UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER 2001 BOARD MEETING

SCIENTIFIC AND TECHNICAL ISSUES

January 30, 2001

Longstreet Inn

Amargosa Valley, Nevada

NWTRB BOARD MEMBERS PRESENT

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

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Ayako Kurihara, Editor
Linda Hiatt, Management Analyst
Linda Coultry, Staff Assistant

I N D E X

What is the current theoretical and empirical basis for extrapolating the behavior of Alloy-22 for extremely long periods (e.g. 10,000 years)? What are the current significant gaps in understanding? How might those gaps be closed (and how long would it take)? How much of a reduction in uncertainty is likely to take place if that additional work is performed? Is that additional work necessary for making a site-recommendation decision? Why or why not?

For example, TSPA predicts that localized corrosion of Alloy-22 will never occur in Yucca Mountain because the models used in TSPA rely on the open-circuit potential of Alloy-22 never approaching or exceeding a certain critical localized corrosion potential. What theory, data, analysis, etc. form the basis for believing that open-circuit potential will not change significantly over extremely long periods?

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P R O C E E D I N G S

8:05 a.m.

COHON: My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board. I'm very pleased to welcome you to this winter meeting of our Board.

We meet as a full Board three or four times a year, usually in Nevada, and most often in Las Vegas. But we try to hold at least one of our yearly meetings in Nye County, in which the proposed repository at Yucca Mountain is located. By our count, this is the Board's third meeting here in Amargosa Valley. The residents of this community have always extended a warm and generous welcome to the Board, and we sincerely appreciate that. We also have Amargosa Valley to thank for the fact that you see no one up here wearing a tie. In fact, I think it was at our first meeting here when someone went up to the mike and said, "This is the most suits I've seen in Amargosa Valley since the big funeral," or something like that. And ever since then, ties have become a thing of the past, and we feel very comfortable and pleased about that.

I want also to extend a special welcome to all those who travelled from more distant parts of the state to be here at our meeting. We're very pleased that you all could be here. And I also want to extend a particularly special welcome to Commissioner Jeff Taguchi of Nye County, who, after my opening remarks, will say a few words of his own.

As you may know, Congress enacted the Nuclear Waste Police Act in 1982. That Act, among other things, created the Office of Civilian Radioactive Waste Management, or OCRWM, within the U.S. DOE, and it 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 the 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. And I'm assuming everybody here knows where that is.

In those same amendments in 1987, Congress created the Nuclear Waste Technical Review Board, this Board, as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. The Board does not manage the Yucca Mountain project. The Board is not even part of DOE. The Board does not have approval authority, nor does it issue licenses, like NRC. The Board has impact through its independent evaluation of COE's work, as conveyed through reports to Congress and to the Secretary of DOE, which we issue periodically, and which we are required to by the law that created us.

We also convey our views through Congressional testimony. As you may know, we issued a brief letter report last month, and copies of that report are available on the table in the rear.

As specified by the 1987 Act, the President of the United States appoints our Board members from a list of nominees submitted by the National Academy of Sciences. The Act requires that the Board be a highly multi-disciplinary group with areas of expertise covering all aspects of nuclear waste management.

Now I'd like to introduce to you the members of the Board, and in doing so, please keep in mind that we all have other jobs. In my case, I'm president of Carnegie-Mellon University in Pittsburgh, and my technical expertise is in environmental and water resource systems analysis.

John Arendt--John, would you raise your hand so people can see you--is a chemical engineer by training. After retiring from a long and distinguished career at Oak Ridge National Laboratory, John 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 an associate professor of Mechanical Engineering at Iowa State University, where he also coordinates the nuclear engineering program for the University. Dan's areas of expertise include nuclear waste management, performance assessment modeling, and materials science. Dan chairs two of our panels, the Panel on Performance Assessment and the Panel on the Repository.

Norman Christensen 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 is Director of the Center for Innovation and the Environment at the Progressive Policy Institute in Washington, D.C. Later this week, in fact on Wednesday, she joins the Rand Corporation, where she will be in their Science and Technology Division, also located in Washington, D.C. Debra is a former Deputy Assistant Secretary in the Department of Interior, and before that, she was a scientist in the U.S. Geological Survey. 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 in Washington. She's a former professor at the University of Texas in Austin, and is an expert in geotechnical engineering.

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. He's also now vice-president at Shepherd Miller, Inc. His expertise is in geochemistry.

Alberto Sagüés is Distinguished Professor of materials engineering in the Department of Civil Engineering at the University of South Florida in Tampa. He's an expert in materials engineering and 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 and experience in risk assessment and scientific team management. Jeff chairs our Panel on Environment, Regulations and Quality Assurance.

Many of you know and have worked with our staff, who are seated impressively arrayed along the side there. Bill Barnard is the executive director of the Board, and unlike the members who are part-time, all of our staff are full-time. They would say more than full-time.

I'm very pleased to introduce to you today a new member of the staff, John Pye. John, would you stand up so everybody can see you? And many of you know him already, as you should. He comes to the Board from what used to be the Morrison-Knudsen Corporation, a team member of the outgoing Yucca Mountain M&O. John was responsible, among other things, for developing a testing program to confirm post-closure performance of the engineered barrier system for the proposed repository. John has nearly a quarter century of geotechnical experience. He earned his Ph.D in rock mechanics from the University of Nottingham in England. And we're delighted that you could be on the Board. Welcome, John.

Let me turn now to the significance of this particular meeting for the Board, and we think for the program. The DOE is preparing a recommendation on whether to proceed with the development of Yucca Mountain as a site of a radioactive waste repository. This is a culmination of many years of work for the DOE, and a very important milestone in the nation's nuclear waste program. After Commissioner

Taguchi makes his welcoming remarks, Lake Barrett, Acting Director of OCRWM, will provide an overview of the OCRWM program and will discuss the program's activities and priorities over the coming months.

Following Lake, Ken Hess, the General Manager of Bechtel SAIC, LLC, the new Yucca Mountain Project contractor, will comment on the transition in this key part of the program's organization. Ken will also introduce senior members of his management team.

Next up will be Jean-Pierre Duplessy, a member of France's National Scientific Evaluation Committee, whose acronym from the French is CNE. CNE performs the same function in France as our Board performs here in the U.S. We look forward to hearing from Dr. Duplessy and learning more about the CNE's activities.

We will then move into the technical meat, if you will, of this meeting. At that point, I'll turn the gavel over to Don Runnells, who will chair the rest of today's sessions. We will start with Steve Brocoum from the Yucca Mountain Project Office, who will set the stage for the next few OCRWM talks.

Now, please note that in a departure from how Board meetings have been conducted in the recent past, the next five presentations after Steve are organized around five questions posed by the Board in advance of this meeting. The questions deal with waste package corrosion, the behavior of the unsaturated and saturated zones, the critical waste package and engineered barrier system assumptions used in OCRWM's performance assessments, and OCRWM's repository design objectives. The Board asked the project to provide specific answers to those questions and to explain the technical bases for those answers. The questions are available. They'll be displayed on the screen as well so everybody can follow along and know the context for the presentations.

Tomorrow, we will be returning to our more traditional format. John Arendt will chair the meeting at that point. And to get things started, we've asked Mark Peters to come to provide a comprehensive update of the scientific and engineering investigations that are underway. Paul Harrington will discuss the status of OCRWM's repository design initiatives. The next three presentations will be somewhat more general and will look at a number of "big picture" issues. Russ Dyer, the Yucca Mountain Project Manager, will talk about OCRWM decision-making in a learning environment. Bill Boyle will describe efforts to characterize critical uncertainties, and he'll also give the next presentation on DOE's latest views about its Repository Safety Strategy.

Tomorrow's session will conclude with two speakers from outside of OCRWM. Tom Buqo will discuss the Nye County scientific work, in particular, its Early Warning Drilling Program and its plans for conducting alluvial tracer studies. John Kessler, from the Electric Power Research Institute, will describe EPRI's new performance assessment of the proposed Yucca Mountain repository.

Let me say a few things now about opportunities that we've provided in the organization of the meeting for the public to comment and to interact during the meeting itself. This is something that's very important to the Board. We try our best to give the public as many opportunities as possible to participate in our meetings. For both today and tomorrow, public comment periods will take place immediately before the lunch break, and at the end of the day. Those wanting to comment, should sign the public comment register at the check-in table where you came in, where Linda Hiatt and Linda Coultry are sitting, and they'll be happy to help you. Let me point out, and I'll remind you again later when we get to the public comment period, that I may have to limit the amount of time we can allocate to any comment, any one person, because of the number of people signing up.

As an additional opportunity for questions, you can submit written questions to either Linda Hiatt or Linda Coultry during the meeting. The Board member who is chairing the meeting at that time will try to ask the question during the meeting itself rather than waiting for the public comment period. We'll do that, however, only if time allows. We have a very tight agenda, and it may very well be that time will not allow us to do that, to ask the question during the meeting itself. If that's the case, though, if we don't have time to ask the question, we'll ask those questions during the public comment period.

Finally, in addition to written questions to be asked by us during the meeting, we always welcome written comments for the record. Those of you who prefer not to make oral comments during a comment period or pose written 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. Again, if you'll consult one of the Lindas at the table, they'll be happy to help you.

We have also scheduled tomorrow morning at 7 o'clock in this room coffee and donuts. The Board will be here, Board members will be here, and will give a chance for those who would like to interact informally with the Board to do so.

Now, I have to offer what has become our usual disclaimer so that everybody is clear on the conduct of our meeting, what you're hearing and the significance of what you're hearing.

Our meetings are spontaneous by design. You've noticed I've been reading from a script here, but this is the only scripted part of our meeting. Everything else about it is spontaneous.

Those of you who have attended our meetings before know that the members of the Board do not hesitate to speak their minds. And let me emphasize that's precisely what they're doing when they are speaking. They're speaking their minds. They are not speaking on behalf of the Board per se. They're speaking on behalf of themselves. When we are articulating a Board position, we'll let you know. We'll make it clear. Otherwise, we're speaking as individuals.

Now, I have one very important closing comment, and in fact, it follows directly on what I've just said. What you're about to hear is a Board position. It's not just Jerry Cohon talking. I'm speaking on behalf of the Nuclear Waste Technical Review Board. And what I'm about to say will be available in written form on the table later on today.

Over the last six months, the Board has issued several letters and reports outlining its views on the status of DOE's scientific and technical work at Yucca Mountain. Although the Board's views on these matters have been expressed many times in the past, our recent communications have been especially pointed and focused, and they are particularly important now as the Yucca Mountain program nears the site recommendation milestone. For these reasons, I will summarize these key Board positions so everybody is clear on what they are.

The Board has recommended that the DOE focus significant attention on four priority areas dealing with managing uncertainty and coupled processes, which, in the Board's view, are essential elements of any DOE site recommendation. Here are the four priority areas.

(1) Meaningful quantification of conservatisms and uncertainties in DOE's performance assessments.

(2) Progress in understanding the underlying fundamental processes involved in predicting the rate of waste package corrosion.

(3) An evaluation and comparison of the base-case repository design with a low-temperature design.

(4) Development of multiple lines of evidence to support the safety case of the proposed repository. These lines of evidence should be derived independently of performance assessment, and thus, not subject to the limitations of performance assessment.

The four priority areas. In addition to these overarching priorities, the Board has made a number of suggestions about other investigations and studies that can support, complement, and supplement these four areas that

I've mentioned already. Those investigations and studies include research on the unsaturated and saturated zones as well as work to make the performance assessments more transparent and informative. As the Board continues its review of DOE's technical activities, other elements essential to the site recommendation may be identified.

Welcome again to our meeting. We're very glad so many of you could join us. We look forward to a very interesting and stimulating meeting, and I hope you will all participate.

Let me now ask Commissioner Taguchi to welcome us to Amargosa Valley.

Commissioner Taguchi?

TAGUCHI: Good morning. My name is Jeff Taguchi. I'm the Chairman of the Board of the Nye County Commissioners. I'd just like to make a few comments before I make any statement.

As to the issue about people who wear ties in Nye County, our two staff members back there wear ties, Mr. Bradshaw and Mr. Halmeister, if you'd stand up and demonstrate that particular accoutrement. That's right. Thank you very much.

You see, when I was a graduate student, one of the things that they told us was that you never get a second chance at a first impression. How many of you have heard that before? Oh, you've got to raise your hands higher. Thank you. The first impression is now over. We'll dispense with the ties. Thank you very much.

Well, one of the things you get to admire here is the weather. I'm sure some of you came from areas which are significantly colder, and where you cannot see over a hundred miles on a clear day, which we have a lot of that here in Nye County. And one of the other things that we have here is a very nice facility to meet at, and we have no rolling black-outs either, or brown-outs, whatever you want to call them.

On behalf of the Nye County Commissions, also represented by Commissioner Henry Neff over here--Henry, would you please stand up? He is my counterpart in the nuclear waste issue, and we are so glad to have him on board. He came on board just recently. I want to welcome you once again to Nye County, and it seems fitting to me that you should start this very important year with your first meeting here in the Amargosa Valley in the shadow of Yucca Mountain.

Now, this will be a significant year for the Yucca Mountain program. We are at the beginning of a new national administration, but more importantly, this year we will also be facing some extremely critical milestones. How this year unfolds is of utmost importance to the residence of Nye County, as well as the 1,500 residence here in the Amargosa Valley who are hosting us today.

I know that you all anticipated that the Department of Energy would have released its Site Recommendation Consideration Report by now, and that would be a topic of lively discussion at this meeting. That has not happened, of course, and the report has been delayed pending the completion of the Department of Energy's Inspector General's investigation into potential contractor bias in the conduct of the scientific work leading up to the possible selection of Yucca Mountain. And we here in Nye County welcome that particular investigation.

You have heard many times before that Nye County is neutral on the question of whether or not Yucca Mountain should be selected as the nation's nuclear waste repository. But you have also heard that we are not neutral on what should be the basis for that selection. Nye has always insisted that any site selection decision should be based only on science, not politics. If there is any hint whatsoever to the contrary, now is the time to find out and make that known. Any delay occasioned by the Inspector General's report is meaningless, and well worthwhile--or meaningful. I'm sorry. What cannot be tolerated is a recommendation to the President and Congress that is motivated by anything other than sound science.

Someone needs to fix the spell-check on these computers. It's just one of those amazing things, you know.

How many of you have typed the word "from" and typed the work "form" at the same time? Has anybody done that? Thank you very much. You must be right-handed. See, what did I tell you? Right-handed people, this always happens to them.

Of over 3,000 counties in the nation, Nye is the only one singled out by the federal government to permanently bear the burden of the nation's entire inventory of high-level nuclear waste from both commercial and defense activities. No community in the United States wants this dubious honor. Other states and regions have made strenuous and successful political efforts over two decades to avoid selection as a location for either temporary or permanent storage of these highly radioactive wastes.

Now, the population of Nye County has more than doubled in the last ten years. How many of you have been here ten years ago? This hotel wasn't here four years ago. We are the fastest growing county in Nevada, and among the fastest growing in the country, another dubious honor. We here in Nye County do not want our future defined by our potential selection as host to these wastes, but we have not been asked. We have not had, and do not now have, a choice to accept or reject them.

Yucca Mountain is, as you know, just one in a long series of federal impositions on a single rural community. Over 97 per cent of our county is managed by the federal government. Early in World War II, a portion of our county four times the size of the state of Rhode Island was removed from the public domain for use as the Nellis Bombing and Gunnery Range. In the early 1950's under President Truman, a portion of this area, itself larger than Rhode Island, was designated as the nation's nuclear weapons testing site. In 1999, the Department of Energy further designated portions of the Nevada Test Site in Nye County as its preferred site for disposal of low-level wastes generated throughout the defense complex.

You know, I just bought these glasses, and they don't seem to be working correctly. That's better. Well, how many of you have to do that? See. Now, you all tell the truth now. This is one of those significant issues. It's all that reading that I have to do. That's much better.

These federal impositions serve varying national interests, from national security to fiscal. And the use of the NTS alone for the nation's low-level defense wastes potentially saves the federal treasury billions of dollars compared to other alternatives, and at the same time helps open defense sites elsewhere in the country to more attractive economic futures. The Yucca Mountain program itself is for the federal government's convenience, allowing it to meet its obligations to accept spent nuclear fuel from the country's nuclear utilities when no other site is politically acceptable.

Because we have been given no charge to accept or reject this program, Nye has traditionally maintained a neutral stance, focusing instead on our own independent and objective oversight program. We, through our Nuclear Waste Repository Office with which you are very familiar with, have evaluated and critiqued the DOE studies, and have conducted our own independent studies in areas of particular importance to Nye County or areas not fully covered by the Department of Energy. You will be given another update during this meeting tomorrow--I think the agenda says at 3:25--on the Early Warning Drilling Program. We are very proud of that effort, and proud that it has met with universal acceptance and acclaim throughout the program. It represents the flagship of the type of good science Nye County conducts.

Nye County and its residents have been good citizens for the half century or more of these federal impositions on our lives. We have been proud of our contribution, involuntary as it might have been, to the country's security and vital military defense. We realize that we have not been given a choice, such as the State of Nevada's right to issue a notice of disapproval and have Congress vote up or down on that veto. But we do ask, and indeed insist, that whatever decision is made about our future be purely scientific and not political.

The role of this Board is to help in that, of course, and we have always taken comfort in our relationship with you and your capable staff and our knowledge that you take your role very seriously. And as a Commissioner of Nye County, we thank you. We look forward to continuing that relationship as this year, which could bring a Site Recommendation Report, and selection and recommendation to Congress of Yucca Mountain unfolds.

Finally, as you all know, Nye County lost a great friend and valuable leader of our scientific team last year when Nick Stellavato passed away. After a very thorough search and evaluation of severely highly qualified candidates, Nye was fortunate to be able to acquire Dale Hammermeister to succeed Nick as our On-Site Representative and head our scientific programs. Dale, would you stand up, please? You'll recognize Dale over there. He's the one wearing a tie. So you will obviously properly chastise him later as we continue on with the program, therefore, since none of you have the luxury of doing so.

Dale comes to Nye with a wealth of experience and knowledge, and some of you probably know him already, or may remember Dale from his days with the USGS and as an environmental consultant. We are lucky to get him, and he looks forward to carrying on Nick's close working and professional relationship with this Board and our staff.

I'd like to again welcome you to Nye County. Thank you very much for your time this morning. I hope that your discussions are both productive and insightful. I know many members of the public are here to take time to issue some of their concerns, as well as members of our Nye County staff. We appreciate everything that you do in relationship to the waste issues at Yucca Mountain, and we thank you for coming to the Amargosa Valley today and tomorrow. Thank you very much. Have a good meeting.

COHON: Commissioner Taguchi, thank you very much, for the tie, as well. I can take a hint.

TAGUCHI: This actually matches his particular attire.

COHON: It does. I was impressed by that.

Thank you very much for the welcome, and for the excellent contextual remarks that will guide us through the rest of this meeting over the next two days.

It's now my pleasure to introduce Lake Barrett, a man who one can say is never bored at work. Lake has recently taken over as Acting Director of OCRWM for the third time. And by our calculation, he now holds the world record for leading a Civilian Radioactive Waste Management program, and we congratulate him, both on his leadership and his perseverance.

Lake has addressed this Board often, but I think it's fair to say that none of his previous talks have occurred at such a critical junction for the Civilian Radioactive Waste Management program. The program is in the midst of completing a Site Recommendation Report for Yucca Mountain. It's doing this while completing a transition to a new contractor, Bechtel SAIC, and all of this, of course, is happening in the context of the transition to a new national administration.

Lake, we look forward to your remarks.

BARRETT: Thank you, Chairman Cohon.

I want to first start off by thanking the Board for having this meeting here in Nye County, in Amargosa Valley. I think it's very important, and the Board I know feels it's important to be here really in the most important county that is involved in this endeavor. And I would like to thank the Nye County government and the citizens of Nye County for their hosting, not necessarily voluntarily hosting, many federal establishments for a long time here in Nye County. The entire nation is indebted to Nye County for the public service that they have done. And regardless of what happens on Yucca Mountain in the future, Nye County has always been more than fair with the federal government, and we all owe a debt of gratitude to the citizens of Nye County for their activities for many, many, many decades.

I'd like to use my time to address the broader issues going on in the federal government now, and to specifically try to address the Board's September letter, and your December report. Later today, the technical staff will respond to the questions you have posed, and Dr. Brocoum will introduce our responses to those questions in the context of the site recommendation process.

We appreciate your recognition in your September letter, as well as your December report to Congress, of the significant progress that we have made since the 1998 Viability Assessment. This progress includes the collection of new data, improvements in the system and process models, and the increased integration of our technical work. We take seriously the Board's observations and recommendations regarding the technical basis developed and documents for a possible site recommendation. Consistent with the Board's observations, we recognize that needed additional work would improve the technical basis for the Secretary's decision on a possible site recommendation.

Your letter and our subsequent discussions have illuminated to me a broader issue beyond just increasing our technical basis, but also to address communication between the Program and the Board. Our respective organizations play complementary but very separate roles in important national decisions regarding the long-term management and disposition of spent fuel and high-level radioactive waste. These decisions have profound consequences, not only here in the

United States, but globally, in this complex post-cold war world. Therefore, effective communications between all levels of our organizations are central to the public interest.

My evaluation of our communications processes and procedures suggests room for improvement for both our technical and management communications. Accordingly, we at DOE are instituting a broad initiative within the Department and our contractors to improve and better integrate our communications with the Board.

This initiative is being coordinated by Richard Craun, and involves the federal staff in both Washington and Las Vegas, the management and technical staff from our M&O contractor, and scientists from the national laboratories and USGS.

For those who don't know Rick, if you could stand up, as well. I think everyone knows Rick Craun. But he will be our leading focal point and action officer for our improvements in this area.

Our intent is to ensure that we can better understand and respond and resolve Board concerns with our technical program. We hope that our efforts will result in improvements in the technical bases for any possible site recommendation, as well as enhanced confidence in the adequacy of our work. Over the coming weeks, we will discuss our improved communications approach with the Board and its staff. We hope the Board agrees with us that communications should be improved.

Observations in your recent letters and related discussions also suggest that improvements in our technical program are feasible and desirable and needed. While we take pride in the technical work and the effective and efficient management of that work, we also recognize that the scope of the necessary technical work should be constantly reevaluated as we gain additional understanding of the site. Accordingly, the Department relies on three principles to guide the Program: continuous learning, informed decision-making, and responsible stewardship.

These principles embody the process set forth in the Nuclear Waste Policy Act and are reflected in the proposed implementing regulations of the Environmental Protection Agency and Nuclear Regulatory Commission, as well as those within our own Department. Our polices and practices have been shaped by these principles, in one form or another, since the inception of the Program. We remain committed to these principles as we begin consideration of a possible recommendation regarding the Yucca Mountain site. Dr. Dyer will discuss these principles in more detail tomorrow.

In response to the concerns of the Board, and in accordance with these guiding principles, we are implementing and continually refining plans for additional work. As Chairman Cohon just pointed out, the work is focusing on four main areas, and I'm pleased to see that we seem to be on sync with that, from what the Chairman announced this morning.

These four areas would be enhancing the quantification of uncertainties in the total system performance assessment, (2) improving our understanding of the fundamental processes of waste package corrosion, (3) evaluating a lower-temperature operating mode in comparison to the above-boiling operating mode, and (4) further developing additional lines of evidence supporting the safety case.

For uncertainty analyses, we are continuing work and developing plans for new activities to further evaluate uncertainties that have a significant impact on those estimates. These activities include identifying and describing how uncertainties have been quantified or bounded in the current models, and quantifying the uncertainties most significant to the performance that have not yet been captured with a realistic probability distribution. The quantification of previously unquantified uncertainties in component models is also designed to provide insights into the degree of conservatism in the overall dose estimates. This work may be useful to policy-makers if they desire information on the potential trade-off between the projected performance of the repository and the uncertainty of that projected performance.

We appreciate the feedback from the Board through your letter of December 13th, and list of topics that should be considered in our analysis of uncertainty. Dr. Boyle will discuss this work in more detail tomorrow.

In the waste package corrosion area, we also plan additional testing, analyses, and revisions to the process models and their abstractions for the total system model to help quantify, reduce, or mitigate uncertainties. Our goal is to improve the robustness of the analyses of corrosion behavior of the waste package materials. Our technical staff will discuss this work later in the meeting.

In the repository operating mode area, in response to your recommendation, we are further evaluating and assessing the potential significance of uncertainties associated with the above-boiling operational mode of the current referenced design. The performance of lower-temperature operating modes will be further evaluated to address the view that a lower thermal load may reduce uncertainties in the coupled process models and waste package corrosion areas.

The lower-temperature modes under consideration include those that reduce drift wall temperature, waste package surface temperature, and relative humidity in the emplacement drifts. The objective is to maintain a flexible approach that will keep options open to benefit from new information gained through ongoing tests and analyses in the future.

Prior to any decision on any site recommendation, a representative low-temperature operating mode will be developed and will be analyzed. The results from the analyses of both lower-temperature operating modes and the above-boiling mode will be available for comparison and evaluation to support any site recommendation decision.

Dr. Boyle will later also discuss the Repository Safety Strategy and the development of the safety case. We agree that the sole reliance on numerical output from a total system performance assessment to demonstrate repository safety is inappropriate. Our current approach supplements the numerical performance assessment and enhances confidence in the results by demonstrating the adequacy of our testing, experimentation, and our modeling.

Our approach also incorporates the evaluation of defense-in-depth and safety margin, and the consideration of natural and anthropogenic analogue information. Both qualitative and quantitative information will be employed in making the compliance arguments to support a possible site recommendation.

In another area, you have discussed the need for a peer review of the TSPA for site recommendation. Last year, we requested an international peer review of our TSPA work that will be jointly organized by the International Atomic Energy Agency and the Nuclear Energy Agency of the OECD.

Now I would like to update the Board on our M&O contract transition activities. The new contract was awarded to Bechtel SAIC Company last November 14th. Contract transition began immediately after the award, and will be complete with Bechtel SAIC assumes full responsibility on February 12th, which is less than two weeks from today. Senior managers from TRW and Bechtel SAIC are working cooperatively with the Department to ensure a smooth transition.

At this time, I would like to recognize and compliment the entire TRW team, especially George Dials and Jack Bailey, who are here today, and all the people on the program who have completed over 1,000 deliverables under a very complicated period over the last year.

Although Bechtel SAIC will assume M&O responsibilities, our relationship with the national laboratories and the USGS will continue. They will be major contributors to the ongoing scientific and technical work that will support any decisions regarding the repository development and any approach toward the site recommendation.

Ken Hess is here, and other Bechtel senior folks, and they will be introduced later when Ken speaks.

Now I'd like to address some budgetary matters. In the FY 2001 appropriation, we were provided $398 million, which was a reduction of $40 million from the Department's request of $438 million. Additionally, $7 million was transferred to the Department's Safeguard and Security budget, therefore, leaving a net appropriation for us to be $391 million, or basically $46 million less than our request.

I would also note that during the FY 2001, that the DOE's new Office of Advanced Accelerator Application has their budget increased to $68 million, which was an approximately $40 million increase over what the administration had requested. Now, the Accelerators we believe can assist us in this Program, and will be a valuable asset later on.

The Program received approximately $150 million less over the past four years to run this program. Each year, these reductions have forced us to focus our work scope on completing the scientific activities necessary to support the site recommendation decision, but this unfortunately has required us to defer important design and engineering work needed for a license application.

We are now in the process of addressing our 2001 budget shortfalls, and we are focusing on the new work that responds to the Board concerns, and we also are focusing on the key technical issues and interchanges with the Nuclear Regulatory Commission, as well as maintain all the other aspects of the Program.

To allow better informed decision-making, much of this additional work is being moved forward to permit completion prior to any decision on the site recommendation is made. The Program's challenge is to accomplish this work while meeting Congress's expectation for a decision on whether to proceed with further development of the Yucca Mountain site this year. These expectations are clear and were voiced again in the Secretary's nomination hearing earlier this month in the Senate.

As you know, Senator Spencer Abraham was confirmed as our new Secretary of Energy. During his confirmation, he expressed his commitment to making progress on the Program, while ensuring that sound science governs the decisions on site recommendation. It is our responsibility to manage the work to assure that sound science guides the Program and maintain schedules as best possible, consistent with the principles of sound science.

The issue of waste acceptance remains still very high on our agenda, and we are actively working with utilities in an effort to resolve our 1998 obligation and the ongoing litigation that that has brought. There are current

14 cases before the Federal Circuit Court of Claims requesting damages caused by delay in waste acceptance. The totals of those are in many tens of billions of dollars.

As you know, we reached settlement this past July with PECO Energy Company, which is now part of the Exelon Generation Company, and this agreement allows PECO to adjust charges paid into the Nuclear Waste Fund for the Peach Bottom Plant. We are continuing discussions with several other utilities and hope we can reach further agreements.

