. So I don't want to discourage any particular case.

COHON: Daniele?

VENEZIANO: The way I understood it is that these sensitivity analyses were not means for evaluating uncertainty, not at least in a quantitative sense. I may be wrong. Maybe they would be of support to an assessment of the known quantitative uncertainties, or the other uncertainties. But I didn't have the sense that these analyses were aimed at quantifying uncertainties, but rather to show the importance of different components of the system.

In that sense, I think they are very important because they would show where you should focus your attention to sharpen your estimates, or to better assess your uncertainties, to ask more "what if" questions, and so on. So I think in terms of an exploratory value, they are very important.

COHON: Thank you. My second question had to do with the notion of surprise. Is that a qualitatively different thing from uncertainty the way you discussed it, Type II uncertainty, for example? Unknown unknowns, as Abe characterized it. Or is that just another word or phrase for the same thing you were talking about?

NORTH: Warner North. I think we've been talking about this issue for a long, long time. There are lots of risky endeavors that have been undertaken by human beings I think going back to the beginning of recorded history I think of what was done exploring the new world, and so forth.

I think we always have surprises with us, and we always have to anticipate that new knowledge may invalidate even areas where we feel we really understand it. We have to make decisions in the present based on the knowledge we have available in the present. And it seems to me what you probably need to do to deal with surprises is be as creative as you can about what might possibly happen, where might we be wrong. Don't assume that conventional wisdom is right.

It might not be. And involve a large number of skeptics in the process who might ask good questions, where might you be surprised. You know, human nature being one example, let's not rule that out. Let's not rule out that somebody might make mistakes, that standards for constructing the repository might not be adhered to, given the human nature of construction workers, and so on down a long list.

I think we need to be realistic, and skepticism can be extremely valuable. I think one only need look at 19th century science at the number of things that leading scientists declared to be impossible that have become common place in the last century, that is, the 20th, to have a great deal of skepticism on how accurately scientists can foretell the future.

But on the other hand, I think we can't be paralyzed by the specter that we don't understand everything perfectly. We're simply going to have to make decisions in the face of uncertainty, and unknown unknowns, or surprises, are a part of that uncertainty that we really can't avoid.

COHON: Thank you, Warner. Daniele, do you want to add anything?

VENEZIANO: Well, I largely agree, and it seems to me that we have to be truthful to our knowledge and uncertainty, and I think that if we believe too much--or give too much weight to the unknown unknowns, we end up being totally paralyzed, and probably including hypothesis that would be 99 per cent of the time wrong. So I do not believe in giving too much weight to these unknowns, except for thinking as hard as we can about the way the truth might possibly be. I believe that's all we can do.

COHON: Two very good closing comments. Please join me in thanking our panel for an excellent session.

As we turn now to our public comment session, with apologies to two members of the State Legislature, I learned just recently that they were with us today. Are they still here? I'd like to acknowledge them. Bob Price, member of the Assembly, are you here? In the back. Thank you very much for being here. We appreciate it.

Also, is Lawrence Jacobson still here? Please stand. Thank you. Lawrence Jacobson is a senator and in fact is President Pro Tem of the State Senate. Thank you, Gentlemen, for being with us today. We really are pleased by your presence.

We have five people who have signed up to speak. Let me just read their names, and if you wanted to speak and your name isn't on the list, please raise your hand so we know someone else wants to speak.

We have Tom McGowan, Tricia McCraken, John Davies, Sally Devlin and Earle Dixon. Did I miss anybody?

Yes, sir. Tom McGowan will go first, and let's--

Mr. McGowan, let's try to keep it to five minutes, if we can.

MC GOWAN: Mr. Chairman, I would request, with your indulgence permission to go last.

COHON: Yes, sir.

MC GOWAN: Thank you very much. I'd defer to the other speakers.

COHON: Patricia McCraken, please come forward to a mike and we'll be happy to hear from you.

Please state your name again in case I messed it up.

MC CRACKEN: I'm Patricia McCraken. I'm from Augusta, Georgia, around the Savannah River site, and I appreciate the opportunity to observe your meeting, and I look forward to giving more public comment on the environmental assessments, learning more about the Nuclear Waste Fund, and as you know, we have nuclear power in our part of the world. I hope to continue seeing your meeting tomorrow.