The PECO settlement was an effort by the Department to responsibly address the delay in our ability to begin acceptance of commercial nuclear fuel. However, a recent lawsuit by approximately a dozen utilities challenges our authority for the adjustment of charges that PECO will pay into the Nuclear Waste Fund. We will defend that settlement in the courts.

As I'm sure you're aware, the national energy situation is extremely delicate, especially here in California and Nevada, which is very close to California. 20 plus percent of our electricity is nuclear. There is close to 10,000 megawatts of nuclear on the grid here in the west, and they do produce nuclear waste, and we must not necessarily have a repository at Yucca Mountain, but we must have responsible management of this material as we go forth.

One thing I would note is the Palo Verde plant and the San Anophry (phonetic) plant are currently putting in dry storage, temporary dry storage, because of our inability to be able to perform under the contract.

Now I'd like to turn a little bit to the regulatory framework for the siting of Yucca Mountain. The Nuclear Regulatory Commission, the Environmental Protection Agency, and the Department of Energy are each separately working to complete site specific regulatory framework for the Yucca Mountain site. Finalizing this regulatory framework is central to any site recommendation process. On January 17th, the Environmental Protection Agency submitted the draft final radiologic protection standards for Yucca Mountain to the Office of Management and Budget for interagency review. A schedule for completion of that review has not yet been established by OMB. I expect that they will probably do so in the fairly near future.

The Nuclear Regulatory Commission is also continuing work to finalize its technical requirements and criteria for the licensing of a repository at Yucca Mountain. On May 4th, we submitted DOE's draft final Yucca Mountain siting guidelines to the NRC for their review and concurrence. That concurrence process continues internal to the NRC.

Now I'd like to move on to the Site Recommendation Consideration Report. We had previously briefed you on our plans to release the Site Recommendation Consideration Report. We call it the SRCR. As you know, last December, the Secretary announced that he would await the results of the Department's Inspector General's inquiry to determine if any bias compromised the integrity of the reports or documents related to Yucca Mountain before releasing that report.

Now let me provide a few comments on this issue. Many who oversee our Program, including this Board and the Nuclear Regulatory Commission, have asked us to communicate the complex scientific and technical issues more clearly to policy-makers and the general public. Using the lessons learned in developing the Viability Assessment and our draft Environmental Impact Statement, we strived to convey the information in over 1,500 pages in the current draft of the SRCR, and its 10,000 supporting documents, in a form that could clearly communicate these complex technical issues.

Toward that goal, we asked a contractor to prepare an overview of the SRCR similar to the overview that we prepared for the Viability Assessment. The overview itself is not a fundamental scientific document at all. Its primary authors are not scientists, but liberal arts majors, and there's a team of them. They were chosen to be good writers and good communicators. Now, in the process of the developing of the overview, many drafts are written, and they are sent back to the technical community for their review, their comment to make sure that they were accurately portraying the scientific and technical aspects of the base reports within the SRCR.

Unfortunately, there was an inappropriate wrong note written by one of the authors inside the inside cover. That note was wrong, clearly was wrong, and that prompted the Secretary to ask for the Inspector General's inquiry. I think it's important that we had that inquiry. I think it's important that that continue on in a complete, thorough, aggressive manner. All I can say is I don't know what the schedule of that review will be. I do know from reports, and also personal experience, it is aggressive, it is thorough, and it is very comprehensive, and there is a very competent team from the Inspector General performing that. And we wouldn't want it to be any other way.

In conclusion, we have made significant progress over the past few years, despite significant budget constraints. We have fully implemented the integrated safety management program and taken major strides in adopting the nuclear culture program. The bulk of our energy, however, is focused on a sound science program to determine the suitability of the Yucca Mountain site.

We appreciate your constructive feedback on our activities. I believe your comments will make us have a stronger case on whatever we decide to do, and I think that's valuable, very valuable to us. Your comments and your recommendations have led to strengthening of our technical program, especially toward influencing the evolutionary stepwise design process and the analysis of uncertainty that goes with each step.

The stepwise development of the geologic repository with the design and operational flexibility and reversibility, coupled with the continuous learning feedback loops, we both believe are extremely important in a program like this, especially when it's a first-time program on something that has not been accomplished anywhere in this world.

To further elaborate on this stepwise approach, we have asked the National Academy of Science to study and advise us on the stepwise approach. I believe this should complement the messages that you have provided to us about the adequacy of the technical bases, and the sufficiency of those bases, to support the decision stage that we were at, because there are many decisions that we will constantly need to go forward with, and the concept of the learning program, listening, taking feedback into the system, and doing the right thing is what we need to do to satisfy the needs for this generation, and the generations that will follow us.

At this point, I would like to entertain any questions or comments from the Board.

COHON: Thank you very much, Lake. Thank you very much for that good presentation. It's especially pleasing that the priorities for DOE's work match up so well with the comments that the Board conveyed. Thank you for that.

We are woefully behind, which is not Lake's fault, but mine. It better be a good one, Dan. Dan Bullen for one very brief question.

BARRETT: I'll be here for the next two days, so we'll have plenty of time.

BULLEN: Bullen, Board. Just a real quick one, because I'm very pleased that the four points you mentioned match the four points that the Board had recommended. And you commented that there was going to be completion of those four points, and maybe we're stealing the thunder of the presenters early on, but do you think they're going to be sufficiently completed in time for the SR decision by the Secretary, which looks like will be later this year? Will the four points that we've identified and that will be addressed in this meeting be sufficiently completed, in light of the budget cuts and the transition time and transition to a new team? Will that all be sufficiently completed in time, or do you expect it to be?

BARRETT: That is our goal, to do exactly that. I mean, each of these items, as you well know, are very complex items. If they reach a site recommendation decision, and if it is to continue on, that work does not stop. That work goes on for many generations. What we hope to do is to show you the work we're doing, and the work we have added to the Program since last year, and we hope that you would believe that it is sufficient for the step that we will be at.

So, yes, we will be addressing it, and that will be the major topic of the meeting, and the answers to the questions, which are very good questions and very timely. So we hope to demonstrate that to you.

BULLEN: Thank you.

COHON: As you've heard, there's an important transition going on in the Program. Ken Hess is here to brief us and introduce himself and his senior management team.

Ken is leading the transition of the primary contractor. He comes to the project with a wealth of experience in the management of complex nuclear activities. Most recently, he was president of Bechtel Nuclear Power.

The Board welcomes you to this critically important national undertaking, and looks forward to working with you and your team in the coming years.

HESS: Good morning. It's a pleasure to be here, especially in front of such an august group. I welcome interaction between ourselves during the breaks. I have not had the opportunity to meet any of the Board yet, and many of the guests today.

I'd like to quickly, and to try to help with the schedule, brief you on what is going on with transition. As was indicated earlier, one of the additional headaches and Lake has had to go through this year is the major transition of the M&O contractors. One of the goals of that transition has been to make it smooth and seamless to the work that is going on in the project.

Most of our effort, most of our concerns have not been on the technical side, because many of the technical resources with the new Bechtel SAIC Company will be continuing on from the previous contractors. SAIC was a major participant in the program. Our focus has been mainly in the area of personnel, the transfer of personnel from 20 subcontractors into a new limited liability company called Bechtel SAIC company. That has consumed most of our energy.

We have also had to set up the tools necessary to start a new company, including payroll systems, financial systems, scheduling systems, et cetera.

The work force has been remapped to a new organization. We do have a new organization. And, in fact, if we skip to about the sixth page--keep going until you come to the organization. All of this information is in a handout. You can read, as well as I can read the bullets to you. It gives you an indication as to what we've completed so far in the transition. We are on schedule.

As Lake said, we will be assuming the responsibility for this contract in less than two weeks, two weeks from yesterday, in fact.

This is our organization, and basically, we have a matrix organization. At the top, you'll see the general management group. That consists of the General Manager, the Deputy, the Environmental and Safety, ES&H, Quality Assurance, and also a Program Support Office in Washington.

The key to our operation is in the Licensing and Engineering Projects Manager, our Manager of Projects, and that's Nancy Williams. And I'm going to introduce in a few minutes several of the key people in our organization who are here to participate in this meeting today and tomorrow.

Supporting the projects organization are a technical support organization and a business support organization. Those two organizations have functional managers that are responsible for providing the personnel to Nancy and the project managers under Nancy to implement the programs required for this project.

This next slide is Nancy's organization. This is the heart of our organization. Those Bechtel SAIC folks that are in the audience, would you please stand now? In the back is Nancy Williams, the Manager of Projects. You see Michael Voegele. Michael Voegele has been on the project for a few years. Bea Reilly has been on the project for about 15 years, my Communications Manager. Bob Andrews. Bob Andrews has also been on the project for many years. And Jerry King, Jerry King has been on the project for many years. Toward the back is Steve Cereghino. Steve is our Manager of Licensing.

If you could back up two slides, I'd just like to hit briefly some of the goals that we're trying to accomplish. First of all, the project team is characterized by safety and a zero accident philosophy, a nuclear regulatory culture, the right quality assurance, planning through execution, partnering with all participants. What does all that mean?

We expect to communicate, communicate, communicate. We expect our people to work safety. We want the people that come into work each day to be able to go home safely at night. I have worked in a regulatory culture for over 30 years of my life, and a regulatory culture is something that is not generated by procedures, by inspections or by audits. It's a philosophy. It's a way of doing business. That is what we're bringing to the project. That's how we expect to do business.

The right quality assurance. Again, quality assurance cannot be inspected into the job. It has to start with good procedures, and an attitude to follow those procedures, and a questioning attitude.

Partnering with participants. What does that mean? We expect everybody to do their job, but we expect to communicate with one another. We expect to earn your trust. We expect to earn your respect.

Balancing science, regulatory and engineering needs. That is our challenge on this project. We want to move forward. We want to move forward with the new work that has been identified, and we will do that smartly. The project is subject to agreed-upon metrics. What does that mean? We will develop metrics that show you the progress that we are making toward the goals that you have established. We are here to manage the work, to meet those goals, and to manage the objectives of the Department of Energy.

Lastly, we want to acquire and retain the best human resources. This project has tremendous resources, resources that don't exist other than this location in many cases. We have made a lot of efforts over the last two months to retain those resources. We believe we have done that. We have taken the steps necessary to retain the people that are important to this project.

Any questions?

COHON: Thank you very much, Ken. Again, I admonish you to not ask questions. That's good. Thank you. I really appreciate it, and welcome to the Program. Thank you very much again.

Over the years, the Board has benefitted greatly from its contacts with nuclear waste programs in other countries, and we in every case try to strike a relationship with our sister organization in that country. That includes France, and as I mentioned before, Jean-Claude Duplessy is with what is effectively our sister organization in France, and he'll be conveying to us the French experience in scientific and technical review of their high-level nuclear waste program.

Dr. Duplessy has a very distinguished background. He holds a Ph.D. in geology from the University of Paris. He's taught at a number of universities, and is a widely recognized expert on climate change, paleo-oceanography and marine geochemistry.

It's our pleasure to welcome Dr. Duplessy.

DUPLESSY: Thank you very much. I will try to speak with my French accent, so immediately you should recognize that I'm French. I would say that until now, I have heard what you have said, and I wanted to show you some difference between the U.S. review board and the French one, is that most members of the French review board wear a tie, with one exception, and this exception is me. I usually never wear a tie. So if you would allow me to--but I would put it in my pocket.

And I will very quickly show you what is the French Review Board, and later if we have time later during the meeting, I will be happy to answer questions.

Okay, so the French Nuclear Waste Review Board that we call CNE usually administers this by law in December, 1991, and defining the way the French system will work. First, the government defined its strategy, and this strategy is that we should carry that out in several ways and several areas, one of them being transmutation and partitioning the waste.

The second one should be to study how to put the nuclear waste into underground laboratories and underground repositories later, and also to study how to get interim storage. And every year, the CNE is writing a report on the program, and this report is given to the government, which forwards it to the Parliament. And it is expected, according to the law, that after 15 years, in the year 2006, the government will forward to the Parliament a final report on the global evaluation of the research, and possibly with some proposal for future direction.

How the system is working is particularly expressed by this transparency. there are a group of people who are working, doing a technical job, they are French agencies, the Atomic Energy Board, the Agency for Underground Laboratories, and also, it's not actually a board, working on the storage, interim storage. And all of these agencies have the responsibility, and they have also cooperation with both the scientific and university communities.

Every year, we make hearings and we evaluate the progress which has been done in all the three areas there, and we write one report, and it's expected that we should have to write a final report in 2006, and we forward this report to the government.

The good part is that we make recommendations, and those recommendations are taken into account by the agencies, and we progressively review the way the agencies are changing their strategy in response to the recommendations we have done, and with the very close cooperation between the agencies and the French law.

What is probably the most important thing is that after hearing all the actors, collecting also national and international expert advice, we have to place what the French activities are in regard to external activities. We write this report and we summarize the results, suggesting research program, et cetera, and looking at technical developments, different strategies, and specific needs.

So what is the present state of the French activities? First, it's partitioning. I would say that at this point research is going well. Important research has been obtained on the chemical separation. This is a project that has been in progress for many years, and we can see here that there's very significant progression made and we are very optimistic on the possibility of separation by 2006.

One of our suggestions was to execute the concept of partitioning and transmutation to one of partitioning and conditioning. It could be useful for better safety to separate radionuclides and to put one kind of chemical radionuclide into one kind of container, and some studies have been launched on that.

The second part is transmutation. Transmutation is a research which is led mainly for actinides, and some long-lived fission products, but only a few of them, not all. And we know that we should need fast neutron reactors or any kind of new innovative solutions and, therefore, this is a long-term research project and we know that by the year 2006, we just will have made a few progressions, but certainly we will not arrive with a nice device walking exactly as soon as you walk.

So we are to investigate a European frame to develop such a system, which should be both innovative and putting together also a European frame, should not be done by the French only.

Important remarks that was done after a long discussion is that partitioning and transmutation wastes would be extremely difficult, would be extremely expensive and, therefore, it's true that the approach to the problem of high-level radioactivity waste that we have, which is a volume of a few thousand cubic meters, with a huge amount of radioactivity waste, about 100,000 cubic meters, those have medium activities, there are a hundred different ways, nothing could be expected to be done later with the waste, and therefore, our conclusion was that we can't really usefully use partitioning and transmutation, and that the wastes should be taken as waste as just would be probably put into underground repositories.

Now, the geological disposal, which is the second area of the law, here we recognize that we have been slow for plenty of reasons and, therefore, the schedule of the agency who is in charge of geological disposal is extremely tight. We should have the preliminary project on the possibilities for disposal in an argillaceous formation, which has been located in the northeast of France. And we are in the process of beginning the work to evaluate that formation, which is located at Bure.

Okay, so we are looking at the scientific program and we observe that the modeling is running late. This is one of our recent observations. The granite is much more late than this, and we don't expect to have big progress in the next few months.

Now, if we look at the area, the conditioning and interim long-term storage, one of the first things that we have to remind ourselves is that this area has not been fully defined by the law and, therefore, we have to be somewhat careful. Certainly over the next few years, some strategies will be--so we are really going with this job.

Conditioning, a lot of research on new matrices to put radionuclides in glass or high quality ceramics, and so on, and this research I would say is going well. And some recent work has been launched to look at not such long-term, but medium-term behavior of the ceramics and glass.

The last point deals with the storage, interim storage, and here we have several questions, including the general strategy for storage, and a question on the integrity of the container, and we need to know how long the containers will be able to play their role. And I'm very happy to be able to hear what you have done already, and also there is some need for a better coordination between the long-term storage and creation of a repository, and this is something that has to be organized. I would say that the U.S. reflection and your thinking is really of great help for us.

So I will stop at this point, Mr. Chairman, and I will be happy to answer questions at any time.

COHON: Thank you very much, Dr. Duplessy. That was an excellent presentation, and a great deal of information presented very concisely.

Do we have questions from the Board? Dan Bullen?

BULLEN: Bullen, Board.

Dr. Duplessy, you did mention the granite site in passing, and saying that it was a little bit behind the clay site in evaluation. Your Board was primarily responsible for the determination of one site being unsuitable. Could you comment a little bit about the background on that, and maybe give us some insight as to whether or not they were going to shoot the messenger when the message wasn't what they wanted to hear?

DUPLESSY: Okay. Well, I would first say one thing. Our Board was responsible not for the boundings of site, and so on. It was responsible to warn all the agencies and the government on the fact that it will be extremely difficult to demonstrate the safety of the site, and then the government makes a decision. We took no decision at all. That's not our role. And, you know, I was expecting such a question. I would have been surprised, so I brought here a few transparencies and will just show you one or two of those transparencies.

That was the original granite site proposed by ANDRA, and that was the location of the laboratory here. And as you can see, there's two things. First, the sedimentary rocks are there, and they encounter underground water, which is exploited by farmers, and so on. And very closeby, even if you drill here, just below the laboratory, you have no communication between the water and the granite.

But if you just go to the fault, the waste will be open. And we have evidence that when you were drilling inside the granite here at two places, you were pumping in one site, and the other site was showing that the water pressure was changing. So there was communication.

As a geochemist, I cannot resist the pleasure to show you some isotope data. And just to remind that the French rule is that the long-term strategy should rest on the geology and only on the geology. And, therefore, we have to demonstrate to geology the rock has a thick barrier for several hundreds of thousands of years. And when we analyzed--not me, but ANDRA analyzed either the composition of the water, particularly they found values which were on this line, which was exactly the mixing line between some deep granite water and modern water.

You know, in our countries--have been extremely strong over the last glaciation, and with a lot of formation, and if there were very little modern water going to mix with the granite water, we would expect something, a mixing line between this granite water and not the modern water, but the ice age water, which it goes 90 per cent of the time Europe is under glacial conditions. So we were expecting values in the red line and onto the green. Unfortunately, the data falls on the green line, which shows that there's steady state conditions that we're observing today, establishing a few thousand years, which was not what we expect for the long-term.

BULLEN: Thank you very much.

COHON: Good question and very good answer. Thank you.

Dr. Duplessy, would it be possible to get your transparencies so we can make a copy so everybody can get one?

DUPLESSY: Which one?

COHON: All that you showed, if you're willing to do that.

DUPLESSY: Okay, I will give you that.

COHON: Thank you very much. And thank you for your excellent presentation, and for travelling all this way to be with us. We look forward to spending the next two days with you learning more about the French program, and comparing notes.

We will now take a break. We will reconvene in 15 minutes. Thank you to everybody who presented this morning.

(Whereupon, a brief break was taken.)

COHON: The meeting will now be chaired by Dr. Donald Runnells. Don, take it away.

RUNNELLS: Thank you, Jerry.

Well, welcome again to everyone who has come from far and wide to join us at this well attended meeting. We certainly appreciate the attendance, and we're looking forward to a couple of productive days.

I'm Dun Runnells. I'm a geochemist. I will help us through today activities. And just to introduce what's going to go on today, the format will be quite different than in the past. The folks from DOE and M&O have graciously agreed to address a set of specific questions that have--the set has been developed both by the Board and by the staff of the Board, and the goal of these questions is to provide the opportunity for an in depth presentation, and plenty of time for questions and answers at the ends of those presentations. We hope that discussion will be stimulated by this format.

The questions themselves deal with waste package corrosion, flow and transport in the unsaturated and saturated zones, performance assessment and repository design. The questions do not correlate directly with the four areas of primary concern that were discussed by Jerry Cohon and Lake Barrett this morning. But as you'll see, they touch on certain aspects of those four areas of primary concern.

I want to point out that the main theme, however, of the meeting, and certainly of the discussions that will go on here, is I think clear to everyone. It was set very early by Dr. Cohon, and that theme is whether or not you're wearing a tie. Now, I expect the speakers to go up in front, and if they have a tie, they have to take it off before they can proceed to give their presentation.

And with that bit of nonsense, we will proceed with our first speaker. And Dan Metlay is going to put up the specific questions as the speakers come to the front. Our first speaker is Steve Brocoum, who's Assistant Manager of the Office of Licensing and Regulatory Compliance. And Steve is going to talk to us about the question on waste package corrosion.

I should look up when I'm talking, shouldn't I. Steve is going to talk about a framework for a site recommendation decision. And I would say that that's fairly clear by what's on the screen up there, Steve.

BROCOUM: I'm just going to give a few introductory comments. Some of my comments that I make will overlap or amplify or modify Lake's a little bit. So basically, I'm going to talk about the framework for the site recommendation. We'll talk about some of the principles, processes and perspectives for the site recommendation, what we see as remaining work under site characterization, although information gathering continues to go on way beyond that, our approach for enhancing the technical basis for evaluating site suitability and products that will be available for the site recommendation decision, putting the TRB questions and context for responses, and some other topics.

There are, and Lake mentions, we have three principles that guide our program. Continuous learning, and an example of that would be when we learned that percolation flux was higher than we thought a few years ago, we went back and redesigned the design. So, basically, as we understand the site conditions and the behavior of the engineered system, we will continue to improve. We'll revise, the program will change. That's kind of a given.

Informed decision making. Decisions will be based on all relevant information. We want to make sure we know all the important information before we make a key decision. And those decisions can be revisited based on new information. They cannot always be reversed, but they certainly can be revisited. The reason they can't always be reversed, for example, if you were constructing drifts at a certain distance apart, you've already built them, and it's very hard to change that.

Finally, you know, we take our responsibility seriously. You know, we are responsible for all phases of the program, and that includes monitoring and oversight even after permanent closure, according to the Act of 1992.

Siting, which if you take in the broadest sense, which includes site characterization and the decision, licensing, construction, operating, and closing a repository, requires gathering information for a long period of time. It will require changing through time as we learn more. It will take decades or centuries, you know, if we go for 300 years, centuries to complete, and will result in safety geologic disposal, or else we will not go on.

A critical point is coming up, an evaluation of the suitability of Yucca Mountain for consideration as a possible geologic repository. That's our next big milestone in the program.

Under our current planning, we'll evaluate suitability. It will be based on the methods and the criteria that we have defined in our proposed suitability guidelines. That's proposed 10 CFR 60, Part 963. It will be a comprehensive technical basis. It will include multiple lines of evidence and arguments from the field and laboratory and analysis, natural analogs, numerical analysis of the information, and the performance assessments for the postclosure evaluation, consistent with the NRC's licensing criteria, and comparisons to the applicable radiation protection standards for both preclosure and postclosure performance. Some of the key standards are going to be in the proposed EPA's regulation, 40 CFR 197.

To actually go forward with the site

recommendation, of course, all those standards have to be in place. They're all in various stages of being proposed right now, and as Lake said, the EPA has gone into interagency review. I believe it's public at this point in time, I think when it goes into interagency review.

We have extended our schedule to accommodate additional information and hopefully enhance our technical basis for a possible site recommendation decision. We are having additional work done, and we hope to complete this work during this year. This includes, and I think it's very similar to the list given by Dr. Cohon and to the list given by Lake, design with a low-temperature operating mode, updated analysis and modeling reports reflecting the design changes. You have to do that. That's the backup. The TSPA, which represents a low-temperature operating mode, so a TSPA that encompasses that lower-temperature operating mode, and identification and quantification of selected key unquantified uncertainties. That will be talked to by Dr. Boyle tomorrow.

A suitability evaluation that covers both a low-temperature and a high-temperature, or a range of temperatures from low to high, in our view, is a more robust suitability evaluation than one that would just cover the high temperature. So we see that as a more robust suitability if we meet all these goals.

Just to remind ourselves what the site recommendation process looks like. We have site characterization information. Once the Secretary starts to think he may want to recommend the site, he goes into a process where he conducts public hearings on the possible site recommendation in the vicinity of the site.

Then after those hearings, and information reflects those hearings, the Secretary decides on whether to recommend the site to the President. And if he does decide to recommend the site, he has to notify the Nevada governor and the legislature of his intent. That notification has to be at least 30 days before he would send a recommendation to the President.

If he does send a recommendation to the President, and the President recommends the site to Congress, there are two possible paths. After it goes to Congress, within 60 days, the governor or legislature could submit a notice of disapproval. If that happens, the site would be disapproved unless Congress passes a resolution of siting approval during the first 90 days of continuous session following that notice of disapproval.

If the governor or legislature does not submit a notice of disapproval in that 60 day window that they have, the site would be designated effective.

The other choice, of course, is if the Secretary decides not to recommend the site, or if the President decides not to recommend the site, they must notify the governor and immediately stop all site characterization activities, and then within six months, the Secretary has to report to Congress on the recommendations for further action.

In our Program, we are over here somewhere, nearing what we see as the site characterization phase of the Program. So we haven't entered this process yet. This process will not be entered until the Secretary decides he is thinking of possibly recommending the site, and then he has to hold those hearings. So that is the process, just to remind ourselves of where we are in the process.

Our proposed suitability guidelines, 10 CFR 963, are risk informed and performance based, and they focus on overall system performance. They are consistent with the NRC's proposed licensing criteria, the proposed 10 CFR, Part 63. They include, or will include, the evaluation of the capabilities of individual barriers to better understand the performance of the overall system.

They will identify, we hope, uncertainties and quantify key unquantified uncertainties. And most recognize that some uncertainties will remain, and that's where from the NRC's perspective, that concept of reasonable assurance comes. Because for 10,000 years, you can't have proof in the normal sense of the word.

This is very important. Information gathering, under some name or another, will continue for the decades or the centuries until we close the proposed repository, and maybe beyond. We call that site characterization today. Later on, it will be test and evaluation. Performance confirmation, which is a term by the NRC, is a part of our test and evaluation program. But the point is information gathering will continue throughout the life of the Program.

External reviews of our site characterization program have identified concerns related to the technical basis for a possible recommendation. And consistent with our principles, we are going to address these concerns through ongoing testing, analysis and reevaluation.

The concerns are in these four areas, which were mentioned earlier. Quantification of uncertainties in TSPA and process models, and so on, the processes relating to waste package corrosion, comparison and evaluation of the base case design with the lower-temperature operating mode for possible ability to reduce uncertainties, and the development of multiple lines of evidence and arguments for a safety case.

These will all be discussed more during our meeting. This particular one, the multiple lines of evidence, will be discussed when we have a discussion tomorrow led by Bill Boyle on repository safety strategy.

We're trying to refocus the repository safety strategy to the strategic aspects of developing our safety case. The safety case itself would be in our site recommendation, and if we go on, in our license application. The strategy for getting there would be in our repository safety strategy.

Obviously, addressing all these concerns will improve the information available and our understanding of the expected system performance to support any potential SR decision.

We are revising our multi-year plan now. As you know, we're ending our current M&O contractor. We're just about ready to start under the Bechtel SAIC team. The current contractor is developing a plan, which will then be picked up and finalized by Bechtel SAIC, and we will be reviewing that internally over the next several months. That plan will identify specifically the work that we'll be doing for SR, as well as post-SR if the site is recommended.

So we're in a period of transition right now, and we don't have an absolute clear-cut plan at this point in time.

That revised plan may include additional testing analysis. In an earlier draft of this talk, I used the word will include, but then I decided to put the word may, because of the fact we haven't finalized the plan, we haven't costed it out, and we haven't developed all the schedules. But the intent is to address these areas to one degree or another as we move on to site recommendation.

In some areas, we will be able to address and feed directly to site recommendation. In other areas, it will be done as we do site characterization, and will continue later on. For example, the KTI areas, most of these issues are related to a possible license application. But some of the work to address these would be ongoing today. Whereas in this case, completing a TSPA, we would try to update the TSPA to include a low-temperature operating mode in time for an SR.

The kind of supporting information we would have for the SR decision would include the evaluation of uncertainties and a summary report on quantification of key unquantified uncertainties, and you'll hear more about that tomorrow. We'll have improved descriptions of thermal hydrologic models, and so on, incorporation of ongoing work on natural analogs. Obviously, we'll have a different repository layout for a lower-temperature design proposed.

So these are some of the things. Again, the work is ongoing today, and we've started. We may feed directly into the SR. Those that require new work, new testing may be done in parallel with the SR and support the SR, but the final reports may not be available for SR.

Additional work; waste package corrosion analysis model, updating the design documents to incorporate a low-temperature operating mode. I am told that we may not actually update these documents. We may write an impact report. So this may not be correct.

We have underway an international peer review of the TSPA-SR, and that is scheduled to be completed this summer sometime. Or is it early fall? Early fall. So we hope to have that completed before we go to SR. And we would like to do a peer review of waste package testing and also complete that prior to the SR. We haven't started that yet, but we are planning and working to, and this is part of the planning that I mentioned earlier, and we'd like to fund this and do this.

The five questions. I want to make a couple of comments on the five questions. Questions 1, 2 and 3 seem to be focused on understanding and a technical basis for the expected performance of particular natural and engineered barriers, and the significance of associated uncertainties. Question 4, obviously, the role of the waste package in the safety case and potential impacts of the waste package as early failure. And then Question 5 relates to the design objectives and the relative importance of those objectives.

The next talk, waste package, will be by Gerry Gordon, who will be right after me. Performance of the unsaturated and saturated zones will be addressed by Bo

Bodvarsson and Al Eddebbarh, Questions 2 and 3. Bob Andrews will discuss the contribution of the natural and engineering barriers to the system performance, including the significance of any early waste package failure. And, finally, Paul Harrington will discuss the objectives for repository design.

Obviously, we look forward to comments and to have a good dialogue with the TRB in the next two days. Our answers to these questions will be based on data and analyses that we've collected during the site characterization. The same data and analyses will be the basis for our understanding of subsystem and system performance.

I just want to make one comment here. You know, the performance of an individual barrier doesn't necessarily represent the performance of the whole system. So when we de-aggregate the system and we look at individual barriers, you know, we're looking at those for insight to the whole system, how the whole system performs. We don't want to make an error, if you want to say, just because one barrier has this much performance, that represents the performance of the whole system. That's all this viewgraph is trying to say, that bullet.

We're going to collect additional information to enhance our technical basis, as I've said already. And in using our guidelines, we'll assess the overall system performance for any potential site recommendation. We'll have a description of the expected performance of individual barriers and how it contributes to the overall performance. And we'll have the appropriate sensitivity studies to better understand overall system performance.

Then mostly tomorrow, we'll have an update on the scientific programs by Mark Peters, an update on repository design by Paul Harrington. Russ, and I believe this will be in the roundtable discussion, will discuss our approach to decision making in a learning environment. We'll also discuss the repository safety strategy by Bill Boyle I believe in the roundtable environment, and then Bill will also present our approach to evaluating uncertainties and the status of that work. We're putting a lot of effort into that.

So, final points. The geologic repository, the development is a lengthy process, decades to centuries. Testing, design and analysis will continue throughout the repository development. We can pull a site characterization today, we can pull tests and evaluation in the future, we can pull performance confirmation when we're meeting an NRC regulation.

The decision process is information-based and can be revisited based on new information. As we learn something, we can go back and revisit past decisions. And we've extended the SR process to address certain internal issues. This was supposed to be edited out. That just refers to the Inspector General's report, investigation. And to address external concerns that will enhance, we hope, our technical basis for an SR decision.

And the next viewgraph I think is very repetitive, so I think I've said all these things already. So that's basically my presentation.

Thank you.

RUNNELLS: Thank you, Steve, for a very nice overview.

You were a little sparse on the waste package corrosion that I introduced you as talking about. We'll let that go until Dr. Gordon talks.

I have one quick question before we open it up to the Board. Could you link in for me a little more clearly the performance confirmation aspect of this work? I'm specifically concerned or wondering about when it ends. Does performance confirmation include monitoring activities after permanent closure of a repository?

BROCOUM: I think formally, performance confirmation begins during site characterization, so prior to submitting a license application, and ends during repository closure. But it doesn't prohibit former additional monitoring beyond that, as I recall the NRC regulations.

However, I just want to stress that performance confirmation is a subset of our overall testing and evaluation program. Performance confirmation is required by the NRC to address specific regulatory concerns of the NRC. We will have a much more extensive testing and evaluation program throughout the period of performance confirmation, and maybe beyond, that will address many other aspects of the program.

RUNNELLS: Is there a plan, is there a document that discusses monitoring beyond the performance confirmation that you just described that's set by regulatory issues?

BROCOUM: I'm not aware of a document that discusses beyond the operating period. But that might be something we can decide during the operating period, depending on, you know, where we are at that time. I mean, we're talking about decades or centuries into the future, so I don't think--but it's not precluded. That's my issue. The issue is not precluded.

RUNNELLS: Right. Okay, good. Thanks very much.

Paul Craig has a question?

CRAIG: Paul Craig. Steve, you mentioned on one of your viewgraphs an international peer review. This is new to me, and it seems like a really good idea. A few years back, you did an internal review with the WIPP panel that yielded a lot of useful information, led to some important changes in the program. Could you tell us more about the proposed international review?

BROCOUM: If Abe could come to the microphone?

CRAIG: What's the schedule and who's going to be on it?

BROCOUM: Yeah, I started, and I think those reviewers have been named, and it's a combination of IAEA and NEA. But I think Abe could give you actually more details that, you know, might be helpful to you. Here he comes.

VAN LUIK: This is Abe Van Luik, DOE.

While I was walking up, you said everything I was going to say. What we have done is we have sent a letter to the IAEA and the NEA both asking them to coordinate a unified one peer review of our TSPA-SR. We have designated the principals. DOE principal person will be myself, and we have designed principal person, contact person, at both the other agencies.

Right now, we are in negotiating the terms of reference, and the nature of the contracts that we will sign with both of these agencies. And when that is finished, we will, as part of the terms of reference, we are proposing a schedule that begins in April, with a meeting here in the Las Vegas area, including a site visit, which will be a public meeting in which we will share information with them, and they will grill us on the materials that they have read, and the questions that they have.

And then they would go home basically and take materials with them to study. They would write a report, submit it to us. We would check it for facts only. We don't check for the tone or the contents of their recommendations or their insights, but strictly a fact checking operation. And then they would issue their final report.

And we are currently asking the NEA to publish that report so that we don't do it. You know, it looks--it could be perceived wrong if DOE published the proceedings.

CRAIG: How does the timing of this relate to the Secretary's possible decision relative to licensing? I'd like to understand how you think about the kind of information that should be available for a site recommendation in contrast to the information that should be available for a licensing decision. Is it the same or different?

MR. BROCOUM: This report would be available in time for site recommendation. In fact, originally I was pushing Abe to complete this by June. But now he tells me the fall. However, implementing all the recommendations of that report may be something we do for LA. We have to see what the report says, of course. You know, some recommendations in it may implement relatively quickly; others may require some more time. The report itself, the Secretary will have that information in his decision making.

VAN LUIK: I think it's worthy of note that these agencies are quite independent and don't want to be pushed around. And when I first submitted an idea that we start in February and finish by the end of June, they said go get someone else. So they don't want to be rushed. They want to do a good job. They will give us a critical review, and that's what we're asking for.

RUNNELLS: Priscilla?

NELSON: Nelson, Board. Steve, you may deflect this to someone else or to a specific later presentation, but my question deals with the fact that on Page 6, you identified that you're planning to complete during FY 2001, I guess, as opposed to calendar 2001, a TSPA representing a lower-temperature operating mode, and containing new site characterization information. And elsewhere, you refer to modifying TSPA to accommodate a low-temperature operating mode.

Other than geometric changes in the repository layout that might be decided in arriving at a representative lower-temperature operating mode, what other modifications to TSPA are being thought about in this very tight time frame?

BROCOUM: I think--well, first of all, we have all that uncertainty work we're doing, and any of that that we can bring into the TSPA, we would like to do that. I'm not sure if it's realistic to bring it in now. I think we can look to Bob Andrews for that. And any new information coming to the project, in other words, just updating the TSPA to incorporate the latest information that we have in the project. In some cases, it doesn't make any changes. But the major, I think, impact and the low-temperature aspects, and that's the key, that of course is the key thing to get done this year to be able to make that comparison between a low-temperature and a high-temperature design in terms of performance space. So those are the areas. Did I cover it, Bob, or is there anything else?

NELSON: Well, I'm wondering about with some more specificity. The TSPA that existed before did not have a whole lot of detail on coupled processes, and what happens in the short-term heat up/cool down. And given that that's the time framework over which the differences between the higher and lower temperature operating modes are going to be, I'm wondering exactly how TSPA is going to be modified to represent a low-temperature operating mode.

ANDREWS: This is Bob Andrews with the M&O.

There's a lot of changes that have been made since--well, let's back up a little bit here.

The TSPA-SR Rev 0 that I think the Board was given in the November/December sort of time frame, and we've had a number of briefings on prior to that, was based on scientific information and models and analyses that were more or less frozen, you know, last spring, you know, in the

March/April/May time frame. Many of those models and analyses, and the process model reports that summarize those, were documented last summer, more or less, time frame.

Many of those, you know, based on comments and based on new information that was being collected at the site, in particular, a lot of the seepage work, a lot of the coupled process work, in particular, the thermal hydrochemical coupled process work, could not be incorporated just from a timing point of view.

There have been revisions to some of those analysis model reports that were completed in November, December, and in fact this month, that we would incorporate into the revision of the TSPA which we'll call TSPA-SR Rev 1.

So, in particular, there are some stochastic analyses of thermal seepage, thermally driven seepage, that we would include. There have been modifications to the thermal hydrochemical coupled process models that we would include into this revision of the TSPA. We probably need to go point by point through some of the details of what's changed or what new information has become available since last spring. You know, I think Mark will talk a little bit about it tomorrow from the testing side and, you know, I encourage you to question Bo and Al and Jerry about the new information in their respective technical areas. But that's kind of in a nutshell on the coupled process part.

NELSON: Thank you. I think it would be interesting to go through that at some point, but probably not during this meeting, just really to understand exactly what parts you expect to change or modify to permit evaluation of the low-temperature design.

RUNNELLS: Question from Dan?

BULLEN: Bullen, Board. Maybe this is better posed at Abe than Steve, but the question I have is that we just heard about the revisions to the TSPA for SR. Which version is going to be evaluated by the international peer review panel? And when will they freeze their information and have to evaluate it? I know it's a dynamic process, but can you give us a little insight on that?

VAN LUIK: This is Van Luik, DOE. We provided the panelists, as soon as they are named, the copy is already there, Rev 0 of the TSPA-SR, which is the same Rev that you have seen. When they come out in April, we will tell them what to expect for Rev 1. When Rev 1 is still in draft, but is in the readable form in the July time frame, we will provide that to them, because they are, you know, basically on the inside working I wouldn't say for us, but working with us. And so when the document itself becomes available, they will have seen the content and will have commented on it in their peer review. That's one reason that we wanted to slip it into the September time frame.

BULLEN: So, in other words, the international peer review will indeed review the TSPA that will be used for the SR?

VAN LUIK: I couldn't have said it better myself.

BULLEN: Thank you. One more quick question, Mr. Chairman.

There was another peer review that was alluded to that was new to me, which was the waste package materials performance peer review. Could you give us a little bit of information about that, please, Steve?

BROCOUM: Paige--where's Paige? Because that hasn't started yet, so we'd like to undertake that. The reason it's not started yet, it hasn't been funded as part of this replan we're doing this year. And while she's walking, let me just make one point here. The Program is always collecting new information and we're always--we issue a document and new information keeps flowing in, and we get the kind of questions we got from Dr. Bullen as to, you know, freezing. Our lawyers, if we had appropriate classes, would want us to freeze everything once we start to think of going to site characterization until we're all done. But the reality is lots of new information is coming in all the time.

RUSSELL: Russell, DOE. What Steve just said about, we're in the process right now of pulling together that peer review. Gerry Gordon sitting right here, he's the next speaker, he is our lead in coordinating that effort. We're in a preliminary stage of gathering the names of the individuals that we feel would be appropriate for the topic, and we are in the planning stage of making sure that we have the right scope for the review planned and funded and scheduled properly for this year.

BULLEN: Bullen, Board. You just answered the question I think when you said for this year. Do you expect it to be completed in time for SR?

RUSSELL: We would expect that we would have the review complete. That's our hope today, is to be able to have it completed today. Like I said, we're in the planning process of scoping, scheduling and funding it.

BULLEN: We'll be interested to follow that as it develops. Thanks, Paige.

RUSSELL: Dan, I just got some feedback here. The initial round of comments should be in this year.

BULLEN: Thank you.

RUNNELLS: Yes, question from Dr. Sagüés?

SAGÜÉS: Yes, this is simply an addition to what Dan Bullen indicated. Your language used the word "would like" and "may be." What are the chances that that review actually will not be conducted?

BROCOUM: I think that the chance our review will occur is pretty high, because we intend to do it. The exact schedule is not fully under our control, just like the exact schedule for the TSPA is not under our control, because those people are independent and you can't actually dictate a schedule. So it has to be negotiated. I think our intent is to do the review. And the only reason I used the word "may" is we're still in the planning process, and that's why I made that comment earlier.

SAGÜÉS: Okay, thank you.

RUNNELLS: Yes, Dick?

PARIZEK: Parizek, Board. A question about the National Academy of Science review process. What's the time schedule on that initiative?

BROCOUM: The National Academy? I'm not sure which--are you talking about the report they're doing from last year? I think it's--Lake, you may have the latest information on that.

BARRETT: Barrett, DOE. You're referring to the stepwise analysis?

PARIZEK: Yes.

BARRETT: We have asked them to start it this year. We gave them a letter last year and they've agreed to do it. We've put aside the funding to do it. We've committed to the funding. And they're in the process of scoping it out now, the Board of Radioactive Waste Management. Exactly when that report will come out, I suspect it would probably be in calendar year 2002. It takes some time. They may have a letter report maybe in the fall, but a full National Academy report is a long process. For example, the one from '99 should be coming out maybe this winter or spring on the international situations. So I don't expect it to be the final report in 01.

RUNNELLS: Other questions from Board members?

(No response.)

RUNNELLS: Any questions from the staff?

(No response.)

RUNNELLS: Okay, seeing none, thank you very much, Steve. We appreciate the presentation.

We have a period for public comment, questions, and so on, following the next presentation. So we won't open it up right now for questions from the floor. We'll put that off until the end of the next presentation.

The next presentation is on waste package corrosion by Dr. Gerald Gordon. And Dr. Gordon is responsible for waste package materials testing. Dan Metlay is putting on the screen the question itself. For those of you who might not have it, it's in the agenda. And we'll turn the time over to Dr. Gordon.

GORDON: Good morning. For the next 40 or 45 minutes, I'd like to review with you some of the key experimental results, theoretical considerations, and planned path forward effort that goes into the answer to Question Number 1, which deals with Alloy 22 corrosion rates, the current status, the uncertainties associated with the corrosion rates, and corrosion behavior, the approach to extrapolating to long times, and the path forward to reduce uncertainties that currently exist. (See Question 1 in it's entirety in the Index.)

What I'd like to do is go over initially the basis for the initial selection of Alloy 22 as the corrosion resistant outer barrier on the waste package, and then review the current status of experimental theoretical work and general corrosion over long times, localized corrosion, the environment on the waste package, and long-term passive film stability considerations, and then very briefly the path forward to reduce the remaining uncertainties. Much more detail in the path forward is listed in some of the backup slides at the back of the presentation. I don't think we have time to go into that. And then some conclusions.

The next three slides list the question and a narrative answer. I don't intend to read the answer, because the presentation goes into the basis for the answer. So we can maybe skip the next--go on to the next slide.

I should point out that Alloy 22, which is a nickel, chromium, molybdenum, tungsten containing alloy, a nickel-base alloy, was developed in the early 1980s. So it's a fairly recent alloy, but it's actually the fourth generation in a series of increasingly more corrosion-resistant nickel-based alloy. The nickel/chromium alloys actually go back a hundred years, or so, and Alloy C was developed in the 1930s. It's very similar in composition to Alloy 22, or initially it was called C-22. They're both nickel, chromium, molybdenum, tungsten alloys.

The primary difference, Alloy C has somewhat more molybdenum, but more importantly, it has a fairly high maximum carbon content limit that was representative of the steel refining process back in the Thirties. And during welding or some of the thermal processing, that can result in the equivalent of sensitization and much more corrosion than one would like.

As the steel melting practice evolved over time, more corrosion resistant alloys, generations leading to C-22, were developed. But Alloy C in a sense is a commercial analog to Alloy C-22, because of its similarities. And one particular result during marine exposure at the Kure Beach test facility in North Carolina, Alloy C was exposed seaside for 57 years. In fact, samples are still being exposed. But the 57 year exposure sample was removed from the test racks. The surface was washed off of the deposits and debris, and the surface retained a mirror finish, I'll show you that on a subsequent slide, indicating a very thin stable passive film for 57 years of exposure to chloride containing environments.

The major applications for Alloy 22 are in highly corrosive environments in the petrochemical and chemical industries, and I've just listed some examples.

This is an example after washing the debris off the surface, a reflection of a flower in the surface, the mirror finish indicating the thin stable passive film after 57 years exposure.

The test results, the initial test results, plus the more recent results that I'll review, were generated under a broad range of repository relevant environments, and they provide the basis to describe the expected corrosion behavior. And the combination of the industrial experience, plus the project results, plus theoretical considerations I'll talk about, provide the basis for confidence in the empirically projected long-term performance. Because of necessity, the corrosion data we have currently and relevant environments is up to a little over two years that we've evaluated. And in the tanks, the samples have seen about three years exposure currently. And I'll review briefly the detailed experimental program and theoretical corrosion model development and qualification that's underway or planned to reduce the remaining uncertainties in this area.

What I'd like to do first is go over the current status in each of these areas, the general corrosion behavior, localized corrosion, and I'll talk more about what that is, the waste package environment, and the issue of long-term passive film stability.

In terms of general corrosion status, the available data I've broken up in summary form as long-term or short-term, long-term being up to about 2.3 years of exposure in a number of environments and temperatures in the long-term corrosion test facility at Lawrence Livermore National Laboratory. And the samples have been tested over a range of metallurgical conditions that include annealed and welded material, as well as more recently thermally aged material, and they've been tested, both uncreviced and creviced, in a range of concentrated environments. J-13 is some of the groundwaters in the vicinity of the site. It's been tested in the long-term corrosion test facility in concentrations from ten times to 3000 times, and in some of the shorter term electrochemical tests, up to about 50,000 concentration. That's near fully saturated and represents the concentration at which the chloride tends to peak. So potentially, in terms of chloride, it's the most aggressive of the environments.

The pH has been tested over a pretty broad range, and the long-term tests from 2.76 to close to 10, and in the shorter term tests, up to very basic pH value of 13. Both carbonate containing waters, like the J-13 waters, and carbonate-free, which is more representative of concentrated pore waters, have been tested in the long-term facility.

The test temperatures in the long-term facility of 60 and 90 degrees C, and in the shorter term tests, over the full range of temperatures up to the boiling point, or just below the boiling point of the highest boiling variation of concentrated J-13 solutions. And based on the long-term corrosion test results after 2.3 years of exposure, the upper bound rate is .07 microns per year of metal loss, measured after 2.3 years in the long-term test facility. And the mean rate is .01 microns per year, which corresponds to 100 angstroms per year, which is a very, very low range, on the order of 100 atom layers of metal removal.

Because of the low rate, it's very difficult to measure with weight loss specimens where you're limited by the sensitivity of the balance and dimensions of the sample, and so on. So we do plan to do more sophisticated electrochemical measurements that have higher sensitivity. And I'll talk a little about that. And when we observed the rate, it is decreasing with time, as one would expect with a protective film on the surface. That's shown in the next slide.

What I plotted here are the mean rates of the data from the long-term corrosion test facilities after six months, one year, and 2.3 years exposure. Each mean is compiled from at least 144 individual specimen measurements, and the rate does decrease with time. It appears to be levelling off, slowly decreasing at two years of exposure. And in the TSPA, the two year rate is selected as a conservative measure of the rate to extrapolate over time.

This is some independent corroboration of the rate by electrochemical measurements, in this case, linear polarization corrosion rate measurements over a several month period. This is in 10X J-13 water. And the mean rate measured electrochemically agrees very well with the mean rate after two years measured by weight loss.

This is another way to corroborate the corrosion rate. This is the surface of a specimen examined after one year at 90 centigrade in this simulated acidic water, which is approximately 1000 to 3000 concentration, and pH 2.7. And this sample is from the vapor phase exposures, because the vapor phase tends to have less deposits on it, and one can see more clearly closer to the metal surface what the corrosion products look like. This is the as machine's starting surface, and this is after one year exposure, and the vertical axis in this atomic force micrograph is three-tenths of a micron. So one can see that the thickness of the corrosion deposits, this isn't directly measuring metal loss, but at least the thickness of the corrosion products are down in the range of weight loss measurements for one year samples.

We observe on samples, especially those in the acidic water in the aqueous phase, occasional deposits of a silica rich, probably SiO_2 deposits, that they appear in patches. This happened to be the thickest patch that we found. And in profiling it, it came out at about a quarter of a micron thickness. This was after one year exposure. And when one converts that through the density of SiO_2 to an effective incremental corrosion rate, we get .063 microns per year.

And the reason we're interested in that is because this is, on the scale sample, the ASTM procedure for weight loss requires the scaling in a very acid solution, and likely the silica deposits are, in general, removed. But in some cases, they may not be, and so the weight loss is biased to a lower value by the weight of the silica. And so conservatively, we correct for that silica deposit.

This is the cumulative probability distribution, uncorrected and corrected, after the two years of exposure. And the TSPA does use the silica corrected corrosion rate as a base rate, and then it applies additional conservatisms, and you can see that on the next slide.

There's a factor of two multiplication for microbiologically influenced corrosion possibility. We don't think that so-called MIC will occur on Alloy 22. However, there is a possibility, and based on some accelerated electrochemical tests, we've picked this factor of two. Also, we don't think thermal aging will occur under the repository time/temperature history. That's documented in one of the AMRs and in the process model report on waste package degradation. But we do apply a factor of two and a half, and this is scaled from a factor of one to two and a half in a distribution function. And similarly for the MIC in the TSPA.

Another conservatism is the waste package sets for the regulatory period and beyond under the drip shield, until eventually the drip shield corrodes and there's no longer an effective barrier. But we assume the environment on the surface of the waste package, once the humidity reaches 50 per cent, which is the lowest relevant deliquescent point for forming potential saturated solutions from deposits on the waste package surface, and so we assume if the humidity reaches 50 per cent, the corrosion rate of the waste package is the same in terms of the environmental effect as if there were no drip shield.

I won't go into this, but this is the so-called logic diagram, or Decision Three, that's used in the model. It takes into account whether there's dripping or no dripping, the temperature, relative humidity, whether we have just hot air corrosion or humid air corrosion or aqueous corrosion, whether or not we can have localized corrosion. And I'll talk more about the corrosion potential relative to the critical potential, electrochemical potential, at which the film could break down. And ultimately, we get down to an effective corrosion rate, which we then multiply in some distribution fashion for MIC or thermal aging.

Going on in terms of the status, in addition to the excellent very low corrosion rates that we observed in the fairly short term experimental results, and the commercial alloy analogs like Alloy C and some of the industrial higher corrosion resistent nickel alloys.

Also a mineral exists that you've heard of, Josephinite, which is a mineral that's rich in a nickel-iron alloy, Ni3Fe, and the fact that this mineral has survived in the ambient environment, actually it was formed over a million years ago, based on radio data, and I'll talk more about that, and some initial characterization, a little later, but the fact that one can potentially have this nickel-iron alloy exposed to the ambient environment and not corrode away over time is a potential indication of passivity, and we do intend to characterize the film on this mineral, and we've started to do that.

But based on the pretty extensive experimental database, and the industry experience, commercial analogs, and so forth, we're confident that significantly more corrosion resistant Alloy 22 will maintain passivity for the required period, very importantly under repository type exposure conditions. And I'll show you that under very aggressive conditions, you can break down the passive film on Alloy 22, or almost on any alloy. However, we do have extensive path forward efforts underway to decrease the remaining uncertainties.

Switching from general corrosion to so-called localized corrosion, localized corrosion will occur if one can break down the protective passive film on the surface. And that can either be locally leading to pitting or over a broader area of the surface, leading to localized corrosion, crevice corrosion, and so forth.

And the concern with localized corrosion is if the protectiveness of the passive film is breached, then the corrosion rates can increase very, very significantly. The resistance to localized corrosion, and I'll go over some of the experimental and theoretical bases, is confirmed by extensive project and literature data.

However, as I mentioned, under aggressive conditions, and by that I mean very oxidizing, high applied potential, and in concentrated chloride solutions without the presence of beneficial ions, which I'll call buffer or inhibitor ions, like nitrate, carbonate, silicate, sulfate, that one or more are always present as the waters in the vicinity of the repository concentrate. So they provide a degree of protection, as we'll see.

This is an example, these are crevice corrosion samples of Alloy 22. There's a polished washer, flat surface. Pressed against it is another serrated washer. You can see the outline of the serrations. It's torqued, spring loaded, to form a very tight crevice. And then these samples in this particular case are exposed electrochemically to either so-called basic saturated water, or on the right, to sodium chloride, without any of the beneficial ions. And on the left is a ceramic washer crevice, which isn't as tight or aggressive as the Teflon washer crevice, which under the applied spring force, tends to creep and form a very, very tight crevice, which tends to be more aggressive.

In both of these cases, the potential on the sample is ramped upward to 550 millivolts. This is a silver, silver-chloride reference electrode. And in this case, to 800 millivolts, and we see no evidence of crevice corrosion. In this case, you see staining, but when you look at high magnification at this, it's a very thin protective oxide.

In contrast, in pure sodium chloride without any nitrate, carbonate, sulfate, and so forth, you do get crevice attack at 100 degrees Centigrade at 350 millivolts. This is the composition of the basic saturated water. It contains about 9 to 10 per cent chloride, which appears to be about as high as the chloride content can get as you evaporate J-13. And it also contains these beneficial buffer ions, as well as a small amount of fluoride that could potentially act similar to the chloride.

Crevice corrosion can occur under very oxidizing conditions when the corrosion potential of the sample, if it were to drift off to the critical potential or repassivation potential, then the possibility of crevice corrosion exists. And as we'll see, there's significant margin measured between the corrosion potential and the passive film breakdown potential over a range of relevant environments.

And the cyclic polarization measurements of crevice corrosion that I'll show you agree very well with the observations on the samples in the long-term corrosion test facility that were creviced by Teflon loaded, spring loaded crevices. And when they were taken apart after two years and samples were removed from the tanks and the surfaces cleaned and looked at at high magnification, there's no evidence of localized attack or crevice corrosion.

This is just an example of a cyclic polarization curve. This is for platinum, which remains inert in these environments. And the samples in the solution, in this case, simulated concentrated J-13 at 90 Centigrade, it starts out at the open circuit or corrosion potential, and using a potentiostat, you can polarize or ramp the potential on the sample relative to a reference electrode at some ramp rate. And you see what is normally termed passive behavior where the current, and the current density, these are one square centimeter samples, so this occurring is equivalent to the current density in amps per square centimeter, which in turn is related to the corrosion rate of the material.

And you can see passive behavior over a broad range of potentials, and eventually, the current or corrosion, apparent corrosion rate goes up. And at this point, we observe the start of oxygen evolution in this particular environment from the deposition of water as you get very oxidizing, and that continues on up, and you reach a maximum potential, or current density, and then you reverse the scan. So this is a typical cyclic polarization curve, in this case, on an inert material.

This is a curve on Alloy 22 and simulated acidic water at 90 Centigrade. And we see a behavior that looks very similar in this case to the platinum, and this is known as the maximum current density, which can be related to a corrosion rate. Because you're forcing the potential, this corrosion rate is really not representative of the true corrosion rate of the sample in a freely exposed condition. But we do see this oxygen evolution potential. It's also possible that as you get up to this point and the current starts up, that you could force the chromium oxide passive film on the surface to start dissolving and form a soluble chromate, but we don't observe that. We do observe oxygen evolution at this point, and on up.

And we go up in this case to a little over 1000 millivolts, and then we reverse the scan. When we look at this specimen after this cyclic polarization test, we find no evidence of localized corrosion. We still have the thin protective passive film on the surface. And in the process model report and the associated AMRs, this potential difference between the corrosion potential and the first threshold potential at which we see this increase in corrosion current is taken as a conservative minimum localized corrosion margin. It's quite conservative, because in reality, even at 1000 millivolts, for this particular material and environment conditions, we don't see localized corrosion.

The next slide just shows the temperature dependency of the corrosion potential. As the temperature drops, the oxygen solubility and the water increases, and that leads to a small increase in potential through the Nernst Equation. Similarly, these threshold or critical potentials, as they're called, tend to increase also with decreasing temperature.

This is a similar cyclic polarization test, this one done on the US NIC sponsored work at the Center for

Nuclear Waste Regulatory Analysis. I show it because it's a test in pure chloride without the beneficial buffer ions. And we see a similar type, not exactly the same, of passive behavior. We see a nose on the curve, and then it reverts back to a passive behavior, and then transpassive behavior, which relates to oxygen evolution again.

The curve in this case is ramped up to about 900 millivolts--or actually I think 5 milliamps was their limit, and then they reverse it. This hysteresis loop, as it's called, where this reverse scan intersects the passive line is known as the repassivation potential, and my arrow moved somehow. It's supposed to point to that intersection. That is the lowest potential at which if you initiate, say, crevice corrosion or localized corrosion at a higher potential, it will arrest at that point. And there's a lot of data in the literature that indicates that. That's a pretty conservative lower bound for localized corrosion.

The next slide is a plot of that repassivation potential, again in various unbuffered chloride media. And you can see a pretty steep increase in repassivation potential with decreasing temperature, and in particular at the lower chloride, it's still high, but the lower chloride contents.

Considering things from a theoretical standpoint, as the waste package is exposed in the repository and the temperature will drop over time, as we saw, the oxygen solubility increases cause a small corrosion potential increase, and that's a well defined increase, so many millivolts per decade of oxygen solubility increase. And the repassivation potential also increases, but it increases more steeply than the corrosion potential. So the difference tends to increase with time as the temperature drops.