Thank you.

COHON: Thank you. You failed to invite us to Aiken. That would be a nice place for a meeting actually. No one's agreeing with me. How about Augusta instead?

Dr. John Davies, University of Colorado.

DAVIES: Thank you, Mr. Chairman. My name is Dr. John Davies. I'm the lead author with Professor Archibald on two published papers on hydrological models that fit all the data, but are unfavorable to DOE and USGS positions.

We had a lot of trouble getting these published in the U.S. because of, shall we say, the old boy network. But, however, after presenting them at the IUGG, one paper was invited for publication in the proceedings in Tectonic Physics, and Environmental Geology, a German publication, snapped up the other.

Now, Director Itkin has said that the best available science should be considered. Best is subjective. Available, you can cut that out quite easily by stopping publication. Best is subjective, and as every geologist here knows, one geologist can pick up a rock and tell you it's something, and another geologist will pick it up and tell you it's something else. But usually they're both right, it is a rock.

However, I'd like to ask in terms that uncertainty is lack of information, and that it's dependent on the operating physical processes that are considered in these models. The question is why hasn't the Board requested myself and other fellow independent scientists who have unfavorable models, why haven't we been invited to appear before them? Question, why is this Board, through its staff, hiring USGS related scientists to insult and defame these scientists and their work? And question, isn't this restriction of exposure to alternative models producing uncertainty in the validity of any assessment by this Board?

Thank you.

COHON: Thank you. We'll look into your charges.

Ms. Devlin?

DEVLIN: Thank you, Dr. Cohon, and welcome to Nevada, as always, and members of the Board and staff and everybody here, and I hope there's a lot more public tomorrow.

My name is Sally Devlin. I'm from Pahrump, Nye County, and that's why I've been coming to these meetings for over six and a half years, and I came today for two things.

The first is the map with the two railroad tracks through Pahrump. The first one we knew on the Von Schmidt; the second one we never saw until one week before the EIS meeting, and I wanted all the documentation on this second railroad plan. It is in a worse flood plain than the Von Schmidt line, so I'm asking you formally, I want to know when this was done, how this was done, and where it was done, and how it was done. It was a big shocker to get this.

The second thing I'd like to say is that there was no mention, and when you talk about uncertainty, one of my-- over the year has been, which was announced from the Congressional Report three and a half years ago when we met at the Paradise Holiday, and that was that Ronald Reagan in '87 gave DOD the right to put 10 per cent of their classified waste in Yucca Mountain.

And as I have stated time and time again, you cannot put classified waste in my mountain, and I read the NRC report, how they're going to handle it for licensing. It is totally unacceptable, and I want to know more about this DOD waste. You talk about uncertainty. It probably belongs to DOE. I don't know which hand washes the other one.

But the public must know what the DOE is. It cannot be licensed to go in the mountain, and classified waste has no place in my mountain. And that includes 700 degree C. fissile fuel, which in their report that they sent me, and I read all 16 pounds, mentions this, that they want to put the fissile fuel from Russia in the mountain at 700 degrees C. It's terrifying.

But the third thing that I came for was to tell you a joke. And as you know, after every meeting, we always do a Shaggy Dog story, so I thought I'd tell the whole group a cute Shaggy Dog story I heard the other day. And that is Clancy loved to nip a little bit, and he was a good Irishman, and he's driving down the street and he sees this new bar going up and it's called Finnigan's, and he drives back and forth for many, many months, and finally he sees the sign where Finnigan's is going to open. And the parking lot is filling up and all kinds of people are there, and he gets out of the car, out of his truck, and he sees the bouncer, and the bouncer says, "I'm sorry, Clancy, you can't come in here." And Clancy says, "Why not?" And he says, "Because you don't have a tie." So he says, "Oh, my goodness, I'll go back to the truck and I'll get me a tie."

So he goes back to the truck and he hasn't got a rag, he hasn't got a piece of paper, he hasn't got anything, but he finds his jumper cables, and he takes the jumper cables and he puts them around his neck and he ties them into a tie. And he gets out of the truck and he goes back to the bouncer, and he says, "Are my jumper cables acceptable?" And the bouncer looks at him and he picks up a jumper cable and he says, "Yes, they're just fine if you don't try to start anything."