Also, the very low Alloy 22 corrosion rate appears to be approaching steady state after two years. In fact, I think if our resolution was better, it probably approaches steady state in a much shorter time. But that will minimize the tendency for the corrosion potential to drift upward with time due to so-called mixed potentials, where the oxygen reduction and the metal oxidation reactions intersect and fix the corrosion potential on the metal surface. If the corrosion potential remains fairly stable with time, that mixed potential should remain stable with time.

And as I'll show you on the next slide, we have some preliminary measurements. These are some data generated at General Electric Corporate Research Center on Alloy 22. This happens to be a stress corrosion compact tension specimen exposed in this basic saturated water at 110 Centigrade. And we have measurements over a couple thousand hours that we can compare with a reference, a platinum reference electrode. It's difficult. Normally, one uses a silver, silver-chloride or Calomel or some other reference electrode. But at 110 Centigrade in this environment, the reference electrodes, really, they're at room temperature, but there's a soft bridge into the environment, and bubbles tend to form and the reference tends to not be a stable value.

The platinum is stable, so we have a good indication of, in this case, a small downward trend over time. There are a number of these samples that have been measured in different autoclave systems, and the trend is always stable, or somewhat downward. We don't see an upward trend with time.

We think that if the corrosion potential were to drift up over time, when it reaches that oxygen evolution potential, the potential is buffered by the fact we have an air saturated water environment on the surface, so we're not oxygen limited. That can draw an awful lot of current and keep the potential from drifting upwards. And that evolution potential in our measurements lies below the passive film breakdown potential.

In terms of what is the passive film, the literature indicates that in alloys of this type, the film generally consists of two layers, an inner layer next to the metal that's the more protective, chromium oxide rich layer. It contains in the case of Alloy 22, molybdenum, nickel and also tungsten. It tends to be very thin, on the order of 1000 angstroms, even thinner. And the outer layer is somewhat less protective and generally is something like chromium hydroxide, containing some of these alloy elements.

The Pourbaix diagram, which was developed by Marcelle Pourbaix back in the 1960s, indicates domains of thermodynamic stability, and I'll just show you some examples. The Pourbaix diagram does indicate that this Cr2O3 that's been measured does appear to be thermodynamically stable over a range of pH and corrosion potential. Because it's thermodynamically stable, rather than being metastable, you wouldn't expect the composition to change over time.

We are in the process at Lawrence Livermore, with the help of Professor Larry Kaufman at MIT, of doing a detailed alloy specific Pourbaix diagram calculation for Alloy 22 in a range of relevant environments.

I won't dwell on this. It's probably hard to see. But these are the Pourbaix diagrams for the individual elements in Alloy 22. This is the composition. The chromium oxide, the open circuit, or corrosion potential, in our environments tends to be about zero. This is on the hydrogen scale, and which tends to lie right in this range of Cr2O3 stability, which goes up to fairly high pH and down to a fairly low pH.

The Pourbaix diagram for tungsten indicates that

Wo3, the tungsten oxide, that is stable down to very, very low pH values, and that tends to stabilize this oxide down to even lower pH values. Molybdenum has a similar stabilizing effect, particularly in the case of chloride environments, and it has an interaction with chloride, and displacement in the film tends to reduce the tendency for chloride to displace atoms in the film.

So based on very brief but experimental theoretical consideration review, the observed localized corrosion margin is expected to be maintained, or to increase in repository relevant environments as the temperature drops over time. And the path forward efforts that are either underway or planned in the next year will provide a very significant body of added data that will increase our confidence.

In terms of the waste package environment, as I showed on one of the first slides, the corrosion testing has been performed over a broad range of potential surface environments, including a more recently identified, through evaporative concentration experiments, simulated saturated water which is nite, potassium, sodium nitrate chloride environment without any of the other anions, and it has the highest boiling point of about a little over 120 Centigrade. And the basic saturated water, which tends to have the highest chloride content of the evaporated waters, and it has a very basic pH. And these have all been tested either in long or short-term tests over a range of conditions, and the environments are bounding in terms of pH, temperature, chloride concentration, dissolved oxygen, and so forth.

One of the concerns that was raised by the state at a previous meeting was the effect of minor or trace elements, such as lead, mercury, arsenic, on both localized corrosion and stress corrosion cracking, which we haven't talked a lot about here. We are doing testing in lead chloride now, and we do plan to do testing with arsenic, mercury, some of the other elements.

There's a detailed trace element analysis of J-13 water in the backup slides, some 28 trace elements. And the lead tends to be at about six parts per billion, but it will concentrate as the water evaporates, and the actual lead content will depend on the compounds that it forms with sulfate and carbonate, and so on, as it concentrates. There is water chemistry definition effort underway to look at the chemical forms of the lead, and then arsenic, mercury, and so on, as it concentrates, and whether the lead is available to participate in corrosion reactions.

Some initial results, and I put one of the slides, I think it's the last slide in the handouts, in the backup, were done at 75 Centigrade, adding a very large amount of lead as lead chloride. And this is an area that water without any buffer ions at 75 Centigrade, and we saw no effect on either localized corrosion or stress corrosion. It's just the start of a test campaign looking at different concentrations, different forms of the lead.

In terms of the long-term passive film stability area, the film on Alloy 22 in the relevant environments does appear to be thermodynamically stable. The more specific alloy specific Pourbaix diagram calculations we hope will verify that.

Also, the Josephinite mineral, it's a natural mineral that contains a nickel iron alloy, Ni3Fe approximately in composition that I mentioned earlier has survived for actually millions of years in the ambient environment, and many millennia in stream beds, for example, in Oregon. And we have some of these mineral nuggets that were at Lawrence Livermore Laboratory, where we're starting to do some analyses that were exposed in stream beds in the Oregon area.

But the mineral Josephinite contains what's called Awaruite, which is the nickel iron alloy. It's a rock that is formed from a serpentinization reaction. Serpentine is a magnesium silicate, and back a million years or more ago, it reacted with water at 300 to 500 Centigrade under pressure in the rocks, and hydrogen gases evolved, and that leads to reduction of nickel and iron bearing oxides and sulfides in the mineral, and to this nickel iron alloy.

but as the temperature drops, the reducing conditions become less, and so you tend to get outer layers that tend to be non-metallic, but you also find metallic appearing areas which we're in the process of analyzing. I have a small nugget here that I'll pass around to the Board of Josephinite. This particular one appears to have an awful lot of metallic surface appearance to it, as well as the darker serpentine.

This is an example of one of these nuggets cut open and metallography done at very high magnification. And you can see the serpentine magnesium silicate. There are also these areas, metallic areas, at the surface. They may have formed by tumbling in the streams over the millennia. We're not sure. But we do intend to characterize the surface films here to determine if passive films do exist, what their structure is, and so forth.

In addition, one can obtain this Awaruite, which is found mainly in New Zealand, by itself without the serpentine, or with much less serpentine, and we're in the process of obtaining samples of that, and also some of the iron nickel meteorites that tend to have like 40 or 50 per cent nickel, that also have existed in the ambient environment for a millennia or longer.

In terms of our path forward to reduce remaining uncertainties, we feel that the current state of knowledge provides confidence in our understanding of the relevant Alloy 22 corrosion degradation behavior over time. But it's obviously important to reduce the remaining uncertainties and to further increase confidence in long-term behavior.

We do have an extensive path forward program I mentioned. There's a more detailed outline in the backup, but it would take a very thick backup to go into all the detail. But the program does focus on these key areas, and in particular on long-term passive film stability, because it's particularly important to successful long-term performance.

There are a number of potential degradation mechanisms that could degrade the protectiveness of the passive film over time. Some of them are listed here. There are others as well. We feel after looking at these mechanisms and where they've been observed in different alloy environment combinations, that they're unlikely to occur in Alloy 22 under relevant environments. But we are focusing on each of these, and these other potential degrading mechanisms with critical tests that we plan to do to eliminate these systematically.

The next chart is an overview of the experimental program that is either underway or in the plans. I apologize for the small type. You can probably read it in your handout. It's a multi-disciplinary, multi-laboratory plan.

The base laboratory is Lawrence Livermore National Laboratory, and the principal investigators there include Dr. Dan McCright, Dr. Gdowski, Dr. Tammy Summers, and Dr. McCright and Summers are here in the audience.

Also at the University of Nevada, Reno, Professor Denny Jones, who's also here in the audience. At General Electric's Corporate Research Center, Dr. Peter Andresen, and more recently Dr. Yun Kim, who has done a lot of work on characterizing corrosion films.

At the University of Western Ontario, Professor David Shoesmith. At the University of Virginia, Professor John Scully, who has been working on the program for a couple years now. I mentioned Professor Kaufman at MIT, who's doing some of the thermodynamic calculations with people at Livermore, and more recently, Professor Digby MacDonald at Penn State University has agreed to provide some basis in terms of fundamental mechanistic models. He's one of the preeminent mechanistic modelers for passive film performance.

And, again, there is extensive effort on confirming the expected corrosion rates, characterizing the corrosion mechanisms, developing a mechanistic model that we can benchmark, use to extrapolate over time, and continue with the demonstration of the thermodynamic stability of the film, and more effort on the passive films on natural minerals as potential analogs.

I mentioned we do need a benchmark passive film mechanistic model. A point defect model appears to be the currently most acceptable model that treats the defect migration across films of this type, anion and cation mobility, and anion and cation vacancy migration. And as was mentioned previously, we do plan to hold a peer review, or I'll call it more of a workshop, with a number of international corrosion/passivity experts to review our path forward program in detail, and to identify any key missing elements that we need to include. And we're pushing to get this going as soon as we can, budgets permitting, and so forth, as you heard from Paige Russell a few minutes ago.

So in terms of conclusions, our current extrapolation of two year plus data, which are relatively short-term, to periods on the order of 10,000 years, is based on a very extensive database that does contain a number of conservatisms, many of which I pointed out.

There are other multiple lines of evidence, such as the commercial analogs, that go back close to 60 years of demonstration of passive film performance, and the Josephinite, which once we verify the passive films, will tend to support the basis for this extrapolation over time.

The program is underway. We do expect to have extensive confirmatory experimental results by October-December. We're starting to get a lot of the results now.

One very important point I didn't mention is we do plan to measure the corrosion potentials on the specimens in the long-term corrosion test facility tanks, with up to three years of exposure. That effort is underway. And to compare that with short-term exposure to demonstrate the potential dependency over time, which hopefully is stable, or may decrease, as we've observed in some of the other tests.

And we do expect performance projections to improve as we start to remove some of these conservatisms as we get a more detailed experimental base to do that.

Thank you.

RUNNELLS: Thank you, Gerry. According to my watch, you finished about two minutes ahead of schedule.

Congratulations.

You have given us a huge amount of information, and I think in that information lie the answers to most of the questions, subquestions up there on the screen. But I want to make sure that we in fact sort of pull things together at the end in the context of the question itself. So could we go back to your Slide Number 3?

Could you bring it together for us now. Using the large amount of information you've given us, with a lot of comments on extrapolations and so on, could you now refer to the question itself and your answer and sort of bring it together for us?

GORDON: Okay. This first part on theoretical considerations on the margin that will be maintained over time against the initiation of localized corrosion, they include the expected increasing potential difference or margin between the expected slight increase in corrosion potential as the temperature drops due to the oxygen solubility increasing, compared to the more steeply increasing repassivation potential as the temperature drops. And the difference in those two is the margin, if you will, against localized corrosion.

And based on the data we have in the repository relevant environments, and also in the sodium chloride without the buffer ions, that difference appears to increase as the temperature drops.

Also, as we mentioned, if the corrosion potential does drift upward with time, it's pretty much bounded by the oxygen evolution potential in aerated water, as we have in our case, and that tends to lie below what we measure as the passive film breakdown potential. So if the potential were to drift up to 500 or 600 millivolts on this silver, silver-chloride scale where oxygen evolution occurs, it would tend to stay there and be buffered by the large amount of oxygen in the thin water film on the surface that's equilibrated with air.

In addition, corrosion potential on the surface is set by the mixed potential between the probably oxygen reduction and metal oxidation, and the oxygen reduction tends to stay stable, and the metal oxidation rate is a function of the corrosion rate, and that's starting to stabilize at two years. So one wouldn't expect much change in that. So that mixed potential should stay fairly constant, locking in the corrosion potential. It shouldn't drift significantly over time, based on those considerations.

RUNNELLS: Now, could we have Slide 4, please? Address for us, please Gerry, your estimate of the significant gaps, and so on, on that slide.

GORDON: Okay. The issues that I think represent potential gaps are issues such as the mechanisms that could degrade the potential nature of the passive film over time. And I listed several of those. The issue of minor element effects on localized corrosion hasn't been looked at in this environment system, Alloy 22 with the relevant environments. There was some early work done by Cabet Corporation, which is a predecessor to Hanes Alloys, who developed Alloy 22, and that's been published in the literature. But it's a limited effort, where they also looked at lead chloride and found no effect.

But we don't have a lot of information yet on these trade element effects. We don't expect to see a significant effect in our case because the concentrations are in parts per billion to start with, and even though they may concentrate up to the low parts per million, we don't expect to see an effect with all the buffer ions that will tend to precipitate off, for example, lead compounds. But nonetheless, that's an issue that needs more work to put to bed.

To preclude stress corrosion cracking, which is an issue that can occur under repository conditions at very high stress levels, we have some data in some of the relevant environments that indicates that's a concern, and we've addressed that with mitigation processes, laser peening and induction annealing on the two covers on the waste package that are the final closure weld area. And in both of those cases, the processes put compressive stresses in the surface, but they're limited in depth. I think the induction annealing is like six to nine millimeters of compression. That precludes stress corrosion cracking, and the laser peening of the middle lid is two or three millimeters of compression.

On the other hand, to do the induction annealing, you have to heat the sample, or the closure weld, up to 1120 Centigrade, and rapidly cool it down. It's conceivable that you could get some sort of a thermal aging effect from that heat treatment that might degrade the corrosion behavior. We don't think that's a concern, but we are looking at that. So that's another issue.

How might the gaps be closed and how long would it take? I mentioned the path forward effort. We don't have time to go into a lot of the detail. Some of the details, at least some of the key tasks are in the backups. But there's a lot of detail below that as well. But when we complete this path forward program over the next year, it will yield a significant supporting body of data by site recommendation and additional data by license application. And other data will be forthcoming up through the performance confirmation period.

This other one is more difficult to answer. How much of a reduction in uncertainty is likely to take place if the additional work is performed? That's hard to quantify. As we get more data, our confidence increases and the degree of uncertainty goes down, and it's a continuum, rather than a discrete increase in confidence, if you will.

RUNNELLS: I want to leave time for members of the Board to ask questions. You have two more slides that similarly address specific subquestions on the question you were given. I think we'll just refer the Board members and others to those slides, 5 and 6, in your packet for now, in order to allow time for specific questions from the Board members.

And, Alberto, I saw your hand first. Alberto, and then Dick.

SAGÜÉS: Very good. I wanted to congratulate you, if I may, on a thorough and well organized presentation. You had a lot of material to cover, so that was very good.

I think that we have a lot of short-term information that looks encouraging for the performance of C-22 for this application. Of course, we have an extraordinary large extrapolation gap from the very short-term empiric facts that are being accumulated to the prediction of performance into the far future. And I think that the way to fill that gap is pretty much what we have proposed, which is to try to achieve more fundamental understanding. And I really have one somewhat more overall kind of question.

If you go to Number 39, which is the technical oversight and people that you have to verify long-term corrosion performance, you have there an impressive array of talent. You have individuals and institutions which are recognized as being leaders on understanding the kind of phenomena that need to be understood to really substantiate the very long-term performance. And I guess that we're talking a little bit about the time element, and I understood that you are indicating that a lot of this kind of work will be performed in one year?

GORDON: That's right. A lot of it is underway. Essentially, well, for the areas that are just getting started are Professor Shoesmith and Professor MacDonald, their new efforts. We have statements of work, and we're getting them into our system. All of the others are funded. Professor Denny Jones is a consultant to Lawrence Livermore National Laboratory, and he also has his own programs looking at these issues.

The contract with General Electric Corporate Research Center has been ongoing for a couple of years, and is continuing and is focusing on passive film stability now as well as stress corrosion cracking. That principal investigator will be Dr. Yun Kim, who's done a lot of work on characterizing these corrosion films.

Professor Scully has been under contract for a couple years, and we have a more detailed statement of work for him that we're getting into our system to look in more detail at passive film stability. And I would expect Professor MacDonald and Professor Scully to be working together, because Professor MacDonald is more developing a theoretical model, and not doing experimental work per se, and he needs the experimental work from these other laboratories. So there's an interaction there that we plan to have.

SAGÜÉS: I believe, however, that knowing the pace of previous investigations by many of those scientists, a time frame of a few months to a year, it will be unusual to obtain the kind of detail and understanding that one would look for.

But you indicated that the efforts would continue afterwards?

GORDON: Will continue through LA.

SAGÜÉS: So we're talking the time frame of what would you say?

GORDON: LA, the current date is I think March '02. Or somebody correct me. I don't know if that schedule will slip or remain or not.

BARRETT: Barrett, DOE. I think it's fair to say that this activity will go on for years into the future as part of our test and evaluation program. So this is not going to stop at SR or stop at LA. That's a multi-year process with milestones and deliverables. And there are tests of this sort planned for the performance confirmation period, which goes up potentially to the repository closure, not as detailed.

SAGÜÉS: The other question that has to do with this as well, if you look at the, say, four or five main people over there outside the project, that is, outside--Dr. McCright and Gdowski, the organizations, what fraction of their time would these people be investing in this kind of work? I'm talking about like a few percent, or--

GORDON: No, no, it's significant. I mean, the contracts are significant dollar values. So they represent a lot of man hours.

SAGÜÉS: Okay, thank you.

RUNNELLS: Richard Parizek?

PARIZEK: Parizek, Board. Looking at your backup slide Number 3, and I see pore water chemistry, perched water chemistry and J-13, but I don't see lead, arsenic or mercury reported in some of the other waters. I guess on Page 14, you give a lot on J-13 water. But the waters that are going to see these waste packages are going to come from the roof, and so my question is what's the chemistry of the pore water. And that's relevant to the concerns that the Nevada people reported to us here six months ago, saying how rapidly things could fall apart if these other elements are present in measurable quantities. So do we know anything about that?

GORDON: Measurements are being made at Lawrence Livermore. Maybe Tammy Summers or Dr. McCright or Dr. Summers could answer that in terms of the schedule.

PARIZEK: I don't see anybody jumping to their feet. Well, we could follow up on that.

GORDON: We can follow up on that. It is planned in the very near future to do that?

PARIZEK: It's not reference waters, but real waters.

GORDON: Right, real pour waters and real J-13s.

PARIZEK: Did I understand that hot versus cold doesn't make any difference? I'm just thinking of hot/dry, hot/wet, cold/dry. Thinking about repository options, I think you're data suggests that you could live with either; is that true?

GORDON: I think it does suggest that, yes. You know, cold is going to be better in terms of passive film stability. I think diffusion processes slow down. It's an advantage, but material apparently will work in either case.

PARIZEK: This is sort of a geological analog. Whenever we look at bigger masses of things, we always find imperfections in them. If we want to make a clay liner for a landfill and it's going to be a 100 acre one, it's not the same as a little cork we prepared in a lab to test its properties. So we always have this property problem with size of the sample.

Is there anything wrong with wafer size pieces being tested versus package size canisters that are real later on that are big? I mean, is there something about making metal over big areas that may have imperfections? I mean, my car rusts in funny places. You know, I just had this funny feeling that maybe it's hard to make something that doesn't have imperfections in it in the manufacturing process. I'm not talking about the weld part of it. That's another whole special problem. But just size of material, sheets that you work with.

GORDON: If you measure pitting density, for example, the bigger the surface area, the more likely you are to be able to quantify and measure it, because there are more heterogeneities that might initiate pitting. But in the long-term facility, we're testing thousands of individual coupons.

PARIZEK: And these are selected in some sort of statistical way where you have as good chance at having bad pieces as well as good pieces?

GORDON: I think so, yeah. They represent different heats of materials.

PARIZEK: Then I thought there was a relative humidity note that you mentioned about when water might be seen on the surface of the waste package. But it seems to me that evaporate deposits that might accumulate there as a result of, say, water dripping on a warm surface creates a mineral coating, but there could be, again, moisture contents lower than 50 per cent that could still head for those mineral surfaces. It's a hydroscopic problem in terms of what's in coatings. So is it possible we'd actually have free water at lower relative humidities?

GORDON: It's possible but it's not likely with the kind of anions that we have. The lowest sodium nitrate appears to be the lowest Deliquescent point in terms of relative humidity. I mean, you know, there's magnesium, magnesium chloride could have a very low Deliquescent point, but we're very unlikely to get that because of the carbonates tend to precipitate out magnesium, and it's not there in high concentrations.

So when you look at the lower Deliquescent point, anions that could be there, they're unlikely to be in our case.

PARIZEK: Thank you.

RUNNELLS: We have about five minutes, and two people so far have asked for questions. Dan Bullen first, and then Debra Knopman, and the last question will come from Priscilla Nelson.

BULLEN: Bullen, Board. Could you go to Figure 16, please? And maybe this is just a lead-in to a question that I'll ask Bob Andrews this afternoon. But of interest here is the fact that you've introduced conservatism. You say you've conservatively further multiplied the factor by a factor of five, which is the two and the two and a half for MIC and for the aging; is that correct? Silica, I'm sorry.

The problem that I ran into here is how is this sampled? Is this the top end of a distribution, or is there a normal distribution about this corrosion rate, or is it a log normal distribution? How do you think it's sampled, and then how is it actually sampled in the PA is kind of an interesting question. So what's the connection between the data that you've derived here and the conservative assumption that you've added these multipliers to it, and then how does that tie into the PA?

GORDON: I think depending on the factor, it's either a normal distribution or a triangular distribution that varies from a factor of one, up to two and a half, or up to two. And Bob can elaborate in more detail.

BULLEN: Actually, I'd like your understanding of if it's a normal or a triangular distribution, is that the kind of distribution you would expect, and is that conservative or real or overly conservative, and what you would expect? Because adding a factor of five to the data that you have is a conservatism, and then adding a distribution to that is a further conservatism, is it not?

GORDON: That's true. I think Dr. Yun Kim wants to answer that.

LEE: Joon Lee.

GORDON: I'm sorry. Joon Lee.

LEE: In that distribution, the silica deposit, in fact we are using the CDF as it is. Okay? So in that, we are simulating 400 waste packages or more, if needed. Then we populate the distribution among waste packages. That's one thing.

The second thing is for aging and MIC fact, we are assuming no more distribution for aging, in fact, between 1 and 2.5, and the MIC fact, again, uniform distribution between one and two. But if you look at the sampling, the maximum factor from both MIC and aging could be five on top of the silica deposit distribution.

So when we do multiple realizations with 400 or more waste packages, some waste packages could have a combination of those high varies of those distributions.

BULLEN: Thank you.

RUNNELLS: A couple of minutes left. Debra?

KNOPMAN: Knopman, Board. Gerry, I wanted to go back to your answer to Question 1 on Page 3. In particular it's just not clear to me how--I don't understand these theoretical considerations well enough. I'm not a materials person, and frankly, some of the language is impenetrable to me.

But could you just work your way through the argument for high temperature conditions? We're looking at somewhere between 1500 and 2000 years in the base case design where you're going to be above 100 degrees C., as I understood one of your slides. Your short-term testing only goes up to 90 degrees C. I don't see how you cover yourself in the extrapolation process here in theoretical terms on the temperature.

GORDON: The short-term testing actually goes up to 120 C. It goes up to just below the boiling point. That's in one of the first slides. I had long-term and short-term temperature ranges. And in the short-term tests, we've tested from room temperature, up to just below the boiling point of all of the solutions that were selected, range of solutions.

KNOPMAN: But within the repository, we're going to see temperatures up to 160 degrees C. in the base case.

GORDON: That's true, but you don't have an aqueous film on the surface at those temperatures. It won't start wetting the surface until the temperature drops below the boiling point.

KNOPMAN: Okay. But presumably, there's stuff on the surface that may not be in the aqueous phase, so you've got other--you've got material that's stuck on the surface.

GORDON: Which could hydroscopically glom onto the water and humidity, and form concentrated salt solutions on the surface. But their boiling points are going to be similar to the ones we're talking about.

RUNNELLS: Last tiny question. Dr. Nelson?

NELSON: Nelson, Board. Just sort of hit on that with Debra's tail end to the question, is that this environment is not going to be particularly clean, and there is going to be dust or other materials that settle on the waste packages, on drip shields should they be used. Do you have any understanding or expectation for what that dust will be? And have any observations been made on the kinds of dust that are circulating thus far in the openings underground?

GORDON: There's some reference to that in the process model report and the AMR on the environment. And the ions that are there in the crushed tuff and some of the dirts that could be on the surface are nitrate-chloride containing. The anions are nitrate-chloride containing. There is a path forward effort in the backups to look at introduced materials in great detail, and try to bound, make sure that our solutions that we've tested in bound any hydroscopically generated solutions that could occur. That's part of the path forward effort.

NELSON: Are you actually capturing the dust that's in the ESF, or manufacturing a simulated dust?

GORDON: There's a literate review of what are the likely introduced materials, which will involve sampling of sands and other materials that could be introduced through ventilation, and also looking at what, due to construction, might be left in the drifts. But that's going on. It's a deliverable, a literature review, and that's then going to form the basis for what is actually tested.

RUNNELLS: Okay. Well, thank you very much, Gerry, for a very comprehensive and for most of us almost understandable subject.

I'll turn the time over to Dr. Cohon for the public comment period.

COHON: Thank you, Don.

We're going to turn now to the public comment period. Before we get into it, let me just relieve your anxiety about lunch, in case you're having any--I mean anxiety for lunch, for that matter. The restaurant downstairs is very nicely accommodating us by setting up a buffet lunch. You pay before you eat. You get your food. You eat. And then you're back here, with no problem, on time. There are 130 seats in the restaurant. If you can't all fit, you're more than welcome to bring your food up here, or wherever you want to go with it to be comfortable.

We have a dilemma. Seven people have signed up to comment. I'm very eager to be done by noon so that we do all have time to get lunch. Let me ask first if any of the seven who have signed up, recognizing that we have other comment periods at the end of today and two tomorrow, would be willing to yield their spot at this comment period?

(No response.)

COHON: Okay, you're each restricted to three minutes, and I'm going to be aggressive in enforcing that. I'm sorry, but it's the only way to do it. We'll go in the order in which you signed up. Dr. Jacob Paz? You can just use that mike right there. I'm going to cut you off in three minutes.

PAZ: The only thing which I'd like to say is that, first of all, I meant--what I'm trying to approach here is three issues. The first issue is the issue of complex mixtures. And here is the guidelines by EPA. They're preaching one thing, and practicing something else. The issue has not been addressed.

There is quite vast information in the literature by professional organizations. I'll give some credit to NRC, which directed Yucca Mountain project to address the issue of complex mixtures. And then we have the National Research Council, the Presidential Committee, and the National Council of Research and other professional literature. So this has not been addressed in the environmental impact statement. It might pose a very serious problem.

The second is the issue of what model you're going to address the issue of carcinogens, and how you're going to address it. I criticize Yucca Mountain not to address the issue using physiological pharmachenetic modeling, which takes into consideration impact, metabolism, recommended by EPA and the professional literature.

The third issue, the Nevada Test Site. We have about 200 of underground explosions, and in the direction of plume, which is directed into this direction, and it's a very serious issue. I just want to mention that you have tritium, about 100 million curie, and you have another 200,000--I'm sorry--200 million of other radionuclides, which probably sometime in the future will migrate.

Of course, the rate of corrosion--the rate of radionuclide and the heavy metals would depend upon the rate of corrosion, and I'm going through a question here, is it possible during oxidation reduction rate, we have hydrogen sulfide formation? If so, how its impact on the rate of corrosion.

And, finally, this is my recommendation, is to comply with all EPA guidelines, recommendations, and what appear in the literature. Second, direct the Yucca Mountain project to carry the research at UNLV, because we don't know what is the rate of cancer. The rate of cancer projected in the EIS is questioned, and is supported by scientific literature. And incorporate the Yucca Mountain program, Yucca Mountain's groundwater risk assessment, with the Nevada Test Site, which has not been, to my understanding, very complete. It just touched the surface. And the last one is establish a committee within your technical to address the issue of complex mixtures, because these are very serious issues, and unless we're going to address it scientifically, we have a problem.

Thank you.

COHON: Thank you. I don't know how you did it in that much time, but you did.

Mr. Paz, do you think we could have your overheads?

PAZ: Yes. I have for you a direct proposal.

COHON: Dr. Paz is giving us a direct proposal. Does it include all of your overheads?

PAZ: Yes.

COHON: Okay.

PAZ: Any questions?

COHON: I'm sorry, Dr. Paz, we very much want to have a copy of the overheads, too, for your record. So we'll give them back to you. I promise.

PAZ: Okay. No, I have to leave immediately.

COHON: Oh, do you have to leave?

PAZ: I can mail it to you.

COHON: Okay. Dr. Paz will mail us the overheads. Thank you very much. Don't forget your slides. Okay, we'll get them for you just before you leave.

Next, Dr. John Stuckless from USGS.

(Pause.)

We'll turn now to Ed Hanson, who is Chair of the Pahrump Nuclear Waste Advisory Board.

HANSON: I'm sorry. I must have signed up on the wrong list.

COHON: Oh, we're getting lucky here. Okay. It looks like we have much more time. Dr. Paz, did you have more you wanted to say?

PAZ: I can address it from here.

COHON: Okay, good.

PAZ: I would like to address two scenarios of potential which we talked about. One is for transportation. We have the problem of the heavy metals in the canister outside. We have lead. We have the problem of neutron poisoning. And then we have the radionuclides.

If you're going to make a risk assessment which is directed by DOE, it's inadequate. We follow the guidelines. We have guidelines of EPA.

Second, to elaborate more, the biggest concern for this area has to deal with the groundwater pollution. There is very little specific literature being addressed on the issue of complex mixtures and what it is, synergism or antagonism, and if we're looking in general context, we have another problem is potential hazardous waste site. Because of the corrosion of the heavy metals, and it very much depends on what will happen, and it has to be addressed according to 40 CFR. If it's not being addressed, then we have a problem.

Thank you.

COHON: Thank you, Dr. Paz. And let me add that Dr. Paz is president of J&L Environmental Services in Las Vegas.

Well, as a result of people signing up on the wrong list, and assuming I still have the right list, we should be able to ease the time limit a little bit for five minutes each, including--I'm sorry, is it Moret or Moret (pronouncing)?

MORET: Moret.

COHON: Moret. You can still speak if you like, and have five minutes.

Sally Devlin is next. Sally, do you want to come forward?

DEVLIN: Thank you very much, Dr. Cohon, and welcome to everybody. It is so nice to be with the grownups again, as I always say. Thank you for coming.

The reason that I'm here is we have formed a committee in Pahrump, and we have gone to the legislature, and the reason is we have enough people to be our own county and our own assembly district. This will be done, of course, by law this legislature. And the division of the county would be from the Tonapah Test Range, south to 25 miles of Clark County, which we now service. So you're getting a picture. We want to be in control, and it's about time. And they have not done this in Nevada since 1919 when they carved Pershing County out of Humbolt County. So it's going to be a historical process.

Now, what does this mean to this Board? And that means that we will have some power, and we will keep the people informed. And there are quite a few of us of the public today from Pahrump, and the reason is we are getting the word out. This is going to affect us.

I keep you up on Price Anderson. Price Anderson is now up to 9.43 billion. And when we started eight years ago, I think it was 10 million. The test site is going to get 8.9

million for new roads. So there's lots of stuff going on, and of course transportation is my field. But I want you to know the political implications. You might have to deal with a "me" and I know everybody here would be more than welcome into what we hope to call Mercury County. So that is my latest report and my latest mischief. But we haven't missed a beat on what all you are doing, and I can't wait to hear more about my bugs.

So I think my time is up. Thank you again, and welcome.

COHON: Thank you, Ms. Devlin. Next, Corbin Harney, the West Shoshone Spiritual Elder.

HARNEY: I'm glad to hear from you people here today. Today, I'm going to ask you a lot of questions, especially the DOE employees. They have addressed the main important thing that we're killing off of this mother earth that were put here with us. We, as a human, but look at the animal life today, this radiation has taken their life. Today, we're here together. If we are going to do something, think about our grandchildren and your children and all the animal life, the bird life, and so forth. If we are going to concern about them, let's not say if, I guess, I hope. Those are the words that we shouldn't have.

You should know all about what you're going to present to the public, not thinking, say I think it's going to do this. You've only been here 600 years. Look at the damage that we have done on this mother earth today. Think about it. Life on this mother earth today, whether if it's a plant life, whether if it's birds, animal life, human life, how many people have died with radiation? Today it's getting worse. Look around you how much damage that we have done on this earth today. Look at your water. What are we doing to our water? Don't we think about the future? We just thinking about it today, just like the DOE? They're thinking about accumulating more money. They've got more employees today, been totally lied to. They're going with what they're saying. It's not the way it should be.

The public should know for sure this life is going to continue. This world of our is going to continue to support us. It had support to Indian people for thousands of years. We relied on this mother earth of ours. It gives us the food. It gives us the water. It gives us the medicine. Today, those things are gone. Our water is getting disappeared around the world. What are you going to give your grandchildren and your children, and so forth? What kind of sickness this is going to develop into? Let's not guess at it, lest we all know what it is.

I hope that you guys would understand around the world everybody begins to suffer for water. I hear this throughout the world. We need clean water. You are the people that's sitting around this table today, you are the ones who can change the direction of what the DOE is doing to us.

I hope you understand what I'm saying to you. Let's work together. Let's save this mother earth of ours. Let's save our water, the air that we breathe. Air is getting contaminated. Pretty soon we won't be able to breathe this air, so much sickness today. I hope that you guys would understand the public is concerned about this around the world, not only here in this state of Nevada. The state of Nevada might be wide open country, but remember you've only been here 500 to 600 years, and look at the damage that you have created.

Let's look at this in a serious way so that way, we can save something for the younger generation that's going to be coming behind us. I hope you guys understand those things. If we don't understand it, what are we going to leave? When are we going to leave to where? We're not going to find a cleaner world anywhere. When you get there, wherever you're going, they're going to tell you the same thing. You already contaminated one earth. We don't want you here. So you go back to where you came from.

This is what we're up against today. Every day, we're contaminating this world of ours, every day, including the airplanes, including us, the chemical that we're using today, putting it into the ground and going into the water table. Look at the fish life today throughout the world. They're dying by the millions. Let's not let this continue on.

I'd like to talk again tomorrow a little deeper than what I said today. Maybe we can unite ourselves together as one people around the world, so that way we can have one voice, one head, not two or three heads. Let's not let money divide us from the DOE.

Thank you.

COHON: Thank you, sir. I'll look forward to your comments again tomorrow.

Leuren Moret, there's time if you care to speak.

MORET: I'll give up my five minutes to him.

COHON: Well, he said he would like to come back tomorrow, and we have time for you if you would like to speak.

If I may as she approaches the microphone, she's past president of the Association for Women Geoscientists.

MORET: And founder of Scientists for Indigenous People.

I'd like to read an open letter. This is to Dr. Craig Walton, Professor of Philosophy, and Dr. Allen Zundel, Assistant Professor of Political Science, University of Nevada, Las Vegas.

Dear Craig and Allen. The date is January 8th.

"Judy Treichel e-mailed your report to me today, Environmental Justice in the DOE Yucca Mountain Draft Environmental Impact Statement, an analysis of the treatment of environmental justice issues in the Department of Energy, Draft Environmental Impact Statement for the proposed nuclear waste repository at Yucca Mountain, and other documents. Here are my comments.

In 1995, the Association for Women Geoscientists introduced environmental justice to the scientific community at the annual Geological Society of America conference in New Orleans. It was introduced as an invited symposia and co-sponsored by the GSA Committee on minorities and women, and the National Association for Black Geologists and Geophysicists. It concerned the cancer corridor caused by industrial pollutants released between New Orleans and Baton Rouge in Louisiana.

Because it was well received, we have continued presenting EJ programs at GSA. This year, I organized a program for the annual GSA conference which was held last November in Reno. It seemed appropriate to present an environmental justice case study at a nuclear weapons facility, Lawrence Livermore Lab, the Nevada Test Site and Yucca Mountain. Because I had worked from 1989 to 1991 as a staff scientist at LLNL on the Yucca Mountain project part of the time, I was familiar with Yucca Mountain scientific research and the radiation issues in the Livermore community.

This year, I have worked with Tri-Valley Care on radioactive contamination in the community, and can document 1 million curies from the open literature of radioactive tritium that has been released into the Livermore Valley. 300,000 curies in one day. Elevated levels of tritium have been reported in Valley Wines, indicating that the tritium may be organically bound, increasing the toxicity 250,000 times.

LLNL has used various methods to under-rate the health effects caused by radiation contamination related to their nuclear weapons activities. The lab has monitored skin cancer, mole patrol on employees, but refused to release Social Security numbers which gave access to federal health databases to state health agencies for epidemiological studies on lab workers. Studies on community cancer rates by state agencies had funding cuts which ended their investigations. This was probably related to earlier findings in the community of elevated cancer levels in children by the same agency.

The radiation protection industry has further misrepresented the health effects from radiation by limiting it to cancer, which is only one of many illnesses resulting from exposures. After working very hard for seven months to invite speakers, Judy Treichel, a community activist in

Nevada, Corbin Harney, Dr. Andreas Tupidokis, who resigned from the Lawrence Livermore Nuclear Weapons Program on January 31st last year, Vern Breken, Carrie Dan, Western Shoshone land rights activist, Tom Carpenter, Executive Director of the Government Accountability Project in Seattle, and Dr. Marilyn Underwood from the State of California Department of Health, and with encouragement from GSA officials and members, the program for GSA was cancelled.

Three of the abstracts were arbitrarily rejected by Dr. Dave Verardo, a government employee, without explanation or committee review. It was particularly disappointing, because Dr. Verardo served as a GSA Congressional science advisor, and represents young scientific leadership nationally as the incoming chair of the GSA Public Policy Committee.

It was obvious to me that the public had nothing to do with his concept of public policy, yet the disposal of high-level radioactive waste is the most important scientific issue for this century. Because of the importance of these issues to citizens of Nevada, I would like to organize a program." I'm going to skip over that.

"Your study focused on the ethics and public policy from an environmental justice perspective. Below, are some comments on Yucca Mountain from the geologic perspective. All of these factors must be considered with the community perspective in order to make democratic decisions based on good science.

The issues being considered at Yucca Mountain not only concern the disposal of high-level radioactive waste in the U.S., but our decisions and solutions will be considered in other countries struggling with this problematic issue.

The U.S. should take the moral leadership to resolve this global issue, instead of shoving it in a can, screwing the lid on, and saying it's safe. It is critical, because of the certainty of future radioactive contamination of groundwater in the global environment, to first find a scientifically sound solution in the U.S. Geological burial of radioactive waste, in my opinion, is not suitable for a number of reasons, which should be considered by any decision maker.

Geological burial will result in radioactive contamination of the groundwater from leaking waste. It is just a matter of time. We as a global community cannot afford this. The world is out of water. Geoscientists cannot safely predict with simplistic computer modeling methods now used the complexity of natural systems interacting with high-level waste over deep time, geologic time, which can be thousands, millions or billions of years.

The viability of containers fabricated to hold high-level waste is also an unknown. Because we have been studying radiation for a short time, it is ludicrous for scientists to make statements that it will be safe in containers in underground storage for 10,000 years.

The DOE plan to fill the tunnels with cement destroys the very purpose of selecting geologic burial, the ability to retrieve and monitor high-level waste, and disturbs the natural system selected for it ability to isolate the waste. Site suitability using scientific guidelines for consideration of a geologic repository should evaluate groundwater movement, climatic stability, geologic stability. Yucca Mountain has failed to meet these criteria in investigations outlined in the draft environmental impact statement, and it's unsuitable for many reasons beyond these key factors.

It has been in the interests of the nuclear weapons and the nuclear power industries to downplay the health effects of radiation. These industries are initiating the death crisis of our species, and the disposal of high-level waste will add to the rising death toll. It is a violation of human rights to cause an unwanted attack on a person or their reproductive capacity. There are no safe levels of radiation exposure for living organisms.

Dr. Rosalee Burtell has calculated the real number of victims of the nuclear age in The Ecologist, Volume 29, Number 7, November 1999. During the past 50 years from weapons testing, she reports 376 million cancers, 235 million genetic effects, and 587 million teratogenic effects, which total 1,200 million people affected.

Electricity production from nuclear plants during 1943 to 2000 may have led to another million victims, with as much as 20 per cent resulting in premature cancer deaths. Not officially counted are as many as 500 million stillbirths from radiation exposure while in the womb during that time period."

COHON: Excuse me. I'm very sorry to interrupt. It's now been ten minutes. I wonder if you could summarize--we'll be happy to include the entire letter in the record.

MORET: Well, I can finish it later, too.

COHON: Well, that would be fine.

MORET: I'll sign up another time this afternoon.

COHON: Okay. But would you like to summarize the rest of it just so we have the complete picture?

MORET: I'd rather just read it.

COHON: Okay. Please keep your place and we'll--

MORET: Thank you.

COHON: Thank you. Our last commenter in this public comment period is Judy Treichel, Executive Director of the Nevada Nuclear Waste Task Force.

TREICHEL: I won't say the thing that I had prepared to say, and I'll do it another time. Believe it or not, Leuren

Moret and I have never met. That's the power of the Internet, I guess, and e-mail. There were people who said what you've said would be of interest to such and such, so we had never met each other.

The only thing I would like to do right now is ask Lake if the public here in Nevada and across the country is going to be looking at an SRCR. We would have had it in our laps the week before Christmas had it not been for, as you said, a bad note that was on a report.

You're talking about a lot of work that's going to be going on before a site recommendation. Is there going to be something called an SRCR dropped on us?

BARRETT: That will be Secretary Abraham's decision. I mean, I don't know. We're going to continue the scientific work. We'll present it. If we do go forward, there will be something like an SRCR, which will put the information out there, and we're anxious to get that information out to all, including the public.

TREICHEL: Okay.

BARRETT: But as far as an actual schedule, I'm not going to comment on one.

TREICHEL: And you can't tell us how much of this work that you've discussed today would be done before that happened? You're talking about a site recommendation, but this other would have preceded it, and it would have preceded the rules as well.

BARRETT: Well, the ongoing scientific work is going to continue, and it will be continuing--it's been in the past, the present, and will be in the future well past any recommendations or license applications, et cetera. And we're describing what our activity plans are for 2001, and that's what we're presenting.

TREICHEL: Okay, thank you.

COHON: Thank you, Judy.

We will now break for lunch. We'll reconvene at 1 o'clock. My thanks to all the speakers this morning.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

RUNNELLS: Bo is Laboratory Lead on UZ flow and transport models. We all know Bo. We're looking forward to his presentation. Please proceed.

Let me, while Bo is adjusting the microphone, our plan now is to leave the question up there for a few minutes, and then Dan Metlay will come over and pull it off. A couple of folks have said it's distracting, and we agree with that. So we'll leave it up there long enough for people to look at it, and then we'll pull it off.

BODVARSSON: Okay, good morning, Ladies and Gentlemen. Or good afternoon. Bo Bodvarsson, Lawrence Berkeley Lab.

I have been tasked with addressing a question from the Board on travel time basically, and there are several questions, and I had them ordered so they're also in my presentation. I'll get to them soon. This is my title slide again, unsaturated flow and transport. And here comes the objectives of the presentation.

As you know, this is the first time we have had very focused, or at least the first time I know we have had very focused questions from the Board, and the purpose of my presentation is simply to address the Board.

Do you want me to use that, or do you want me to use the viewgraphs? Who is in charge of this? You are?

Okay, that sounds good. Then I don't have to do this.

I'll pick this out to address the NWTRB on the original question. And the way we do that, this is basically on breakthrough times or travel times, and I'm going to use those terms interchangeably. Basically, what they are is how long does it take the water to move from the repository horizon to the water table. That's the UZ question.

Then the SZ question that Al is going to address later on is how long does it take for the water to move from below the repository to the accessible environment some 20 kilometers away.

So the tool we use is the UZ model to use unsaturated zone flow and transport model. We use that to estimate the breakthrough times. I'm going to discuss processes affecting breakthrough times, the important parameters, important processes from the repository to the water table.

I'm going to talk about what we call the current UZ model, which is the UZ model that is described in the UZ PMR Rev. 0 that was completed in March, or so, last year. I'm also going to talk about some refined evaluations, what we call the expected case, what we think is more of our best estimates of travel times based on our recent work that is going to be documented in the UZ PMR Rev. 1 that is planned to be completed in June or July this fiscal year to support TSPA SR.

The evolution from what I call the current conservative bounding calculations of travel times to what we call the better estimates of travel time is basically the use of a lot more information to directly look at travel times, and some of that is geochemical evidence that I'm going to describe to you.

And then, finally, I'm going to talk about the uncertainties in all of these estimates and the parameters and the processes, and then have a summary and then conclusions. And there's some discussion in between here about the current testing that is going to help us reduce the uncertainties.

These are the questions from the Board. What is the mean and variance of travel time for a conservative species from the repository horizon to the water table? And take note of a conservative species. When you hear the word conservative, that means there is no sorption allowed. No sorption in the rock matrix or in the fractures. It's just the conservative species like chlorides moving with the water, but can diffuse into the rock mass, but no sorption is allowed.

The second question is how did you arrive at this answer? What independent lines of evidence corroborate your answer? Is it just your model, or is it also some other independent lines of evidence that you use to support it? And what are the sources of uncertainty? And this comes back to the uncertainty in parameters and processes and in the models. And how much difference might the uncertainties make?

And the next slide will show basically the one line answers to each one of them, and then we'll go through the analysis.

We believe that the unsaturated zone "travel times" are on the order of thousands of years. Is it 2000 or is it 4000 years? It's not sure, but we think it's on the order of thousands of years. And I'll tell you why.

The variance is certainly in my belief less than an order of magnitude, but of course there is significant variability, and we'll discuss that also. The variance, we haven't quantified as accurately as we plan to do, but we will verify this with additional testing data and simulations.

How did you arrive at this answer? We arrived at this answer by basically use a model that has been calibrated against a lot of information collected at Yucca Mountain, including saturations, water potentials, pneumatic data, geochemical data, temperature data, and other sources of evidence. So we are trying to use the best tool that we have to address this question.

What independent lines of evidence corroborate your estimates of unsaturated zone travel time? A lot of it is related to geochemical data, because as you know, travel time can only be estimated by movement of some kind of tracers that tell us how the water moves, because we cannot recognize one water molecule from another water molecule. It has to be some kind of geochemical evidence, and I'll show you that evidence.

What are the uncertainties in these estimates? How much difference might these uncertainties make? There are quite a few uncertainties. I'm going to list them for you. There are uncertainties in parameters which are important, like the fracture porosities, fracture saturations. There are uncertainties in processes related to perched water bodies occurrences, flow of water in and around perched water bodies. There are uncertainties in the mineralogy in the Calico Hills with respect to zeolitic versus vitric, and that has effect on travel times. So there are uncertainties in these, and we'll talk about that a little bit later.

Now, before we start the discussion, I just want to make sure that we understand one thing, and that is the following. The UZ model and the SZ models, and most of these models are developed for a primary purpose. The primary purpose is to provide total system performance assessment with the date and the need to perform a system evaluation. Therefore, they are aimed towards developing this model for those kind of calculations, but developing flow fields, et cetera, et cetera.

Of course there are other purposes, too, such as getting confidence in the representations of the mountain, evaluate it from conceptual realization for flow and transport, and many, many others.

But the reason I say this is the primary purpose is to estimate dose at the accessible environment, not travel times per se. Therefore, the current model of the UZ that were reported in Rev. 0, the PMR, used some conservative assumptions regarding various items, such as, for example, including fracture flow in Calico Hills layers, such as the vitric Calico Hills where we have now evidence from Busted Butte, for example, that this is very unlikely to occur.

Similarly, we include some fracture flow in the PTn, and other approximations that make the model conservative, but very appropriate for use in dose calculations. To do a rigorous analysis of breakthrough times, if that was the emphasis of our work, we would evaluate and identify all the parameters and processes that contribute to the uncertainties, and perform stochastic analysis of the entire system to get variety of curves, such as the one TSPA shows for dosage, to get the reliable means and variances in travel times.

Therefore, today, I'm only going to talk about discrete cases, because we haven't done this. The aim has not been on travel time, but more on dose calculations.

Now, this just shows you the mountain on the right-hand side there, and some of the important parameters that we have to deal with in the unsaturated zone. We have to deal with the percolation flux that varies in space and time. We have to deal with matrix diffusion, the perched water bodies that are found in the mountain, the fracture/matrix interaction, the diffusion due to that, the flow in the fractures and flow in the matrix blocks, faults as fast pathways, and other things.

Whenever you like, you can ask questions, unless the Chairman doesn't allow that.

The key components of the UZ model are as follows. You see here on the right-hand side, the conceptual model. This is my favorite conceptual model of the mountain, and just to describe it very briefly, you see some nice colors here. Those represent the infiltration patterns, and the higher they are, the more blue they are. The lower they are, the more red they are. There are large regions where we have no infiltration. There are other regions where we have quite a lot of infiltration relatively speaking. Of course, absolute speaking, the infiltration is very low at Yucca Mountain.

Then you have fracture flow here in the Tiva Canyon, and travel times here on the order of two to three years until it reaches the PTn, the Paint Brush Unit, and there you have travel times on the order of a thousand years, or something like that. You reach the repository, and here again in Topopah Springs, you have predominant fracture flow. And then below the repository, you have complications because the zeolitic rock is very impermeable, and you get perched water bodies around it, and the vitric rock, like the Busted Butte material, is highly permeable and is basically like a porous medium.

There is grade three dimensional complexities in the system, and the main things we have to worry about is the conceptual model, because your model is numerically only as good as your conceptualization, the approach of modeling fractures and porous medium layers bounded by faults, the calibration against available data. We conduct very detailed studies of items that we think are very important to performance, such as perched water bodies, PTn, Calico Hills, et cetera. We do drift scale studies of seepage, of THC effect, ambient and thermal tests. We do predictions of breakthrough times, like I'm going to show you a little bit later, but in a discrete fracture, and we consider radionuclide migration. And what we are trying to develop is a credible model for inputs to TSPA, et cetera, et cetera.

This is a very quick slide that just simply says the geological framework comes from the geological framework model. We take all the geology as decided by the geologists and put it straight into the model right here, and then we do discretization and divide it into blocks, because when they do a numerical model, you have to have little blocks you do mass and energy balance on.

We have very discrete grids to represent faults and other major features, and we also represent interfaces, sloping, offsets, and all of the geology in a very detailed fashion.

The mathematical representation is a dual continuum approach. It's a dual permeability approach. It's a favorite approach where fracture flow can occur, matrix flow can occur, and then interaction between those two continuum, as dictated by the hydrological properties of each medium. It basically has 40 layers, all the different permeabilities, porosities, total number of parameters are 100 centimeters.

We use what is called the active fracture model to evaluate the surface area between fractures and matrix block, which is very important not only for fluid flow, but also for diffusion. This is a continuum model, and we determine parameters based on calibrations against all the data.

Now, I'm going to just show you a few examples of calibrations just for confidence building. These are the main data we use in the one dimensional calibrations. They are the saturations, water potentials, and pneumatic data from all boreholes. We simultaneously invert for all of the parameters, all of the layers in all of the boreholes simultaneously. It's not independent one borehole to another. All of the data are simultaneously inverted to get the best estimates of the parameters, plus we also get all this statistical information about (a) how important are the parameters, for example fracture permeabilities are extremely important, and they are very, very well constrained by the pneumatic data.

The fracture alpha parameter is also very important, and it's constrained from it by the saturation data, but not as much as were some of the fracture permeabilities. The inversions also tell us which parameters are not important at all, which is just as useful information as which parameters are important.

Then we do three dimensional calibrations where we go beyond these datasets and incorporate geochemistry, the chlorides, the strontium, the calcites and temperatures. And

I'm going to show that to you next.

This is the geochemistry calibration of the UZ model. The main datasets we use are shown on the right-hand side, and most of this data comes from Los Alamos or U.S. Geological Survey. You have the total chlorides shown on the top here. You have the calcites, this is the WT-24 borehole, and you have the strontium signatures here. The points are generally upserve data. The lines are model results that are fitting the data, so you can see when you have a good fit and when you don't have such a good fit.

Without going into details, of course the calibration against the geochemistry provides confidence in (a) that the flow patterns are about right, and (b) that the velocities that we use are about right. Velocities, of course, are key to travel times.

Percolation flux was one of the emphasis of the Board. On their question, they mentioned specifically percolation flux. The percolation flux comes directly from the infiltration map determined by the U.S. Geological Survey. But besides that, we have independent lines for that event that support that analysis. And the two most important ones are shown here. Number one, the temperature gradient gives good constraints on percolation flux, because if you have very high flux, you just have cold temperatures all the way to the bottom. If you have no flux, you have only conduction.

The other datasets which is extremely useful are the total chlorides, because we know the source term at the surface, we know what the concentration in the water starting to go down the mountain is, and, therefore, we can model that and match the chloride variability we see in the mountain. This is at the repository horizon.

You see two curves here. This is one, and the other one is the line here. Basically, the chloride data says your infiltration map is conservative. Where there's low chloride, there are two high infiltration locations where there's things like this that might be too low of percolation flux. So on the average, what this says is you might be over estimating the infiltration at the crest of the mountain. It may not be 30 or 60. Maybe it's close to something like 8 millimeters per year, which is this line. So this is a very good independent way to estimate percolation flux, and it's conservative, based on our current representation.

Now, the UZ model is presented in the UZ flow and transport PMR, which is shown on the right-hand side here. Rev. 1 coming out in June or July will have 27 contributing AMRs, or so. That is used to develop the UZ models and submodels models.

Now, the main models shown on the right-hand side in the schematics are, of course, the climate and infiltration, the calibrated flow properties models, the ambient chemistry model, going into seepage calculations and obstructions, going into thermal hydrological effect, and chemical ceiling models, two transport models and mountain scale thermal hydrology model, most all of which feed directly into total system performance assessment.

Now, let's look directly at breakthrough times. There are two AMRs that give you curves for breakthrough times that you can use to look at. Number one is the radionuclide transport model under ambient conditions, Rev. 1, and UZ flow models and submodels.

Again, the curves I'm going to show you are based on a model that was developed for dose calculations, not for travel times. And, therefore, these are conservative and bounding values. But it's good to use those to get the feeling for what kind of travel times we are talking about.

Important parameters for breakthrough time estimations, I mentioned some of them before. Here are some others. Percolation flux, fracture-matrix flow components, and included in this is fracture-matrix interaction term, fracture saturation of water, fracture porosities. And fracture porosity simply is the fraction of the total volume occupied by the fractures. And it's generally on the order of 1 per cent of fracture volume. Flow through faults, perched water zones, radionuclide transport characteristics, such as matrix diffusion surface areas, such as matrix diffusion coefficients, et cetera.

On the right-hand side, it just shows a perched water body that is in the model, extending from UZ-14 to SD-9, to some of the others, WT-24, and others underneath the repository, close to the zeolitic rocks.

Now, we are finding very important pattern in the flow from the repository to the water table, and that can be seen here. This is a percolation flux map that shows the amount of water flowing vertically in a color scheme. If it is red, there's almost nothing flowing vertically. If it is blue, it's greater than 15 millimeters per year flowing vertically.

Of course you start with the infiltration map that the U.S. Geological Survey developed, and then when we come to this area here, we see we still have a fairly high percolation flux on the crest, as indicated by the infiltration map. But generally, all over the repository horizon, we have some 5 millimeters per year of flux.

When we look at the bottom close to the water table, how is the water distributed as it goes into the water table? You'll see very clear indication of controls of faults. You see here line to the faults and to the Ghost Dance Fault and some of the other faults, the Solitario Canyon Fault. Now we have somewhat lower values of percolation flux locally in the repository horizon, and more flow associated with the faults hitting the water table. Therefore, this must be important for travel time considerations.

Now, here is the first prediction of travel times that I'm going to show, again, based on the UZ PMR, Rev 0, based on conservative bounding values and approximations. And you see three curves here on the right-hand side. You see for mean, high and low present day infiltration. This is for Technetium, and Technetium, KT is equal to zero. That is no sorption.

What you see here is that just taking the mean values, you see travel times on the order of hundreds of years, something like that, if you take a 20 per cent value, or something like that. If you have a higher mean infiltration, it's like a hundred, a little bit more. If you have a low value, it might be 10,000 or more. So it depends very strongly on the infiltration flux.

You also note if you look at the slides again, down here, you see very large effects of fault, which is emphasized in these figures.

So the results, which are summarized on the left-hand side, faults control transport. Fractures are main pathways, except in the Calico Hills vitric, where fracture effects are small. Matrix diffusion and sorption are very important. Colloid transport could also be important for travel time consideration.

This is another graph summarizing also the travel time estimates based on the conservative PMR Rev 0, and it's shown on the right-hand side here. This simply shows for both a sorbing species and a non-sorbing species, this is Neptunium, this is Technetium, or something like that, that the infiltration rate or the average percolation flux linerally affects the look of the 50 per cent breakthrough times.

So if you now look at 50 per cent breakthrough time and our infiltration or percolation flux, it's on the average 5, 6, 10 millimeters per year. You have some--well, this is the sorbing and this is non-sorbing--you have some thousands of years for the 50 per cent travel time for Technetium, and tens of thousands if not 100,000 for the sorbing tracer.