COHON: There's your standard, Mr. McGowan.

DEVLIN: Well, you know I'm here to start something, and I have something to add that has never been mentioned before, and it came from an NRC report that was sent to me, and there was one little paragraph like the 10 per cent DOD stuff. And it said that there was a secret meeting where the public was not invited, of the SEC. And it was held in October. But if the public wanted to know about it, they could send for the tape, so of course I called Washington and I sent for the tape, and I made copies for you, one for Dr. Itkin and one for you, Jared.

And what this is about is how this whole project is going to affect the stock market, and there is a blue book involved in it, and because I'm giving these to you along with my television tape of my other reports, I want you to send for two, and with your title and your prestige, since I have none, I'm just the public, I would appreciate one of the books when you get it.

And this is very interesting because again, we have never talked about the risk to the businesses and to the markets, and so on, and this is something new, and it should be considered. I'm sure the Hughes Corporation is hysterical about all this. On my tape, there's going to be quotes from Price Anderson, talking about 500 million for an accident and 60 million for the attorneys. That would not build half a casino in Las Vegas, and it is quite shocking.

But this business on uncertainty with financial markets is very real, and I'm a stock broker, I was the third woman licensed in California in '63, and I live off the market, and I think of what I put into my television program about Fluer-Daniel. And, Wendy, I spelled it F-l-e-u-r. I'm very French. And as a result, I said they got a billion dollars to get that mess in Hanford cleaned up, and they've got to pull out the rods and they don't know how to do it.

Now, what if they blow up? There we're talking serious stuff with the tri-cities. So we're getting into a lot of things that have never been mentioned before, and I think financial risk should be mentioned. It is certainly uncertainty.

And I'm going to close--is my five minutes up almost?

COHON: Yeah, your time is up.

DEVLIN: I figured that. I wrote a sentence for Abe because he's my friendly adversary, and I want everybody to hear it in my toastmaster's run on sentence; right? Okay, I'm going to iterate in Monte Carlo, adorned in my assumed uncertainty, which can be dealt with under the context of the moment if it's critical.

Thank you.

COHON: Thank you, Sally. Earle Dixon, University of Nevada, Las Vegas.

DIXON: Good evening. My name is Earle Dixon. I work on behalf of the community advisory board for the Nevada Test Site programs. We're funded under Environmental Management, Department of Energy, Nevada Field Operations Office.

Some of the comments that I want to bring out as this program continues to move forward, and maybe the Nuclear Waste Technical Review Board can ponder it a little bit, is what if you had a field laboratory nearby Yucca Mountain where radionuclides were already dispersed in the groundwater system without any engineered barriers? Would that be of benefit to reduce uncertainty in the Yucca Mountain program?

Also, if the Nevada Test Site was on the Superfund list, the national priority list, would that make a difference in the siting for Yucca Mountain, being that you would be placing a Superfund site downgradient of an existing Superfund site?

Also, if the citizens or the Republic of Nevada are concerned about Yucca Mountain, then where is the consistency for the concern of the existing contamination that's already dispersed in the groundwater system at the Nevada Test Site? That seems to be the worst fear of Yucca Mountain, is what if it gets into the groundwater. Well, folks, we already have some of that stuff in the groundwater and we don't know where it's going. We don't know the speed of the water. We don't know the behavior of the radionuclide contaminants in the water system.

Sorry to bring the joke down, but these are just some of the questions that I ponder, that we already have an existing issue out there, and maybe programs could be working together, plus concerns of people in Nevada and state agencies in Nevada could get on a consistent format and take a look at existing contamination, as well as future.

I find it ironic that Nye County has an Early Warning Drilling program for contaminants. Their program may be a few thousand years too early to monitor those contaminants if the program ever goes forward at Yucca Mountain, but we have no monitoring program that is a sophisticated state of the art program to monitor existing contamination.

Thanks very much.

COHON: Thank you. Mr. McGowan?

MC GOWAN: I'll cope. Mr. Chairman, where do you want me? Take your time with that. Do you want me here or over there?

COHON: It's up to you. Do you prefer here? Come on up.