Now we're going to switch gears. I shows you these curves just to give you a feeling for how the breakthrough time looks like, what are the main parameters affecting it, such as infiltration and other things, and how the faults are important for travel time considerations.

We're now going to look at what if we focus our emphasis on travel time and that is the main emphasis on the model, what would we come up with? That is one part of what we call the expected case, or the best case that we are currently working on. Best case, I'm sorry, expected case or the best estimate.

So let's look at some of the data. Let's look at some of the geochemical data that we have. First of all, the total chloride values, the increased Strontium 87 to 86 ratios within the PTn, background Chloride 36 levels all indicate low percolation flux. And there are various analyses that's listed in various AMRs that support this, all independent.

We have done an analysis of total chlorides separately from the Strontium ratios separately from the background Chloride 36, all of which indicate relatively long travel times and low infiltration and percolation flux rates.

The Survey has also done extensive work on the uranium series, uranium disequilibrium, the radiocarbon dating of opal and calcites, where they take samples and they date it sequentially from the surface of the crystals, into the crystals, and they find deposition rates which are very uniform in time, based on their resolution. That means that we have a stable formation and stable percolation flux and a stable growth of the crystals, with an average percolation flux on the order of 2 millimeters per year, which is fairly low.

The stable isotopes, both deuterium and Oxygen 18, from pore waters and gases, gas phase Carbon 14, show ages increasing with depth generally in the TSw, and ages on the order of thousands of years. For example, there's a nice gas profile from UZ-1, Carbon-14 age dating that indicates that the gas is some tens of thousands of years old. Very old.

Finally, and just as importantly, the best water samples we can get are of course from the perched water bodies, because it's more difficult to squeeze the rock, and you also change the chemical composition when you squeeze the rock. In the perched water bodies, we have Carbon 14 age dating, the background Chloride 36 and chlorides, stable isotopes, all of which suggest thousands of years residence time.

In addition to this, which is not done there, is the fact we don't see Tritium in the perched water bodies. That also indicates that this is old water. Another thing which is not done here is the fact that the age of the groundwater below the unsaturated zone is on the order of 10,000 years, or so. And if what most people believe, that the local recharge is a major component to that water, suggests that the groundwater travel times in reality, or the travel times, are on the order of thousands of years. So these are all geochemical evidences.

Now, UZ model refinement, best case estimate, or best estimate case conceptual approach, it's being developed as we speak. We have completed quite a lot of studies that look at the effects of faults, look at the effects of taking fractures out on the Calico Hills and PTn, look at the effects of incorporating geochemical data, look at the effects of using more accurate transport model, et cetera.

We are currently working on the second part of this, which is looking at seepage issues, flow focusing issues, and all in which we are trying to make a more or best estimate for the UZ model.

Now, the following conclusions we have found. The effects of fractures in the vitric units in the Calico Hills formation do not seem to be very important to either overall performance, dose base, nor the travel time considerations.

We also find surprisingly when you look at our plots, that the properties of the faults are not extremely important for either dose or the travel time considerations. And that's, when you first hear that, that's difficult to understand. And the reason for that is quite simple. Based on our current analysis, we are not going to finish this work until April, so this is our current explanation for this, the global flow patterns in the mountain are most dominated by the global geology in the mountain, obviously.

What is very important is where do we have the zeolitic rocks and where do we have the vitric rock of the Calico Hills, what are the properties of the zeolitic and the vitric Prow Pass. We can't forget that either. And why is this important? It's because the zeolitic rocks have permeabilities which is some four or five orders of magnitude lower in the matrix than the vitric rock. Vitric rock is 100 millidarcies. Zeolitic rock is micro-darcies. But there are, of course, fractures in the zeolitic rocks, but all evidence so far suggests that these fractures are not that prevalent, not that important. Permeability, of course, increases with these fractures, but you still have this variability in hydrological properties.

Number two, perched water bodies are found when we have the low permeability rock. Of course, they will not sit on top of the high permeability rock, obviously. So these are major factors there.

So what happens? You have the global geology and you have the perched water bodies, and then you have a fault here. The dipping of these units, let's say we have a vitric unit here where flow is going down, it may dip towards the fault. Now, if the fault is not very permeable to take up water, it will simply build up saturation and flow next to the fault down. That's why we think the hydrological properties don't need to be very accurately determined, because it will just simply fall next to the fault. Our parameters we are looking at closely, like the perched water bodies.

There is not sufficient evidence now currently to conclude conclusively that the perched water bodies we see in UZ-14, SD-9, SD-12, and others, WT-24, are all connected. Our basic model assumes this. We are doing sensitivity studies with the expected case to look at what if they are isolated bodies, how is that going to affect (a) travel times, and (b) dose calculations.

Now, here are some curves that show basically what we currently think are our best estimates for travel times. We have various curves here. You don't need to know the details of all of these curves. But what we are varying here are the perched water models and the diffusion coefficient.

Some of the diffusion coefficients are for Technetium. But for travel times, we are not interested in a specific chemical, because diffusion coefficients vary depending on the molecule of size, because of the size, so the matrix flux, et cetera, and the matrix diffusion.

What our best estimate is that these curves, best estimate, our current groundwater travel time in the UZ, which are if you take 20 per cent, are on the order of thousands of years, something like 3000 years.

Major uncertainties. Certainly percolation flux, net infiltration map, like we have shown before, detailed spatial distribution of properties, especially, of course, below the repository in the Calico Hills and the Prow Pass, radionuclide transport properties in TSw, molecule diffusion coefficient, like we talked before, uncertainties for the geological model, fault distributions, and mineral distribution, vitric versus zeolitic.

Now, how are we dealing with these uncertainties? This is just a part of our regular program, field testing program and modeling program, that we have ongoing. We are trying to minimize and decrease the uncertainties with field testing and associated modeling. And Mark Peters will talk about that tomorrow.

We are looking at collecting additional isotopic data for vitric and zeolitic units to look at geochemistry and transport time. Geochemical evidence is extremely important in this sense. Systematic evaluation of uncertainties of processes and models, and Bill Boyle will talk about this tomorrow. And sensitivity analyses using alternative models. And I'm going to show you some of the field tests that we had done and are doing to reduce uncertainty.

RUNNELLS: Bo, let me interrupt you just for a second to give you a warning that we're approaching the end of your formal presentation time, maybe three or four more minutes, because we started a bit late, but we'll need, you know, plenty of time for questions. So with that in mind, maybe you want to be selective about which of the slides you show us.

BODVARSSON: Okay. This is Alcove 1 test, where we actually had seepage going into Alcove 1 from the surface through the Tiva Canyon. This gives us confidence in our seepage model. And we also have tracer breakthroughs shown here that give us confidence in the matrix diffusion in the radionuclide transport models.

And the main conclusion here is that the model results indicate that matrix diffusion was very, very important for the tracer breakthrough.

This is a similar test that is ongoing now with Alcove 8 and Niche 3, and Mark will talk a little bit more about this tomorrow. This is Alcove 8, and here is Niche 3. And the scale of this test is very favorable, 20 meters, or so, a rather large scale. And, again, we hope from this to get more confidence in our matrix diffusion and travel time predictions.

This is a concept that we are looking at now that we believe will make the travel times that we are estimating currently to be much, much larger, and this is the Shadow Zone concept that we have talked about this briefly before. Many of you know that capillary barrier concept that says most of the water is going to go around the drift, some of it, or 13 per cent of the drift is actually going to seep, which is a major importance to performance.

What we have here underneath the drift in this case is what we call the drift shadow zone, where there is very little water flow, or no water flow. That suggests that any transport in this may be dominated by diffusion. If it is dominated by diffusion, there might be thousands and thousands of years in performance with respect to travel time in those, in this shadow zone.

The other important thing about the shadow zone is with respect to colloids, and it's very important, too. If this is all dry, like it is, and it's even going to be dryer when you heat up the rock around it, if it is dry, and if no or little water is moving around this, then the colloids are forced not to go into the fractures, because there's no water in the fractures, but into the matrix. And in many cases, the colloid, the size of the colloid is too big to go into the matrix. So this may help a lot for the colloids issues, if this proves to be a viable concept.

The other thing we are looking at is discrete fractures. We use a continuum model, but in fact we all believe that flow in the mountain is really through very discrete features, maybe 1 per cent, or much less than that, of the fractures actually flow. So you have features maybe 5 to 10 meters apart that carry most of the water. The effect of this on travel time and for TSPA is being evaluated currently.

The last thing here is lateral flow in the PTn.

Really, Parvis Montezar and Wilson, this is one of their conceptual model ideas. Recent model studies and data suggest that this actually may be more important than we thought and, therefore, we may have less flow than we expected at the repository horizon.

So, summary and conclusions, we believe breakthrough times and analysis in the UZ PMR where you see travel times on the order of hundreds of years, or maybe thousands, is conservative. Currently, we are doing refinements of the UZ models, and we believe that our current estimates of thousands of years is much more realistic. We believe that current and planned field testing will help verify our results and reduce uncertainties.

RUNNELLS: Thank you, Bo. We appreciate the very nice presentation.

Was it deliberate to put two are's in that first bullet? Are are conservative? I mean, was that an effort to emphasize how conservative they are?

BODVARSSON: Yeah, they are conservative, and they are very conservative.

RUNNELLS: Okay. That's what I thought.

BODVARSSON: I thought about putting to the second power here. It's a typo.

RUNNELLS: Okay. I have one quick question before I call on questions from the Board.

It appears to me that the nature of the material in the Calico Hills beneath the repository is important for a lot of reasons. The zeolitic versus the vitric, how far underneath the repository the perched water may extend, lots of different reasons. What are the plans for testing, for identifying, for characterizing those materials in the Calico Hills beneath the repository, beneath the proposed repository?

BODVARSSON: Well, I think the project has already spent a lot of effort analyzing all the cores that we have below the repository, looking at the mineralogy of all of those, and coming up with a mineralogic model which is our best representation based on the available data. In order to get more data from below the repository, obviously you have to have (a) more boreholes, or (b) a tunnel there. And I don't think there is any plans for either one of those. And somebody can correct me if we are starting a tunnel tomorrow. I see a lot of things going this way, the heads, you know, they're all going--

RUNNELLS: I'm going to turn the time over to the other members of the Board for questions. Priscilla?

NELSON: Good afternoon, Bo. Thank you very much.

I've got two questions. The first is the overwhelming perception I have that the travel times that you're talking about, although you want to resist the idea of breaking it down into subsystems and look at the overall mountain, in fact, the overwhelming impression I have is that the delays in breakthrough are caused by the Paint Brush and the Calico Hills predominantly. And I don't know whether you've broken it out, but in fact I think the tuffs above the Paint Brush and the Topopah Springs, these are all highly conductive.

So that's my overwhelming impression, and in fact that's part of the reason why the faults don't really demonstrate any import, because of the phenomenal import of these other two layers. So I'd like you to tell me why that's not a good perception.

And then just secondly, because so much of what you talked about was vertical flow, and your model is geared towards vertical flow predominantly in the unsaturated zone. You had one slide there about horizontal flow coming through the PTn, it might be important, but I'm not sure what that means, but the possibility of also getting flow into the Topopah Springs out of Solitario Canyon, which would also be a horizontal flow coming in is raised again, at least in my mind.

So there's two questions. Do we have a tin roof and a tin floor now, back to the old days of what we were thinking about? And what about horizontal flow and horizontal recharge?

BODVARSSON: These are good questions. Let me answer the first one first, and if I understand it correctly, that relates to the separate contributions for the different barriers to a travel time dose, or whatever.

The interesting thing is if you take TSPA/VA, and you look at the dose, the importance of the different units below the repository, you find actually TSw is the primary retardation unit for neptunium, for example, that retards because of matrix diffusion. And I think it's still true that that's the case, although they are fairly equal in value, the TSw, the vitric Calico Hills is also fairly good, but the KT is lower there. There's only one versus four. Or it used to be like that, one or four. Maybe it's lower now.

With respect to travel time, the PTn, as you said, is exactly right. There's thousands of years travel time through the PTn, obviously, because it's a porous medium material. The same thing with the vitric Calico Hills. There's thousands of years through it, because it's 40 per cent, and it just takes a long time to move through it.

There is also significant contributions from both TSw, because of matrix diffusion, and the zeolitic rocks in the north.

With respect to your second question, and that was on the Solitario Canyon, it's a very valid point. We do not have sufficient data to rule out in flow from Solitario

Canyon, horizontal flowing through the repository horizon from there. So that's an open question.

RUNNELLS: Dan Bullen, and then Debra.

BULLEN: Bullen, Board. I guess I should comment on your overall presentation, because I think the more I hear you speak about this, the more I begin to understand, and I think it probably has a lot to do with that Berkeley accident that you have.

But I have a couple of questions about--could you go to Slide 18, please?

BODVARSSON: Icelanders in Berkeley?

BULLEN: No, no, it's got to be the Berkeley accident.

When you're taking a look at the curves that are at the top for the high, mean and low transport of Technetium 99, for example, you mentioned that one of the problems that you're running into is the change for climate change. So when the climate change happens, and I think if you go to the Slide 17, which is just the immediate predecessor to that one, where you take a look at these infiltration rates and the distribution of the percolation flux, how would you expect it to change if, you know, say tomorrow a super-pluvial kicks in and we're raining all over the mountain? Can your model handle the changing climate? And how would it change the distributions that you ended up with?

BODVARSSON: That's a good point. The answer is the model handles climate in the following fashion. We developed three dimensional flow fields for TSPA for use in transport calculations, et cetera, for percolation flux, for seepage calculations, et cetera. We developed 3D flow fields for all climate states, going from modern to glacial to transition, whatever. They are all there. So it's all included. So we have--in the PMR that have all the different climate states.

The only thing we are sure is that there is a sharp transition from one to the other. Like after 600 years, we change, and then on and on. And I personally don't believe that's a very--I think it's a good assumption.

BULLEN: Okay. One more quick question, and that was with respect to the shadow zone, which I think was Figure 29. It's a very interesting phenomenon. I guess the question that I have is how many tunnel diameters do you expect the zone to actually exist as it goes down? Because obviously there's some sort of dispersion as it travels. And is there any experimental evidence that this really exists, or do you have to test it, you're going to test for it?

BODVARSSON: It's a very good question. The answer is the following. There is some analytical solution by Phillips that is basically concentrated on, of course, the tunnel has a capillary barrier. At the same time, since was an analytical formulation, he gets a solution of everything in the domain, based on "approximations." If I remember correctly, of course this shadow zone is going to get smaller and smaller, but it extends diameters down, like if I remember correctly, three to five diameters down, but it becomes smaller and smaller.

Now, with this heterogeneous factor system that we have here, we would expect that to be lower at Yucca Mountain, but not a lot lower.

Finally, you know with just an invert that is less than a meter, with diffusion processes just in the invert makes a huge difference. So just having a few meters may make a huge difference.

BULLEN: Is there a test for this?

BODVARSSON: No, what the plan is, or DOE is considering, I think that's the right word, in front of the NRC KTI meetings that I've learned, DOE is considering a replan model evaluation of this concept and some laboratory tests on this concept to see if it is viable, and then we move forward. Is that clear for you?

BULLEN: Thank you.

RUNNELLS: Debra?

KNOPMAN: Knopman, Board. I have a lot of questions, Bo. Let me try to just focus in on one right now.

I'm trying to put together what you said during the presentation about maybe relative insensitivities of your model to actual estimates of transport times, breakthrough curves, and then your strategies to address uncertainties in the UZ model. And I guess it would help me if you could recap what you think are the, let's say, soft spots or the points of greatest sensitivity in the model where you feel that you have insufficient data, and testing would be most useful, distinct from what might actually be going on now in terms of testing or plans for it. I just want to understand what insight you have gotten from your model as to what you need in the way of additional information about the system.

BODVARSSON: A clarifying question to you. The answer depends on what the question is. For example, I'll answer differently for different aspects of the UZ model. If you're asking me this question in terms of coupled processes, I'll give you an Answer A. In terms of travel times, I'll give you B. In terms of seepage, I'll give you C. So which one do you want me to answer?

KNOPMAN: Well, I was going to ask about coupled processes anyhow. Let's start with coupled processes. But I'd like to hear it for each one of those.

BODVARSSON: Okay. Well, let's, if we can, start with travel times. Can we just start with travel times?

KNOPMAN: Okay.

BODVARSSON: And then we'll go to coupled processes.

Like I said before, one of the slides, and I don't remember which one, in order of importance with respect to travel times, there is (a) the geological structure, global geological structure, very important, (b) the perched water bodies, (c) we get into parameters. The first parameter would be fracture porosity. We need more measurements of fracture porosity, and that can be easily obtained by concentration dose. B, fracture saturations. We have no information on fracture saturation, and we don't even have a clue how to get it. Those are the main things with respect to travel time.

With respect to coupled processes, let's start with TH processes. If you take thermohydrological processes, one of the comments by the Board was where does the water go? Will it seep back in? That's an open question, and the cross-drift test will help us start to address that. But we are also doing a lot of model studies that--I'll go back to Priscilla's question to Bob Andrews earlier, what do we have for SR. That stochastic variability in the seepage, thermally into seepage, and the drainage, and that we will have a TSPA/SR. I think those models will help us. Do we need any more testing besides maybe the cross-drift thermal testing.

With respect to THC processes, we have two things that come to mind. A, we have a fracture that sealed up in two weeks in a lab test, based on water that ran through a TSw core has the chemical signature or TSw core, and moved through the fracture and precipitated calcite and silica through the SM and sealed up in two weeks.

B, we have a THC model that says you don't have to worry so much about chemical ceiling. These are two kind of end members in a sense. So what the project is doing about that, which I think is the right approach, is to apply the model to the fracture data to see if the current model just would do that, too. Because there is no other fracture to get the water out. It has to seep. And if that's the case, we don't really need a lot of testing. But also the plan is in--the replan is to supplement it by a test of multiple fractures. And I think that will really take care of the THC issue, at least in my mind.

With respect to THM, the drift scale test has shown little effect of TSM permeability changes based on past permeability measurements. So I'm not sure personally if you need a lot more.

The final thing was seepage. The concern of the NRC of evaporation processes during seepage testing is a very, very good one, and the project is looking into it. Every test we do we have evaporation pans now to make sure we capture that water.

The other thing that DOE has decided to do is to do a mass balance on the seepage testing, which I think is an excellent idea, to make sure that the water goes where we think it goes, around them instead of somewhere else. So I think the project's plan, as is in the replan now, is basically what I would personally think--I think we are, in summary, I think we are heading the biggest part of what we need.

The exception, obviously, is you can never have a very detailed model of things below the repository unless you have a lot more boreholes, and it may not be cost effective to have a lot more boreholes. Is that fair?

RUNNELLS: Richard Parizek?

PARIZEK: Bo, again I enjoyed your presentation because the rocks come back in as giving us some protection, or serve a valuable role. And if it's a thousand years mean, or for several thousands of years, now you're buying the program a lot of good and, therefore, it pays to spend some more money justifying that or proving it.

BODVARSSON: Well said.

PARIZEK: If you said 100 or 200 years, then everybody would let you go home. But if it's 2000 or 3000 or more years, then you definitely are entitled to demonstrate that shadow zone. And a little model in the lab, you know, of some little sand box, would give you kind of a sense that maybe there is such a shadow. But really a field test of that is in order, and the field test has to be at a site where the rocks are so like Yucca Mountain rocks, and so this idea of the tunnel, the magical tunnel somewhere where that could be demonstrated, because that's more than 2000 or 3000 years. You're saying that that diffusion barrier could be worth 3000 or 4000 years by itself. So you could maybe buy the program 6000 years, and have double the money for your efforts. Has the program given serious thought to this--

RUNNELLS: Well, said, Richard.

PARIZEK: Well, I mean, it's science that's going to pay if it's in the 3000 year category.

BODVARSSON: Well, let me--there is a presentation coming up tomorrow afternoon by Russ Dyer about how quickly does the project change decisions. Is that correct? What is it called, Russ?

This is a very good example, because, I mean, I actually, this concept of the shadow zone basically came up very recently because of the diffusion characteristics of the invert, which is only half a meter thick. I mean, realize then if this zone would be much larger, like a shadow zone, it would buy us a lot of stuff. And I think very quickly, DOE has decided to investigate this through model exercises. We haven't decided on a field test, because number one, a field test is very difficult to do with this concept, because diffusion to stop the flow around the drift, and a shadow zone, to me, would take a thousand years, and we just--I mean, I'm getting close to retirement already.

Therefore, the only thing I can think of is, number one, do a lab test that can give us this and scale it up. But the most important thing would be natural analogs again, and maybe that's your--what you are suggesting.

PARIZEK: Existing tunnels that are long-standing tunnels.

BODVARSSON: Yeah, like what John Stuckless has been looking at, caves and stuff like that, and do boreholes down around them, look at the Carbon 14, 18 in the shadow zone, and see if it is thousands of years old, look at the chloride distributions and the chemicals around it, construct the model and convince yourselves this is a viable concept.

PARIZEK: That's the concept I'm after.

Now, there's some inconsistences in your presentation. It may not be inconsistent, but they come out looking that way.

BODVARSSON: Okay.

RUNNELLS: I have to interrupt just for a second.

We have about two minutes left.

PARIZEK: I'll talk faster. Page 16 shows a perched water body which is quite large. Page 23 suggests that maybe it's not one big perched water body, but a group of smaller ones, as you suggested. You have a pneumatic test that shows that faults are permeable. You have a diagram that shows that faults drain. That's from the roof in the PTn. I'd kind of like to know why the PTn is back in there, what new observations you have for that.

On the other hand, then you also have perched water bodies perched against faults, suggesting they're not permeable. So faults are sometimes not permeable to help perched water on the down-dip side, and sometimes they are permeable, as seen by the PTn drain, as well as the pneumatic test. So how can they be both things?

BODVARSSON: Okay, I think maybe my presentation wasn't as good, was not good, but I think this is all consistent. A, the perched water body close to SD-7 is next to the Ghost Dance Fault. Clearly the Ghost Dance Fault is impermeable there because the water body is only like ten meters, twenty meters in extent, and it still stays there.

B, all of the pneumatic data indicate that faults are very permeable on a global scale. It doesn't mean on a local scale like at SD-7, you can't have local perched water bodies. C, the perched water bodies, just like I mentioned, can be either one body, because the geochemical signatures are similar, or they can be separate bodies. Therefore, we are just doing sensitivity studies to evaluate which--how it affects travel times and dose, and we are not saying that we believe that exclusively we think it's one body or many bodies.

PARIZEK: Okay. Now, perched water does suggest, though, that maybe the Calico Hills does have some low permeability zones, even though we don't have much data, direct observations on it, perched water suggests that it's not zeolites?

BODVARSSON: Yeah.

PARIZEK: Where you really don't know whether it has or not.

BODVARSSON: Yeah. The zeolites are fairly tight, but there's not much fracturing in that location.

RUNNELLS: We have to stop. We just have to stop to give the next speaker a chance.

Thanks, Bo. I know staff members had questions, and perhaps they'll have a chance to grab you and continue these conversations. Sorry, Richard, but we just have to stop. Thanks, Bo. It was a very nice presentation.

Our next speaker is Dr. Al Eddebbarh, and I hope I didn't slaughter that name too badly. Was I close?

EDDEBBARH: You did very well.

RUNNELLS: Oh, thank you very much. From Los Alamos. He is the lead on the saturated zone studies, responsible for saturated zone flow and transport models. And the question is coming up as we speak. Dan Metlay will put it up. And then in just a few moments, we'll take that question down so you won't be distracted.

Dr. Eddebbarh, please proceed.

EDDEBBARH: Thank you. Good afternoon.

I see we have some technical difficulties with the projection system. I'll probably start with the question here. (See Question 3 in its entirety in the Index.)

The question of the SZ that the Board had put before us is what is the mean and variance of travel time of a conservative species from the water table below the potential repository to the accessible environment? And how did we arrive at this answer, and include a discussion on the specific discharge, which is a most important parameter in that process? And what independent lines of evidence corroborate the estimate of travel time in the saturated zone? And what are the sources of uncertainty in these estimates? And how much this difference is, or how much uncertainties will make in terms of differences?

I would like to start with a brief summary of our answers to the question at hand. The TSPA/SR which was completed last march has a lot of conservative assumptions, as Bo has signalled. And the estimation of the mean travel time, which is the breakthrough time of 50th percentile is about 640 years, with a variance of one order of magnitude each way. And that breakthrough time is arrived through using median parameter values, and I will explain what we mean by the median parameter values later on.

But using the mean parameter values, that breakthrough time is about 900 years. And since the development of the TSPA/SR CR, we have acquired more data, and also we have acquired a better understanding of the processes and the concept. And using that current data, and that current state of knowledge, we developed a refined approach, as Bo called it a little while ago, the best estimate case. And that best estimate case mean breakthrough time is about 1300 years.

Now, the source of uncertainties in the Carbon 14 transportation, and I would like to mention here again what Bo has said before, that a conservative species means a species that's going to travel with the velocity of the groundwater particles. It's not going to go any other processes like sorption or dispersion, or what have you.

And the sources of uncertainties in the Carbon transport times are specific discharge, which I will show later on in the discussion, that it is the most sensitive parameter. And also parameters associated with the alluvial tuff transition zone, and I will show later in this presentation the water table, transition from being in the tuff, organic tuffs, into being in the alluvium. And we had uncertainty related to the location of that transition zone, and that's the second most sensitive parameter.

Then we have flowing interval fractures, and we also have the effective diffusion coefficient as sensitive parameters.

Now, how are the parameter variabilities handled in the TSPA? In TSPA, the parameters or the variabilities in these parameters is handled stochastically. And as I mentioned before, the specific discharge is the most important parameter.

We have used geochemical and hydrochemical evidence and also natural and anthropogenic analogs to corroborate the result which we obtained through our models. The program is also conducting an organic Carbon 14 study to determine groundwater ages. And also, we believe that new data and revisions to models and model parameters will yield a slower expected breakthrough time.

The general approach to answering the Board's question, how would we arrive at the answers, it's first of all we looked at the existing TSPA SRCR. The TSPA was completed last March, and I will cover the salient aspects of that TSPA SRCR SZ analysis. I will be talking about the calibrated steady-state flow field, which is used as the backbone of the SZ flow and transport modeling. I will talk about the transport calculations using the particle tracking approach to minimize dispersion, which is inherent into finance, difference of element methods. I will talk about the stochastic treatment of uncertain parameters, and also I will talk about how the parameter uncertainty consideration and analysis, and I will also talk about ongoing programs to reduce the uncertainties in--and these programs are field and lab testing that are ongoing. And I will also talk about the effects of new data and modeling assumptions on the system performance.

I would like to step back and just cover some basic concepts of migration in the SZ zone. The saturated zone is the last barrier in a defense-in-depth system, and it does so by delaying migration of radionuclides, and also by introducing concentrations at the accessible environment. As elements or radionuclide reach the water table, they are transported down gradient by the groundwater flow velocity, and they're also undergoing several processes, such as matrix diffusion, dispersion, and sorption.

How did we gain our understanding of the behavior of the saturated zone? I would like to step back and cover some regional conceptualizations.

The Yucca Mountain and its surrounding areas are part of the Death Valley regional flow system. And that regional system is characterized by the upper aquifers, which are the volcanic tuffs and the alluvium, and also the lower aquifer carbonate, which is composed by the carbonate aquifers, or the carbonate rocks.

The recharge, as we're going to see in the next slide, at the regional scale happens in high altitude areas, up in the mountains, and also intermittently in washes, like Forty Mile Wash, and the discharge are by evapotranspirations in the different flats. And we will cover that in the next slides. And basically, the regional potentiometric surface or the regional understanding with the recharge area and the discharge area allow us to have a general idea on the flow at a regional system, and also at the subregional system. This framework here, this slide shows the recharge area. The Chocolate Mountain, the Timber Mountain, Pahute Mesa, Shoshone Mountains, and the Calico Hills.

Some of the regional evapotranspiration area include Craters Playa, somewhere around there, Franklin Lake Playa, Ash Meadows and Death Valley and Furnace Creek.

I would like to talk about the regional model which is used by Yucca Mountain to establish or to derive the boundary conditions for the site scale model. The figures that they showed before were borrowed from the 1997 regional model which was developed by the United States Geological Survey, and they would like to add that in addition to DOE, Yucca Mountain and the Nevada Test Site, there are other stakeholders of that regional model, and those are federal stakeholders like the Fish and Wildlife, the Park Service, and also state and local stakeholders, including Nye County and Inyo County.

The USGS is about to release a refinement of the

1997 model, and some Board members have seen first-hand the progress that was made with the regional model, and the project will use the current regional model, which is refined from the old 1997 model, and derive boundary conditions and see how those boundary conditions will impact the analysis that was done with the site scale model, using the 1997 model.

At the local level, transport of radionuclide in the SZ is expected to occur from beneath the potential repository to the southeast towards Forty-Mile Wash, and then south approximately parallel to Forty Mile Wash, and into the Amargosa Valley.

Now, I would like to cover some basic concepts of transport in the SZ, in the saturated zone. As the potential radionuclides reach the water table, they are going to be transported by advection, and we assume that advective transport occurs only in the fractures. In the SZ, we don't think take any credit for advective transport in the matrix. So we use a single continuum with a single permeability, and that is a permeability of the fractures.

As you have transported in the fractures, radionuclides are allowed to diffuse into the matrix through matrix diffusions, and once they are in the matrix, they're allowed to sorb into the matrix. We do not account for any sorption in the fractures.

And also, as they are transported, radionuclides are allowed to disperse in the three directions of the flow, the longitudinal dispersion, the transverse vertical and horizontal.

Down the road, and before they reach the 20 kilometer compliance boundary, the radionuclides which are transported close to the water table, change from being transported in the volcanic tuffs, into being in the alluvium. And as I said before, we have a certain amount of uncertainty related to that transition zone, and the Nye County Program is helping us reduce this uncertainty.

Basically, this conceptual understanding of flow and transport below Yucca Mountain is fed into a numerical model, which uses FEHM, a finite element method as the numerical code to build a numerical flow and transport for the site. That numerical model covers an area of 30 kilometers by 45 kilometers, and it goes as deep as 2750 meters below the water table. And that's a depth that's a coincidence with the depth of the regional model, with the vertical extent of the regional model.

The hydrogeologic framework model, which is the backbone of the site scale flow and transport model, contains 19 units, 19 geologic units, with different properties and different attributes. And basically, that hydrogeological framework model is developed by the USGS, and we'll take that model with all the information it has with all the geologic units, and grid it into our flow and transport model.

The model uses an orthogonal grid of 500 meters spacing, and variable resolution in the vertical directions. Our resolution in the vertical directions start with a grid size of 10 meters, and it goes down below as close as 500 meters, because the transport will occur close to the surface because of the upward gradient that keeps the flow paths from below the mountains at the water table at the surface.

And, by the way, the processes that are included in the site scale flow and transport model are processes that were verified through field and lab testing.

The flow model calibration is used to obtain the best parameter estimates of hydrolic conductivities and other model parameters. The model calibration and validation use water level measurements in wells, and I will show later on a map that shows all the wells that are included in the monitoring program, and which provide water level data for the model calibrations.

We use simulated groundwater fluxes at lateral boundaries, and as I mentioned before, those boundary fluxes are extracted from the regional model, because the regional model is a closed system. It has natural boundaries and it has control over the discharge and recharge within the closed system.

We also use inferred flow paths derived from hydrochemical and isotope analysis, and I will show a slide to that effect. And also, we use and duplicate the upward hydraulic gradient caused by the high water level in the carbonate aquifer, and we use the ranges of permeabilities from different testing.

At Yucca Mountain, we have more than single and multiple well tests, hydraulic testing, that's yielded permeabilities, and these permeabilities are used to constrain the model calibration. We also use average specific discharge in volcanic aquifer, which is derived from the expert elicitation panel.

To obtain conservative species breakthrough time, we use a site scale flow and transport model to simulate breakthrough times at 20 kilometer boundaries. We use a 3-D advective dispersive particle tracking to generate transport breakthrough curves. And also use local velocity from FEHM flow model, and we used a dispersion sensor to simulate the dispersion process, and we also used the analytical matrix diffusion as documented by Sudicky and Frind in 1982.

This slide here shows the mapping of the different faults and fractures in the site model domain. And basically, all known fractures and faults are directly input into the hydro-framework model, and they are represented into the numerical model with different hydraulic properties than the rest of the model domain, and also with high anisotropic ratio. So basically, this is what we will call later on the base case.

When we start doing the comparison with the anisotropic case, and basically the base case represents in it the fast flow and features, faults and fractures, and also has an anisotropical ratio to enhance flow in the direction of faults and fractures.

The well data that's used for the inverse calibration of the flow model includes 115 water level measurements, and these water level measurements include 18 new data points, which consist of the 18 Nye County wells that have been drilled so far in Phase I and Phase II, and I believe tomorrow, Mark Peters will be talking about, in his update, about the ongoing Phase III Nye County Drilling. And he's also going to be talking about the ongoing ATC alluvial testing complex activities, and I will touch a little bit on them later in this presentation.

Basically, the particle tracking method is used because the model domain covered by the site scale is 30 by 45 kilometers, and the grid size is 500 meters by 500 meters. And as we know, if we use direct finite elements for the transport process, we will have numerical dispersions, and we'll also have difficulties representing small source terms at the water table, small source terms which reflect the failure of a single package or similar things.

The result of the breakthrough curves are obtained at 20 kilometers, and then for each breakthrough to construct a breakthrough curve, we use 1000 particles that are put at the source, at one source, and they're allowed to travel to 20 kilometers compliance boundary. And I will show later on an animation that will show the transport of this 1000 particles, and the different arrival times for each particle reflect the variance in the breakthrough time, and also reflect the processes. I mean, some particles will travel at the speed of groundwater. Others are going to undergo matrix diffusions. Others are going to disperse.

This is a brief animation that will show the different regions of hydraulic properties at the site scale level, and also it will show the difference between the breakthrough of a conservative species as opposed to a species that will react or will absorb in the matrix or in the alluvium. And also, I think the most important aspect is it shows that conservatively transport in the fractures happens very, very, very fast. And the red particles here, as we're going to see, represent Carbon 14, which is a conservative species. And the green one is Neptunium.

And as you see, in the fracture tuff, there is very little difference between the conservative species and the reactive species, as both of them are travelling in the fractures at very high conservative velocity. And as we get into the alluvium, some of the reactive particles will sorb into the alluvium material that's slowing the breakthrough time. The average travel times represented here is the arrival time or the breakthrough time for the 50 per cent of the 1000 particles.

By the way, this is a good picture of the mountain with the compliance boundary.

The uncertainties in the SZ flow and transport for conservative species. As I mentioned before, the most sensitive parameter is the specific discharge. And for specific discharge, the general approach to the SZ flow and transport abstraction is the use of the flow and transport site scale model, and then we use four sources for the region below the repository to simulate, to start the simulation of transport, and the particle tracking is used to generate transport breakthrough curves, and we use the calibrated steady state flow field under current conditions.

The TSPA simulates the change in climates. After 600 years, we have a transitional climate. And after I think 10,000 years, we have a super-pluvial climate.

This slide shows the four regions for the source. And, of course, for the cold design, the source region will be expanded to cover the footprint of the potential repository.

The uncertainties in the SZ flow and transport include the specific discharge, and for the specific discharge, we used three values, a low value, a medium value, and a high value. Also, anisotrophy, two discrete cases are used in the simulations. The base case is the case I covered before. I described it before where we used the hydrogeologic model, and where we explicitly represent the fractures and faults, and gave them their own permeabilities, which are higher than the rest of the model domain, and also they have porosities or effective porosities that are higher than the rest of the model domain. And also, we gave them an anisotrophy ratio to enhance flow along the fractures, along the faults.

As I said before, the alluvial uncertainty zone, which is the zone where the water table transitioned from being in the volcanic tuffs, into the alluvium. This is also a very sensitive parameter. And variability and uncertainty is treated in TSPA stochastically. And the parameters that are treated stochastically in TSPA are the flowing interval spacing, the effective diffusion coefficient in the fractures, and the flowing interval porosity, which together with the permeability of the fractures give us the seepage flux, or the advective velocity, and then also the effective porosity in the alluvium, the dispersivities, and also the source location. And that's why in the TSPA we use the four source regions.

This slide shows the uncertainty zone of the transition from the volcanic tuffs into the alluvium, and we in TSPA SRCR, this transition zone varied from like this point here, which basically results in an alluvial part of the 20 kilometer of one kilometer, and we varied this all the way to nine kilometers. And then east/west we varied it all the way to Forty Mile Wash. This point here is 19-D, and 19-D has 600 feet of saturated alluvium in it.

So we are hoping that with Nye County Phase III, which is going to start in a couple months, we will be able to reduce the uncertainty here. Nye County is planning to put two wells north of 19-D, and this was R-20D and 22-F.

This slide shows the distribution of the specific discharge used in the TSPA and the performance analysis. As I said, we used three discrete cases for the SZ site scale model. This is a low flow case. This is a medium flow case. And this is a high flow case. And this just shows the probabilities of the fluxes.

I think this is what we have been waiting for. This is a breakthrough curve for Carbon 14, which is a conservative species, and this breakthrough curve was generated using median values for the parameters. First, we established a range for the parameters. Then we estimated the median, and then we used median to generate the median breakthrough curve.

So this breakthrough curve represents--is constructed by plotting the arrival time of the 1000 particles that were released at one location, and the breakthrough curve here reflects the different processes that a single particle will undergo before it arrives at the 20 kilometer boundary.

Now, what I showed before is a single breakthrough curve developed using mean values for the different parameters. If you take each parameter and develop the range of that parameter, and if you sample values from each range, you end up with a collection of breakthrough curves, in this case 100 breakthrough curves that represent the uncertainty in all the parameters used in the site scale flow and transport model, and TSPA takes just 100 curves and samples from them to incorporate the performance of the SZ in the total system performance assessment.

Now, if you take the median of each breakthrough curve, and what I mean by the median is the arrival or the breakthrough time of the 50th percentile, and plot it in a histogram, you have this distribution here. And if we can analyze this histogram, we find that the histogram has three modes. This mode which corresponds to a very low specific discharge, because the Board had asked specifically how the specific discharge, how sensitive the results are to specific discharge, and how specific discharge is handled. And this is the median value for the specific discharge, and this is the low value for the specific discharge.

And as you can see here with the low value of the specific discharge, the breakthrough times are in the order of tens of thousands of years. And we are in the process of refining that variability or that range of the specific discharge. Right now, we use a range of one order of magnitude, and we're going to be able, with the new data from the Nye County and also with going back to the C-well data and analyze it, we are going to be able to use the range from one order of magnitude into three times, and divide them by three.

RUNNELLS: I just have to warn you that you're just about out of time, about three more minutes.

EDDEBBARH: Okay, I think it will go quick.

Okay, this just shows the result of the sensitivity analysis. And as I mentioned before, the most sensitive parameter is specific discharge, followed by the uncertainty zone. And we are in the process of reducing uncertainties of these two parameters.

I, just like Bo has mentioned before, the current process models for which I have presented the breakthrough curves were developed primarily for TSPA and the evaluation of dose. In their current form, they have a lot of conservatism and aspects that will lead to conservative breakthrough times.

Now, I would like to cover very briefly the best estimate case, which is based on new available data and more current understanding. And basically, the basis for that is we use new available data available to us after the completion of the TSPA SR, and we used that new understanding and the new data to run the models with the new values, and also we validated the model results.

The parameters involved are effective diffusion, specific discharge, effective porosity, flowing interval spacing, et cetera.

And this breakthrough curve shows the difference between the analysis that was done or completed and documented in the SZ PMR and was completed in March of 2000, and some preliminary results of breakthrough curves using the new data and the refined best estimates. And we can see that for the 50th percentile here, we have the travel times are double of what we had before.

Now, independent lines of evidence. The travel paths that are predicted by the model were constrained by travel paths inferred from hydrochemistry and from isotope analysis. The Carbon data from boreholes downstream from the repository are consistent with the breakthrough curves predicted by the site scale model.

Observed Carbon 14 activities at the new Nye County wells, which is probably at the 20 kilometer fence is consistent with the distribution of breakthrough time for combined UZ and SZ flow predicted for the best estimate.

And we did mixing calculations which yielding 2 to 16 per cent of the water downstream to have younger ages. And by young, we mean here less than 1000 years old.

This small portion of young water is qualitatively in agreement with the breakthrough curve that was presented. I mean, if you look at this 2 to 16 per cent and you examine the breakthrough curves, if you go to the breakthrough curve time corresponding, you will find it is consistent with this.

Just continuing with the independent line of evidence for the breakthrough times, the Carbon 14 ages, and collected one, indicate that the waters in the area is 12,000 to 18,000 years old, and that age is indicative of not very significant recharge in the area.

Also, the Redox potential is indicative of also low recharge, and basically to save time, all this evidence here is consistent with flow fluxes or flow travel time in the SZ.

This slide just shows the red part here of the flow path is the one predicted by the site scale model, and the light blue one are different chemicals, different isotopes to kind of concentrations, to kind of constrain the travel paths from below the repository to the compliance boundary.

Tomorrow, Bill Boyle will talk about the uncertainties, and these are some of the parameters in the SZ that Bill will talk about.

This is the last slide, and it's just a slide that shows the different activities ongoing at the Alluvial Testing Complex. As we speak, all the hydraulic testing is completed, and as we speak, two of the three planned single well tracer tests have been completed. The third one, the injection part is completed, and we are in the shut-off period, and in 30 days, we will start pumping back the tracer. And the remaining ATC injection wells and also the remaining Nye County wells will be installed starting in May. And the cross-hole testing for the hydraulic hole test in the cross-hole, and also the tracer testing will be starting at the end of FY01 and continue into FY03.

And that's the last slide I have.

RUNNELLS: Very good. Thank you very much.

Just one quick question from me. I missed I guess when you pointed out where the ATC is on a map. I'm not sure where that location is. Could you show us maybe on Slide 18?

EDDEBBARH: If you go back to the uncertainty zone slide? Mark Peters tomorrow will cover in detail the ATC testing. I will be very glad to show you the location, but if you would like to have more detail, Mark Peters is going to cover that tomorrow.

RUNNELLS: Just point on the map--

EDDEBBARH: Basically around here.

RUNNELLS: All right, thank you very much.

Questions from the Board? Debra?

KNOPMAN: Knopman, Board. Al, you didn't talk a lot about dispersion. You talked about diffusion and you talked about specific discharge. What now is the current thinking? I mean, it looks like you're not assuming very much dispersion at all. It looks like fairly focused flow paths once the plume hits Forty Mile Wash. On what evidence are you basing that assumption, or is that incorrect?

EDDEBBARH: Right now, the project takes very little credit for dispersion, because all of the mass that crosses the compliance boundary is divided into the critical group volume. So all the mass that crosses the compliance fence is divided into that volume. So that gives little or no importance to dispersion. But the process models that we use are built to deal with dispersion, and also some of the testing that we are doing at the ATC have some elements in them to help us derive estimates of dispersion.

Now, the longitudinal dispersion is going to affect the breakthrough time, and we have values that were derived from the C-well testing that we are currently using, and as I said, we are in the process of, through the Alluvial Testing Complex, of deriving some field estimates of longitudinal and hopefully transverse dispersion.

RUNNELLS: Other questions from Board members? Richard?

PARIZEK: Parizek, Board. You did indicate climate states were changed in the model? I think you said that.

EDDEBBARH: Yes, the different climate change occurred at 600 years, and that's the transitional climate. And then at 10,000 years, and that's the super-pluvial climate.

PARIZEK: So, again, the program has gained a lot of ground from the modeling exercises in the saturated zone. I mean, everything--and you will revise the regional model input boundaries, because right now, the fluxes that you use are the old fluxes from the three layer model, but that's to be updated, as you indicated. So we'll have the full benefit of the regional model updates going into your boundary conditions or flux boundaries?

EDDEBBARH: Yeah, that's correct. I think the USGS is planning to release the regional model within the next few weeks or few months, and we will take the regional model, we will extract boundary fluxes. And I think the first step is to compare those fluxes with what we used before, and if they are different, put them into the site scale model and see how that affects the calibration. If there is no effect, we'll just document that.

PARIZEK: And there's also a grid orientation question, whether that's going to be resolved for the next round of modeling. The regional model grid orientation is parallel to your grid orientation?

EDDEBBARH: Yeah, that's a very important question that we tackled. I mean, first of all, we had to orient our grids similar to the regional model. Otherwise, we would have a lot of problems, you know, using the boundary fluxes from the regional model.

And then second, we didn't find a particular orientation that will be pertinent for the whole model domain, because the factors have different orientations. So what we are doing, we are doing some analysis to identify or assess the impact of the grid orientation on the flow fields and on transport breakthrough.

PARIZEK: One other question. The different paths always want to head southeastward into the Forty Mile Wash. What keeps it going that way? I mean, it could go straight south, but for the moment, it's going southeastward and hits the alluvium quicker, and that's good for the program if that's what it does. But is there any new evidence to say that it really is going to go to the southeast and then south, or come straight south, as Linda Lehman has suggested at one time or another at these Board meetings?

EDDEBBARH: Right now, we're in the process of, and this was the result of some of the KTI meetings, I think the one that you attended in Albuquerque, the NRC has suggested that we use some features, and we are in the process of completing the analysis to see the impact of these features on the flow direction. And basically, during the calibration process, we eliminated a lot of conceptual models that--I mean, including the one that goes straight. And I think one of the problems that were conceived before is the anisotrophy problem.

And as I explained in the presentation, we represent the known faults and features in the model, and we give those features high hydraulic conductivities, low effective porosities, and also we gave them a high anisotrophy issue, sometimes as much as 50, in the direction of flow, enhanced flow in that direction. I mean, it's an issue that we're taking very seriously. I mean, when you add the five to one anisotrophy in TSPA, that puts, you know, the flow directly south, and we'll also examine very carefully other independent lines of evidence, such as the hydrochemistry.

And then as you saw, you know, the flow paths inferred from hydrochemistry are pretty much doing the same thing, you know, back east to Forty Mile Wash, and then south. And if you look at the regional potentiometric surface, the arrow that I showed before, that's also indicated because of the large gradient to the north, and also the moderate hydraulic gradient to the west favors, you know, that flow direction.

PARIZEK: Nye County will add some more control if that program continues.

EDDEBBARH: Definitely.

PARIZEK: And that will be a critical area to help pin that down.

EDDEBBARH: Definitely. I think the first Phase I and Phase II of Nye County was to drill wells perpendicular to the flow path, and I think now they are drilling wells along the flow paths, and hopefully that will provide, you know, a lot of insight into both the--regarding the flow directions, and also guiding the transition zone from the tuff to the alluvium, and also regarding uncertainties related to specific discharge and other hydraulic parameters.

PARIZEK: Now, rocks are getting better. I feel much better. I'm going to sleep good tonight because both the unsaturated zone and the saturated zone are looking a lot stronger, because a lot of the assumptions that were in before are being removed. Are there any others left on the table that you still could remove to make me feel even better and sleep even better? Or pretty much now it's going to be data dependent? I mean, you don't really have many more conservatisms left over that you can remove from this model?

EDDEBBARH: Well, again, it depends, you know, on the objective, you know, and on how much uncertainties the project is willing to live with. And you know this better than I do, you know, like you're not going to eliminate uncertainties 100 per cent. But you will reduce them.

I mean, as I said, the two most important ones, which the Nye County program is really helping with, are the specific discharge and that transition zone. And the transition zone, we're going to be able to reduce that from like between 1 and 9, to probably within, you know, a couple model grid zones.

PARIZEK: Thank you.

RUNNELLS: Priscilla?

NELSON: I yield to Paul.

RUNNELLS: Okay. You yielded to Paul.

CRAIG: Craig, Board. I must admit I'm confused, but I'm not a hydrologist. When I look at--and what I want to talk about is your remarks on narrowing uncertainty. When I look at your Figure 28, which I guess is the present state of your runs, you've got something like a quarter of your median runs which are showing breakthrough times, median breakthrough times, of 100 years or so. So that's a big fraction of your runs are yielding times which are 100 years, which is pretty short.

Well, if a quarter of them are showing times which are 100 years, what kind of a role is the saturated zone playing? It looks like it's not playing much of a role.

And then you gave some independent lines of evidence that related to ages of carbon, but of course that convolutes the UZ and the saturated zone, so it doesn't really tell you much about this problem of the short time frames, because there may be long hold-up times in the UZ.

So you made some remarks about new information that may narrow this uncertainty band down, and I'd like you to repeat, if you would, what kinds of new information might narrow the uncertainty range down and compress this distribution, and how much narrowing down might you expect if you're optimistic?

EDDEBBARH: That's a very important question, because the range associated with the specific discharge that was used for the TSPA SR is the range that was offered by the expert elicitation panel, and it was based on their expert judgment and the little data that they were presented with at the time. And I think they must have not done a good job into explaining that this analysis, the TSPA analysis, was--I mean, this exercise here was started in, like, late 1998 when the SZ site scale flow and transport was developed, then it was abstracted, and then it was given to TSPA to do their performance assessment, and then the documentation. So the whole process is a very lengthy one.

And what I would say is in the meantime, since this exercise here, we were able to analyze the C-well testing data. We were able to have the information from the Nye

County wells. We were able to have more hydrochemical data and analyses. And that data helped us generate the best estimate case. And even in the best estimate case, I mean, right now, the position of the project is we are not taking any credit for flow in the matrix. We use a single continuum with a single permeability, and that is the permeability of the fractures, which is a lot higher than the neighboring continuum. And we also used some effective porosities of the fractures, which are like ten to the minus three, very, very, very small.

CRAIG: But you told us at the beginning of your presentation that your present uncertainty bounds are about an order of magnitude.

EDDEBBARH: Right.

CRAIG: And if I take 600, 800 years as the mean and I put an order of magnitude on that, I'm down to 60 to 100 years, which is very consistent with this graph.

EDDEBBARH: Right.

CRAIG: So that would lead me to conclude that you have not compressed your error estimates over this.

EDDEBBARH: Yeah, this is, again, this was the TSPA SRCR, which was documented in March, and the data that was used was from expert elicitation which took place in 1997.

CRAIG: Well, what do you expect that your uncertainty bands will be at the end of this calendar year?

EDDEBBARH: We expect, as I said before, we expect to narrow it down from like a one order of magnitude, to like three times, which means that the median will be around 1000 years, and then either, you know, divide by three, which is around 400, or multiply by three, around 3000 years.

CRAIG: And what are the primary new pieces of data? You said this, but there was so much information it didn't get through to me, what are the primary new pieces of data that will allow you to narrow that band down?

EDDEBBARH: The main pieces of information are data from the C-well testing, which will give us--which will help us narrow the specific discharge parameter, and also the portions of the flow that is in the volcanic tuff as opposed to the alluvium.

As I said before, I mean, in the volcanic tuffs, the transport is occurring into the fractures. It's like pipelines. The minute the particle is there, it goes. Now, right now, we have 19 kilometers of the 20 kilometers compliance of the transport path is in the fractures. I mean, with the Nye County wells, as I said, right now, 19-D has 600 feet of saturated thickness, and 19-D is located three to four kilometers north of the compliance boundary. So right then, we cut off the uncertainty from being, you know, like one to nine, into being four to nine. So this will help, you know, reduce the range.

And I think we'll probably be looking at some of the conservatism in the specific discharge in the fractures. We look in detail into the effective porosities, most of the information that we have from the C-wells and other data indicate that the effective porosity is much, much bigger than ten to the minus three. It's more, you know, in the order of ten to the minus two, ten to the minus one. And that's not two orders of magnitudes.

RUNNELLS: We're going to have to terminate this now.

Thank you very much, Dr. Eddebbarh. We appreciate it. We'll now take a ten minute break.

(Whereupon, a brief break was taken.)

RUNNELLS: Our next speaker is Bob Andrews. He's going to talk to us about TSPA. Bob is Manager of Performance Assessment Operations, and we'll turn the time over to him.

ANDREWS: Okay, thank you, Don.

The Board has asked a very detailed question here, which you have in your agenda. We'll keep it up here for a few minutes to allow you a chance to reread it. (See Question 4 in its entirety in the Index.)

We did not copy the question onto our viewgraphs because it would have extended the length of the presentation a little too much. But there's a lot of questions and buried questions in this, where the first question is really explain to me TSPA in as transparent a fashion and as clear a fashion as you can.

RUNNELLS: Bob, let me interrupt you.

Folks, time to start, please. The conversations back there in the back, either go into the hall or terminate the conversations, please. Thank you.

ANDREWS: In trying to explain that in as clear and as transparent a fashion as possible, there's a lot of individual questions, you know, in the review that the Board has conducted of draft materials that were presented either in August or final materials presented in December, there was questions, you know, detailed questions that say, well, we don't quite understand how this happened. And that's the nature of some of the sub-elements of the question.

So we thought in preparing this, rather than answering question and sub-question one at a time, we would answer the global issue of transparently explaining the performance assessment and the contribution of the different barriers in the performance assessment, and then peel off the onion, you know, as we say, and try to look at the contribution of each as we walk through the system. And hopefully by the time we're done, I can say we've answered all the questions and we'll come back to the question.

So, with that, I'm going to turn this off, and take it down, in fact, so that Priscilla, you know, can see, because I hate it when somebody can't see. Now I just have a safety issue of tripping over the cord.

So we're going to walk through the question, talk a little bit in one or two slides about the tool we've used to address the question, walk through the barriers, and then look at various approaches, but focus on the contribution results. And we're going to go into the contribution results and break it up as the Board asked in their question, first looking at the nominal waste package scenario class, then looking at a few cases, specific cases, where the waste package is not a major contributor. So you're kind of taking the waste package out of the equation, and re-addressing and re-answering the question.

The main part of the question was to clarify the roles of the different barriers in the total system performance assessment, address the over reliance on the package in the safety case, and in answering these questions, do these sub-questions. That was my paraphrasing of that very long set of questions.

So, we have a tool. The tool is the total system performance assessment indicated by this wheel. That tool integrates a wide variety of processes, features and events that can affect the post-closure performance of a potential repository at Yucca Mountain. It starts with the unsaturated zone flow, continues around to the environments that the packages would see, both the thermal hydrologic environments and the geochemical environments, continues with the package, the waste form, the transport out of the engineered barriers, transport through the unsaturated zone and saturated zone, and finally the biosphere.

Already today you've heard from Bo about the unsaturated zone flow and the unsaturated zone transport. You've heard from Al on the saturated zone flow and transport. And you've heard from Gerry Gordon about the waste package. He mostly focused on the waste package degradation modes and methods, but those are applicable as well to the drip shield.

What you haven't heard much of is the environments, and you haven't heard much about the EBS transport. I'm going to focus a little bit on both of these to complete the story, if you will, to explain some of the total system results. But this wheel and all the sub-elements of the wheel kind of indicates the comprehensiveness of the performance assessment, and also kind of indicates the complexity. These processes that we're trying to integrate and allow information to flow from one to the other are fairly complex processes. You've heard, you know, snippets of the details of some of them as we've gone through.

It's also a point that the Board raised in their September 20th letter, and I think that wasn't the first time they raised it, they've raised it in other communications to the Department, that some barriers, some uncertainty can mask the contributions of other barriers. And, therefore, it's sometimes difficult to see the individual contribution of an individual part of the system when one barrier is masking another barrier. So, therefore, sometimes to more clearly elucidate the role and contribution of the different barriers, we need to do some alternative methods, some alternative graphical methods, peel the layers off of this system and look at the contributions of each one separately.

Okay, the barriers that we've explicitly included in the TSPA for the site recommendation, the one that was just completed last December, Rev 0, includes these nine barrier contributions. And starting at the surface and walking down all the way to the saturated zone, we see we have really two natural system barriers here in the rocks overlying the repository. We have three engineered barriers, if you will. The waste form is kind of an engineered barrier. The drift invert is either an engineered or a natural system barrier, depending on how you conceptualize the world. And then finally beneath the repository, we have two natural system barriers again.

The next three slides just put those barriers and the functions of those barriers into some construct. It ties those things to the attributes of the system, which were the elements of the repository safety strategy that the Board has also reviewed, and I think it's going to be a part of some discussion tomorrow afternoon, and the individual what we've terms MPA process model factors. So these are the individual piece part components that go into the total system performance assessment.

So I don't mean to go through these in detail. These are mostly for your information. Anyway, let's skip through these. They're in there for your information.

Okay, as I've pointed out, we've talked about it's useful to stop before going into the results and start looking at some of the concepts that are behind the results. And if we can understand the concepts of what's happening in the package and the unsaturated zone and the saturated zone and in the drift, then we can more clearly I think peel the layers off of the onion and understand the results and the way they are.

Some of those have already been hit on by Gerry, Bo and Al, but inside the drift, we haven't really hit on it. So let me go to the next slide, and go on in the drift and look at some of the processes going on in the drift at a conceptual level, not at a data level, not at a model level, not at a parameter level, just what's going on within the model with respect to the processes that are acting within the drift.

And I have a series of four slides here. Two of the slides are for the cases where there's dripping, you know, that occurs in the drift environments, i.e. there's seepage. That happens roughly about 15 per cent of the time in the most maximum climate state that we have, the highest infiltration rate state we have. So this set of environments occurs 15 per cent of the time over 15 per cent of the repository, if you will. The other set of slides are going to be non-dripping environments, i.e. in the absence of dripping, now what goes on. So we have two sets of conditions.

There's two sets of processes that go on, too. I mean, there's a lot of processes, but I've kind of broken them up into two sets. One are the hydrologic processes, so the thermal and hydrologic processes that are going on, and the other are the transport and chemical processes that are going on.