MC GOWAN: Just contemporaneous here, let the record reflect that nobody responded to the questions that were seriously posed by Mr. Dixon. They were very intelligent questions, very germane. And it's even more germane that nobody responded. That's what's significant. Take your time with that one. Why does this have more base? You can give me a little bit of trouble. I'm a young fellow.

Mr. Chairman, if you'll grant me an additional ten and a half seconds, okay? Thank you very much.

Sally told an Irish joke. I happen to be Irish and Italian. A gentleman ran into a store and he was in an apparent hurry, and he said to the clerk, "Give me a pound and a half of lean ground round, two pounds of thin spaghetti, six fresh tomatoes, some onions, garlic, olive oil, some grated Romanno cheese, and a bottle of Prego red, and snap it up. My wife is out in the car. She's waiting to make dinner. We're expecting company." And the clerk said, "Excuse me, sir, but you must be Italian." And he said, "Well, really, what made you think so?" He said, "Because this is a hardware store."

And that's exactly the picture here. This repository isn't a repository. I don't know what it is you're talking about. You're in a five mile tunnel? Lots of luck.

Anyhow, Tom McGowan is my name, Las Vegas, Nevada. Mr. Greg White, representative of NURAC, gave an excellent presentation. Perhaps ironically utilized the phrase final solution, which I thought was particularly apt, and I'll leave you to cope with that at your discretion and convenience. Again, no response.

My comment is unequivocal and uncompromising, and I'll really get right into it now. The underground hydrogeologic domain is naturally in a state of variable from inception through completion of the entire enduring term of geologic continuum. Correct me if I'm mistaken. Consequently, it's axiomatic that the safe, secure and human intrusion impervious underground storage for high-level nuclear waste is impossible to achieve, and long sustained over any enduring term by any combination of natural engineered barriers, either at Yucca Mountain, Nevada or elsewhere nationally, or anywhere on the planet, not withstanding Dr. van Luik's apparent obsession with the Oclo experience. Is that correct, Abe?

Hello. How are you? Evolution or creation, what's the difference? Don't you know they both go around at the same time all the time all over the universe?

The issue of nuclear waste is not Nevada centric, and it is a national, global and inter-generational context, significance of enduring effective consequence in perpetuity. This is not a simple little limited incremental project. It's a process ongoing in continuum. It will be here a long time.

Therefore, I recommend and request that you, the Chairman and the members of the Nuclear Waste Technical Review Board summarily terminate these activities, convey that message to Washington, D.C., tell the Congress, don't ask, tell the Congress and the President of the United States to repeal the Nuclear Waste Policy Act completely and permanently, and to reject any further attempts by the nuclear power industry and their political pawns to cause this nation, it's leadership, its agencies and its people to become the scourge of mankind and nature combined.

Or ultimately, the generic you, not just you, generic you, including the nuclear power industry, the Congress, President, and you notice the order of significance, the NAS, NRC, the U.S. NRC, the EPA, the DOE, OCRWM, YMPO and the TRB, must stand accountable and indelibly self-labeled as the current general of irresponsible and unreasoning beings who failed utterly themselves, each other, and all posterity by attaining the context of the prior knowledgeable, willful, deliberate and malicious killers of human and all other species of organic life, and the destroyers of natural resources requisite to sustain life, and thereas, ultimately causal of the extinction of human consciousness itself.

The rest of it, forget about it. Human consciousness. Are you prepared to understand exactly what you're doing? Because not withstanding claims to the contrary, that's precisely what the generic you are doing, like self-impelled as juggernauts in precipitous decline, toward oblivion, inialation and extinction, and the inevitable consequences of irresponsibly politicized, militarized and commercialized nuclear energy during the ensuing volume of nuclear waste accumulated beyond manageable control, that's why you're here, was never more eloquently stated than it was by Dr. J. Robert Oppenheimer when in 1945, upon witnessing the detonation of the world's first atomic bomb at Alamagorda, New Mexico, quoted the prophetic words, "Now I have become death, the destroyer of worlds." If you remember that, you'd have an act.

And in a nationally televised news interview in the early Sixties when asked whether he thought nuclear power either could or should be placed under international control, he replied with characteristic candor, "It's too late. It was too late the day after Trinity." I wonder what he meant by that, as if we didn't know.

I agree with Dr. Oppenheimer assessment, qualified by the realization that both then and now it was and is not only too late, but also too soon, too soon for mankind to attain to the level of science technology, ethics, morality and integrity requisite to responsibly address and resolve the issue of nuclear power and the cumulative volume of nuclear waste in the genuine best public interest, inclusively, and inter-generationally. It's irrefutable that the generic you are currently unqualified to address the issue on all of those grounds.

Instead, like mindless and souless, devoid of integrity and conscience, you succumb to the imposition by self-serving expediency driven political and commercial interests to engage in meaningless exercise in futility. Costly and protracted quest of a confounding, illusive and intrinsicly unattainable goal, falsely and misleading the described as the "safe, secure, deep geologic repository for the permanent underground storage of high-level nuclear waste," which is both a physical impossibility and an oxymoron to begin with. There's nothing deep geologic, permanent or repository about it, and it constitutes the direct injection of toxic radionuclides into the hydrogeologic domain and eventually into the human accessible environment, with ensured ensuring consequences.

Now, you knew that from the beginning, didn't you. Of course you did. Furthermore, based upon--actually beyond the near infinitive of geophysical variables, complexities and uncertainties that plague both the repository project and the process, respective of human and geo-political variables and uncertainties, makes it impossible to guarantee effective institutional control over any such storage repository, over any substantially enduring term extending for hundreds of thousands of successive generations by any known traditional means, by any surviving and intelligible language or other communicated means.

What are you going to do, plant a plaque somewhere? Does anybody here read cuneiform? I don't. Maybe somebody does. That's only a few thousand years ago. I'm going to skip to the end, with your indulgence, Mr. Chairman, because this gets better.

I should just inject this, though. It's no secret that the dedicated Dr. Oppenheimer and his Soviet counterpart, Dr. Andre Sakarof, were each and both castigated and relegated to the scrap heap of scientific history by their respective governments, one accused of being a communist sympathizer, the other of being pro-western democracy, and each and both of which anomalous persona non grata were considered dangerous threats to the respective status quo establishment, and also with Galileo in his time, since the admission of truth is risk inherent.

So there is a danger in what you do. If you tell the absolute truth as you know it to be, you risk everything. And if you don't, you risk everything for everybody else. Now, who's going to prevail? Let's go down to the bottom line here. I want to make it very clear, and in tomorrow's comment, I'll take an opportunity to address the alternative solution. There is one. I just wanted to say this. the problem is not nuclear waste; it's human nature, as Dr. Warner North so astutely pointed out, that's exactly what it is. It's us. We have met the enemy. It is us. That's exactly the problem.

Human nature places limited special interest and expediency above the value of life itself, which proves that quantum mechanics at the fastest pathway and the densest singularity is the one between the ears.

There is a viable alternative, and it happens to be, in my view, a combination of surface based storage and monitoring, transport, and the foundation of a nuclear waste dedicated secular priesthood, enduring in perpetuity. It's too late for anything else, guys. It's over. What government is going to be here? What language will they speak? If you hadn't started secular priesthood yesterday, it may be too late for that.

But the alternative is predicated on the irrefutable fact that underground storage of nuclear waste is absolutely impossible. I'm going to get it down at this point. This is contingent for effective address of the alternative upon master fundamental reform, invocative of a public policy in process, paradigm shift toward voluntary attainment to a higher idealized standard of human spiritual effectiveness in terms of ethics, morality, reason, integrity, responsibility, and above all, conscience, in the genuine best public interest inter-generationally, and the supreme being. Because simply stated, there is no other way. This isn't genius. It's simply logic mixed in with a little bit of--a sprinkle or two of emotion, because I happen to be from the public, along with Abe van Luik. And there is no other way.

So go back to the Congress and the President and tell them the truth, not for your sake, not for my sake, but for God's sake.

Thank you.

COHON: Thank you, Mr. McGowan. That concludes today's meeting. Let me remind you that breakfast will be available in this room starting at 7:15 tomorrow. You'll all be our guests we hope for a Continental breakfast and some discussion.

The meeting reconvenes tomorrow at 8:30. Thank you.

(Whereupon, at 5:15 p.m., the meeting was adjourned.)