So let's just start here and walk through what goes on once I get a drip conceptually, and that's what's in fact in the model. The actual parameters we'll get to later on, and how those parameters lead to the performance that's been projected. But let's just talk about it conceptually first.

Given that we have seepage, which is a function of a lot of things, and Bo alluded to many of those things this morning, there's a lot of things going on in that seepage model that give us the possibility of seepage in a certain fraction of water which actually drips into the drift.

For that which drips in--there's supposed to be a drip shield here somewhere. I think you can kind of see it. I think it's better in the handout than it is on this. A certain fraction of that--all of it hits the drip shield. A certain fraction of it runs off the drip shield, until such time as the drip shield fails, and then it goes through the drip shield, and then it hits the package. And a certain fraction of that runs off the package, until such time as the package fails and degrades and has a hole sufficiently in size that water can drip through that hole.

And then it hits the waste form. And here in these four slides, I tried to pick out the one or two really key assumptions that are pretty darned important to performance, and a conceptualization had to be developed and a simplification had to be applied in the absence of a very detailed complex understanding of what really happens inside a package thousands of years after the package has been emplaced to the innards of the package when water hits it. And we made a very conservative assumption that every drop of water that gets into the package sees every ounce of waste that's inside the package.

You say, well, that's crazy. You know, the likelihood of that drop of water, or a few drops of water seeing the entire inventory of exposed waste is pretty small.

And you're right, and we're going to evaluate the significance of that particular conservatism as we go through the next while. But it's at least conservative.

COHON: Bob, do we need to understand what exposed means? Or does that mean all the waste in the package?

ANDREWS: It's all the waste--it depends on the waste form now, whether I have a glass waste form or a DOE spent fuel waste form or a commercial spent fuel waste form. If it's a commercial spent fuel waste form, there is a certain fraction of the waste that's not exposed because the cladding is intact. You know, for the glass waste form, once the waste package barrier is breached, there's no credit taken for the canister. For the DOE spent fuel, there's no credit taken for cladding. For the Naval spent fuel, there is credit taken for the cladding. So we have really four waste forms, and we're tracking those separately, you know, through the analysis.

Another one here is not quite as important, but we assume the flux into the package in a certain number of liters per year equals the flux out of the package. In other words, we're going to have a hole in the top, water gets in, I don't wait for the water to fill up the package before it spills over and over flows, we just say, well, let's just conservatively assume that when I have a hole up here, I've got a hole down there. And that's a reasonable assumption, but conservative assumption, because probably there's some delay time between hole number one and hole number two. And then I get into the invert and back out into the rock. Those are fairly reasonable assumptions.

Let's go on to the next slide on the non-dripping environment. Now, of course you see no arrows because there's no water moving, except in the rock. I probably should have put some arrows in the rock because, as Bo had them on his figures, clearly there's still water. Water is still moving on an average of 5 millimeters per year in the present day climate, and it's going around the drift rather than coming into the drift.

So in this case, I have a humid air environment, you know, above the drip shield. I have a humid air environment on top of the drip shield. I have a certain deliquescent point, a point that came up with Gerry's presentation, on top of the drip shield. I have a humid environment between the drip shield and the package. I have, once the package has breached, I have a humid air environment inside the package, probably close to 100 per cent humidity.

And then on the exposed waste form, it's assumed that that humid air environment has completely covered with 100 per cent humidity that exposed waste form. Finally, I have cracks in the bottom of the package, or I could have cracks at the bottom of the package, and I'll come to the transport aspects of this, which is very important, in a second. But those cracks through the failed waste package are assumed to be saturated with water, i.e. they allow for a conduit for nuclides to get out, not by advection, but by diffusion.

And then another important assumption is the water content in the invert, which clearly is going to be a function of the design, especially for, you know, thousand years where the design and the thermal management scheme are important to that water content, and the rock and invert characteristics. So the amount of water that's in the drift is a function not just of seepage in the case of the dripping environment, but it's a function of the rock and invert characteristics. Water can be sucked in by capillary. So let's go on to the next slide. So that's the hydrologic and thermal environments inside the drift for these two different environments.

Now it's worthwhile to look at the release mechanisms, the transport mechanisms. In the case of the dripping environment, water in hits all the packages, and hits all the waste, and then at that waste form/water contact, remember we have dripping water contacting the waste, a release of nuclides based on the alteration rate of the fuel and the solubility characteristics of the individual radionuclides in that water phase, and also there's some colloids that can go into that water phase, too.

But once I have that point, this assumption that I've assumed that immediately after the first breach, I have that second breach, there's no time delay, and so the mass flux out of the package now in terms of mass of activity per time is a function of the amount of water which got into the package, which changes with time and the chemical characteristics of the dissolution of the waste form and the solubility of the radionuclides inside the package, so it's just a product of those two terms.

And finally, when I have advection through the invert, it's just moving with the advective velocity of how much water seeped around and went through. And that advecting water goes into the fractures. So water drips in, and water drips into the fractures. This happens about 15 per cent of the time.

BULLEN: Bob, before you do that one, what's the residence time of the water on the waste package, on average?

ANDREWS: Rich, do you know the number?

And so it reaches saturation as it passes through with all the radionuclides in which it's coming into contact wherever that solubility is.

BULLEN: Okay, thank you.

ANDREWS: I mean, you can have some alteration dependent releases and solubility limited releases, depending on the solubility of the nuclide in that water phase. That's why when we get to seeing results, we'll see different results for Technetium than we will for Neptunium for that very reason.

In the non-dripping environment, it's very different things that are going on. Remember, I assumed that once I had a breach in the package, that there's a water film, you know, that can coat, a very thin hydroscopic water film that can coat the waste form.

What we've assumed is effectively that that waste form, because we don't know the real degradation characteristics, or we did not model in Rev 0, the real degradation characteristics of the fuel bundles and of the stainless steel support rods and structural members that are inside the package, so we just said for modeling purposes, that waste form is sitting down here at the bottom of the package, just sitting right there. There's no credit taken for diffusion from anywhere inside the package to the edge, inner edge of the package. Time is zero, if you will, from here, the time of diffusion to here, remember there's no advection in this case, there's no dripping, the time of diffusion from here to here is zero, no credit is taken for that particular transport time.

Also through the package, remember my assumption before, as soon as I have a crack, I put that crack essentially at the bottom of the package, or hole at the bottom of the package, now I can get transport through the package by a diffusive mechanism, a concentration gradient, you know, drives nuclide through this very thin water film. And I assumed that the hole in the package--that doesn't really show a hole there very well--but the hole through the package is saturated with water. So radionuclides can diffuse through that particular area.

They can also diffuse through the invert, depending on the liquid saturation characteristics, the diffusive characteristics and the transport characteristics of the invert, radionuclides can diffuse through the invert.

And finally, the last conservative assumption for diffusive related transports out of the package and through the engineered barrier is that when that diffusive flux hits the rock or hits this point here, it also goes into the fractures. Those little conceptual drawings of the drift shadow zone is essentially assumed not to occur, and it's even more conservative than that, we don't diffuse into the rock matrix, we diffuse into the fractures. And then the nuclides are then transported in the fracture flow that Bo has already talked to you about.

So with that conceptualization, let's go on to the next slide and look at the five or six cases that we're going to use to help peel off the onion.

The first one is what we'll call the nominal case, base case. It happens 99.99 per cent of the time. It uses nominal models that Gerry talked to you about with respect to the package. We'll look at the results of that here in a second. There's uncertainty in a lot of those models and a lot of those parameters, so we have a wide distribution of package degradation rates and a wide distribution of the fraction of packages degraded at any particular time and within any particular realization. So there's a lot of uncertainty there, but we'll see the results that will show that there's only about a 1 per cent probability of having a single package breach prior to about 11,000 years. It's about 10,500 years. That's one case, and we'll use that as a starting point.

But then we'll take a number of alternative cases to try to elucidate what's going on. First off, a thing that we've occasionally called a juvenile package failure. In your question, I think it was referred to as the juvenile package failure, and we sometimes call it an early waste package failure, too. So this is a non-mechanistic degradation, non-mechanistic failure of a single package. So it looks at a single package and tries to understand what goes on.

It puts that breach at the time of emplacement. It says it assumes it's breached, has a hole in it at the time the package is emplaced. The size of that hole is about 300 centimeters squared, and that's just simply the size of one patch on the package. Each package has about 1000 what we call patches, and we just said one patch is degraded, completely removed. Every other part of the system is treated as a nominal case, and in fact we don't know where that package is, so we said okay, randomly it's located around the repository, 15 per cent of the time it's in those dripping environments we talked about, and 85 per cent of the time it's in the non-dripping environments.

We looked at another one. It was very similar to the juvenile or early waste package failure, which we called the neutralized waste package scenario. The neutralized package scenario assumes all the packages were like that, every single package at receipt had a hole--at emplacement, I should say--maybe not at receipt, but when it was emplaced, it had a hole of about 300 centimeters squared that went through it. Everything else from that to the early package failure scenario is the same.

We looked at another one that we called the degraded waste package barrier analysis. In this one, we took about the top seven or eight parameters in the waste package degradation model. Some of these had to do, as Gerry pointed out some of them, I think, you know, the stress state at the weld, the defect distribution at the welds, the aging factor, the MIC factor, the corrosion rate uncertainty and variability. So a number of these key waste package degradation parameters we fixed at their near maximum value. Sometimes the maximum value is near the 5th percentile. It's the one that would lead to a more rapid degradation of the engineered package materials. And in that case, we have another rate and amount of package degradation tied to that set of assumptions.

Final case that we looked at, not directly related to trying to understand and elucidate the contribution of the package or the contribution of the rest of the system when the package is removed, but there's another scenario that effectively removed the package from the equation, and that's the igneous intrusion scenario. In that particular case, with a low probability of about, you know, 1.6, ten to the minus eight as the mean distribution around it, it comes up and intersects the drifts, and effectively completely neutralizes, i.e. not only a hole, but the entire surface of the package that is assumed to be degraded.

That igneous event has a temperature of I don't know what it is, 1200 degrees C, or so. The package was not meant to withstand 1200 degrees C for any length of time. It was not its function. So we just assumed about 200 packages that are documented in some of the analyses are completely neutralized, which means about 400 breaches, each breach about 300 centimeters squared. So you essentially remove the whole package.

I mean, not only that, when this event occurs, we remove the drip shields and the cladding. So all three of those barriers are completely removed from the equation. It has one slight little variant which caused the results to be a little bit, not difficult to explain, but a little different than the rest of the case. That is the solubility. Instead of being controlled by the in drift chemical environment, it now becomes controlled by the in rock chemical environment, which we thought was a fairly reasonable assumption.

All the other components are treated the same as the nominal case with whatever uncertainty they had in the nominal case.

So, now let's go through some of the results to explain what's going on. I think before we get to that, let's go on to the next slide.

We're going to look at the subsystem performance for the nominal scenario class. We're going to look at subsystem performance for the early package failure and these degraded and neutralized, and the volcanic class.

I want to point out that there's a wide range of other both degraded and enhanced barrier importance analyses that are documented in the TSPA SR report, and documented in the current version of the repository safety strategy. So I'm just pulling out some to help explain things. But there's many others in there.

What are the subsystem performance measures we're going to look at? First, we're going to start with the total system part, the dose rate, and then start looking backwards, look back up the system. First, we're going to explain that dose rate and its dependence on the package and the drip shield, because they are highly dependent, especially for the nominal case. Then we're going to look at some individual release rates. And just as a word of caution, when I get to the release rate part, my axis are going to change. You know, they're going to change from millirems per year to grams per year. So it's a mass release across the boundary rather than a dose rate attributed to that mass release which would have been dissolved in a certain volume of water.

Okay, so the very first set of curves. In all the plots that follow--I tried to be consistent--I tried to show the actual realizations, so the full breadth of the uncertainty, as we did in TSPA SR, and some particular statistical measures, you know, that try to capture that uncertainty in a more simple fashion, in particular, the 95th percentile, the mean, the median, or 50th percentile, and the 5th percentile. But the gray lines that sometimes look like just a gray mass are all the realizations behind that.

You know, in one particular case, I put in the backup for one example because it was more elucidating, and I picked out one realization, you know, to share with you. But that's in the backup and we won't probably go into that.

So here's the total dose. This is, if you will, the total system performance measure for the nominal scenario class. So this is in the absence of the volcanic intrusion or extrusion class. And we see, as I talked about earlier, you know, there's no dose until the first package fails. The package is completely containing the waste for more than 10,000 years for the nominal set of scenarios and nominal models that are used for the package degradation.

SAGÜÉS: How many scenarios?

ANDREWS: This is 300 curves, 300 lines on there.

What's it attributed to? Well, to look at what's driving the results, you have to first look at what nuclides are driving the results. So I plotted here the two dominant nuclides. At earlier times, you know, out to about 40,00 years or so, the doses are dominated by Technetium 99. After that time, Neptunium dose starts taking over, and it becomes the dominant contributor, such that at 100,000 years, Neptunium is providing 90-something per cent of the total dose, whereas at 20,000 years, more than 90 per cent of the dose is attributed to Technetium. So I've switched which nuclide is controlling.

Let's try to peel the onion off a little bit and start with the Technetium part. Technetium is a high solubility. It's advective. Travel times through both the unsaturated and saturated zone are close to the values that Bo and Al talked to, which is a few thousand years, or less in the present day climate, and becomes less than that in future climate states. They diffuse rapidly, too, because that high solubility, they diffuse out of any hole relatively quickly through whatever water film is there.

So, in fact, the total uncertainty and spread and start time of the Technetium dose is almost wholly explainable by the rate at which waste packages are degrading, where this rate is the number of packages that come on line, if you will, or start degrading as a function of time. Compare that mean curve, and that mean curve, they're almost explainable exactly as is. So the rate at which packages fail is the rate at which Technetium is released, it drives the rate at which Technetium is released across individual barriers, drives the dose. That's applicable to any high solubility nuclide. Technetium just has to be the highest inventory and a fairly high dose conversion factor. But the same response would be seen with iodine and Technetium, any high solubility nuclide. They're just lower than Technetium is.

Next slide does the same thing with Neptunium. Now

Neptunium is a little different. It's a low solubility. It does diffuse. It does advect. But it's not so much dependent on the rate at which packages fail or the engineered barriers are degraded, it's much more dependent about the cumulative amount of degradation.

So what we've plotted here is just the cumulative breach area, the cumulative amount of area of the packages that are degraded as a function of time. Number of packages times total area that's degraded, because packages, once they start degrading, they continue to degrade. You don't just have one hole, you have many holes with time.

So, you see that the dose rate is a function of the cumulative breach area. You say, well, why is that? Well, the answer is the cumulative breach area defines the total volumetric flow that goes past the waste. And it also defines the cumulative area available for diffusion out of that package.

So as we add more and more area, which is greater area available for diffusion, greater area available for advection, we get more and more release. As we get more and more release, we get higher and higher dose.

Okay, now we're going to break up the system into releases across the engineered barrier, releases across the unsaturated zone at the water table, and then releases at the 20 kilometer point.

As you can clearly see, the differences here in these curves--no, you can't, I mean there's too many things on here, so let's go to the next slide. This is results, and now we're going to go to the analysis of those results on the next slide, and I'm just going to focus in on the dominant dose contributor over the 100,000 years, which is Neptunium.

On the top side, or just picking the mean release rates from the previous slide, and the median release rates across those three barriers, edge of the EBS, edge of the UZ, edge of the SZ, it's still somewhat difficult to see, you know, the contribution of each of the barriers on a log kind of time scale. So what we've done down here is blow up just this portion of the curve. You know, out here it's 60, 70--well, 50,000 to 80,000 years I think I picked in both cases. Yeah, 50,000 to 80,000 years, and I hope it's clearer in your handouts. And look at these. And when I look at the mean, the mean time of delay of Neptunium in the unsaturated zone is about 1000 years. The mean time of delay, and this little light blue line is the SZ, from the UZ to SZ is also 1000 years. So the mean delay time is about 1000 years for both of these.

This is after climate change, or in fact two climate changes, and this is a slightly retarded radionuclide. So it's slightly different than the results that Al and Bo talked to you about, but it shows you the contribution for the means is about 1000 years in each.

If I look over to the medians, so the 50th percentile of the distribution, the UZ is given about 2000 years, and the SZ is about 10,000 years. Why the difference between the mean and median? Well, it shows the distribution, and I think Al had a good plot of it, the total distribution of travel times, or advective transport times, in the SZ is a highly skewed distribution. It's a very log distributed solution. So there's some possibility of relatively rapid travel times, short travel times, but a large fraction of the total distribution, you know, has much longer travel times. So you kind of have that bi-modal distribution showing up here as the difference between the mean and the median.

Don, how much time do we have?

RUNNELLS: You're doing fine. I'm going to warn you at 4 o'clock. That's about seven or eight minutes from now. So I'll warn you three times instead of the two you asked for.

ANDREWS: Okay. We're looking now--we looked earlier at the EBS release total, mass release across the EBS. It's useful to break that out into those two parts that I started talking to you about. One is the advective part. That's the case where I have dripping. And the other is the diffusive part, which is the time when I have no dripping. So it's just diffusing through.

And, again, up until about 40,000 years, the advection--well, the diffusion is dominant. At about 40,000 years, they become about equal. Remember, this is the total repository. So the effective net advection is six times the diffusion, if you will, just that one-sixth of my packages are sitting in advection, and five-sixths of my packages are sitting in a diffusive transport environment.

Why is that? Why is it 40,000 years? What's the magic here of 40,000 years between this diffusive and advective and between Technetium and Neptunium? It's really two things. Part of it is the drip shield. The drip shield degradation is shown here in the upper left-hand corner. The drip shield starts degrading at about 20,000 years, and most of the drip shields have degraded by 30,000, 40,000, 50,000 years. There's still some lingering ones after that, but it's that time period. So that would be when I have the drip shield intact, clearly there's no advection. I mean, water doesn't drip through the drip shield if the drip shield is still there. But if the drip shield starts degrading, then water can drip through the drip shield. So that defines part of the reason for the difference between advection and diffusion.

The other part is shown over here and requires a little bit more explanation. But for earliest times, the failure mechanism of that package is small cracks, generally at the welds. They're very small hairline cracks. They're a micro or so across, a centimeter or so in length on average, and have a very small cross-sectional area. That small cross-sectional area does allow some diffusion, but doesn't allow any advection. So because the packages have failed by very small hairline cracks, I don't get any advection.

After a certain period of time, though, which is about that same 40,000 years or so, now I start having general corrosion take place, and I have actually holes through the package. So the size of the opening significantly increases out beyond 40,000, 50,000 years.

So, again, these two things explain the reason why we have diffusion for a short period of time, Technetium dominated, versus vection at longer times, Neptunium dominated.

Okay, summary. First, on this part of the presentation, it is true, I think the Board has noted that the package failure distribution, both the rate and the amount, are masking the contributions of other parts of the system. So in order to see those contributions, you've got to take that out and look at the other parts and what they're contributing. And then these other conclusions we've already talked about, and the delay time is several thousand years in both the UZ and SZ.

Let's go on to one of the other scenarios, the degraded package scenario. In the degraded package scenario, a lot of things are fixed.

RUNNELLS: 15 minutes.

ANDREWS: 15. We're okay.

In fixing them, we have a much tighter distribution of package failures, much less uncertainty there, but we also started at an earlier time. I think the first package in one realization was at 7,000 years. That tighter distribution on package failure leads to a tighter distribution on the uncertainty in the dose estimate. It also causes it to occur earlier in time.

So we could have peeled the onion off of each of the individual cases, but I just wanted to explain that in fixing the package, in a lot of ways we've reduced the uncertainty and the projected performance, which implies that this uncertainty, or this uncertainty, which is about three or four orders of magnitude, is other things. It is seepage. It is flux. It is solubility. It is advective travel times. It is biosphere issues, et cetera. So it's other things other than the package.

Okay, let's look now at the early waste package scenario just to reintroduce it. In this case, one package, one hole at time zero, and this is our dose response. You know, the mean is at about ten to the minus two millirems per year. Broke it out again, the Technetium contribution is the dominant contribution up to roughly 1000 years, a little more than 1000 years. Why? It has shorter advective travel times through both the unsaturated zone and the saturated zone.

Neptunium then takes over, and again becomes the dominant dose contributor after about 2000 years. Let's peel this one off. Again, the EBS UZ and SZ, breaking out the mean and the median for this particular case. And, again, if I look at the mean, and there is a light blue line there and I hope it's better in the handout, it's about 1000 years delay across the UZ, and about 1000 years delay across the SZ.

The median is about, you know, 1000 or so years across the UZ, and the SZ, it's kind of hard to tell because there's been a lot more spread. Remember, this is a single package now, not, you know, a lot of distributed packages. So that time delay in the saturated zone from this curve to this curve, you know, it's a much more smeared curve or breakthrough, which is not surprising. You are seeing the dispersive effects of both the unsaturated zone and saturated zone to take over, which is kind of what the TSPA VA peer review thought they would see, you know, for a single package fail. So now we see it.

Let's go on to the next slide where I've broken up the EBS total into the advective part and diffusive part. Again, this is a single package, and it's all diffusion out to the time at which the drip shields start failing. The drip shields start failing out at 20,000 or so years, and then you see advection taking over. So the drip shield is giving you that 20,000 years, even though it's diffusing out of the package.

I know you're curious what's going on with this little hump here, and in the interest of time, I've put that explanation in the backup. Essentially, it's the early time in package chemistry is driving the Neptunium solubility to be high. The pH is, I forgot which way it goes, but the pH in that environment is such that the Neptunium solubility is high, so it creates a slightly higher, about a factor of ten fold increase in the EBS transport during that time.

Okay, so this kind of summarizes those results, and kind of reinforced the results that we just saw for the nominal scenario class.

Okay, now the Board asked for another case. They asked for the complete neutralization--no, sorry. Before I get to the complete neutralization, let's stop here. The case where we said it was neutralized. This is no more than the early waste package failure scenario, multiplied by the total number of packages. I mean, my earlier package failure scenario was one package. This neutralization scenario is just 11,770 packages.

There's slight nuance differences in the fact that the early waste package failure scenario we assumed, just because we wanted to maximize the effect, was a commercial spent nuclear fuel package. This 11,770 includes those commercial spent fuel packages, you know, 63,000 metric tons worth, plus the DOE glass and the DOE spent fuel, and the Naval fuel. So it's kind of distributed amongst a lot of other waste form types. So it's not exactly multiplied by 12,000, but it's darned close. You can see this one is .01. You multiply that by 10 to the fourth, and you get about 100, which is that number. So it comes out darned close.

Okay, one of the sub-sub questions of the Board was we don't quite understand why in this neutralized case, it appears--or in the degraded case, it appears you have a higher dose rate than the neutralized case.

Well, remembering back to how we were peeling the onion off about the total breach area driving the Neptunium dose, so what I've plotted here is the cumulative breach area in these three different scenarios. One, assume that it's a breach at time zero, and then stays breached. The other one is it's breached pretty rapidly. That's the degraded package case. And the last one is the nominal case.

And you can see the three dose curves kind of map onto the cumulative amount of breached areas, the cumulative area of the package that's been degraded. So, you know, performance is fairly simple in a way. This curve and this curve are the same for all practical purposes, and they cross the neutralized package failure at the same time, out there at about, whatever, 60,000 years or so.

Okay, now here's another case. This requires a minute of explanation. We have two sub-scenario classes of volcanic event. One is the extrusive event, you know, it comes to the surface and is dispersed by wind. The other is intrusive event, where the engineered barriers are degraded and removed. And then the nominal processes take place.

To compare it to what we've just been presenting, it's much more germane to talk about the igneous intrusion groundwater scenario class, not the igneous eruption scenario class. So these are the result that we've presented. We probably combined it in our plotting with the erupted event, but the probability weighted doses in 10,000 years are dominated by the igneous intrusion event. So I focused in on that one.

I've only shown for purposes here just the mean curve. The 5th, 50th and 95th percentiles start losing a little meaning when we're talking about a very low probability event to begin with. But it is meaningful to talk about the mean of that distribution. So that's what I've shown here, is the mean.

This has the probability factored in. The probability is, as I said, has a mean of about 1.6 to the minus eight. I want to take that out now. I want to take the probability out of the equation and talk about the unweighted doses. So this would be the risks, if you will, which is the way Part 63 asks, we believe, risk informed performance measure. And now I'm taking the risk part of it out. I'm talking about consequences.

The consequence of that possible event also has a distribution. Depending on when it occurs, the inventory is different, so the consequences are different. The mean of that curve is shown here. So this is the probability taken out.

A couple of points to note is in addition to taking out the package, I've taken out the drip shield and I've taken out the cladding. In order to compare this to the stuff we just finished talking about, I can either normalize to all the packages, or normalize to a single package, and I decided to normalize to a single package. This is 200 packages, roughly.

This is that mean curve that I just talked about, normalized to now a single package, a single package and drip shield and cladding that are completely removed. You can see that the difference between this and my early package failure is about a factor of 300. That factor of 300 is predominantly due to the fact that I've exposed the entire area of the package.

There's a little additional due to the cladding. There's a little additional due to the drip shield, and there's a little bit of additional due--in fact, it's in the reverse direction--to the solubility difference. But it's predominantly due to the package area breach.

Okay, my first slide talked about some major assumptions that we were making, major conservatisms we were making in the EBS flow and the EBS transport area. The Board has pointed this out to us in numerous occasions, and most pointedly on September 20th in their letter, and so we said let's elucidate what's going on with some of those conservative assumptions that are in this particular area inside the drift.

These are four major ones that I had on one of my earlier slides. We have started this work, and I want to show you one example, which is this one, the diffusive release mechanism from the package. Remember, I said it was very conservative, just was at the base of the package and diffusing out and straight into the invert. So let's take a look at the results when we remove that conservative assumption.

Okay, this was a base case that we talked about earlier, and this is putting in a modified diffusive release model from the inside of the package into the invert. So just one of those assumptions that we made has this kind of effect. You can see out there at 20,000, 30,000, 40,000 years, there's no real difference. Once my drip shields start failing, the diffusive characteristics in that assumption across the packages don't make a whole heck of a lot of difference. But until that time, number one, I've delayed it by, what, about 5,000 years, and the other one is I reduced it by about two orders of magnitude. So that one particular conservative assumption had 5,000 years in time and two orders of magnitude in magnitude for that time period. The longer time periods, no impact.

Okay, we'll wrap it up here then. So I hope--and let me now go back to your questions. The aim was to answer your questions, but I'm kind of peeling the onion off rather than going through them one at a time. And we've addressed these issues with the nominal case. We looked at those scenarios you asked for, and we threw in a couple more.

We looked at significance of the different barriers and significance of the degradation mode and release mode from the engineered barriers, the advective versus the diffusive component. We looked at that in particular at this 100,000 year dose of the degraded package versus the neutralized. We looked at the potential dose if all the package were neutralized, using as sort of an example the volcanic igneous intrusion event.

We didn't really look at this one, because in answering what would be the potential dose if one or more packages were released directly to the accessible environment, we thought there were a number of ways we could look at that. One, we could look at that igneous intrusion one. That kind of gives that number. But you have to kind of make an assessment of what's the total volumetric flow and the groundwater regime that you're putting that contents of a single package into. So we said we're going to use that as sort of an example.

We could have used the human intrusion example that we also have documented in the TSPA document, but there's a lot of other assumptions in there that make it not quite as clear to distinguish what's going on.

So we looked at the individual contributions, and finally I hope, and there will be more discussion of this tomorrow with the repository safety strategy and path forward, that the individual contributions under defense-in-depth of all the barriers that we looked at, the package, the drip shield, the invert, the UZ and SZ, give you some sense for the defense-in-depth of the whole system.

So, with that, I'll open the floor to any questions.

RUNNELLS: Thank you, Bob. As always, an excellent presentation. We appreciate it.

Well, as long as our Question Number 4 was, it filled the whole screen, there must be lots of questions from the Board. So we'll start. John?

ARENDT: Arendt, Board. You used breach, degrade and fail interchangeably. I understand that breach and fail would be a failed package. But I do not understand that a degraded package would be a failed package. Now, I notice also in your viewgraph, Slide 15, the copy that we have says failed waste package, and I believe you used degraded.

So I'm kind of curious if I'm understanding you correctly. I don't understand the three to mean the same.

ANDREWS: Well, we have the degradation processes, and we said when those are sufficient to degrade, and they degrade with time a package. When we talked about this degraded barrier, we were kind of using, maybe it was in hindsight for this particular case, I realize it might have been confusing, we're talking about enhanced barrier and the opposite of enhanced, which we thought was degraded. Maybe it should be, you know, on the good side, on the bad side of the barrier. Degraded barrier is a breach, which is a failure. It's a failure of that containment, a failure of that barrier to perform as it was functioned to perform at that time, whenever that time might be.

So I appreciate the concern, and I realized that from the questions, you know, what's the definition of degraded, what's the definition of neutralize, what's the definition of breach, and it has caused some confusion. But all three of them cause a through-going conduit, if you will, through the package.

RUNNELLS: Jerry?

COHON: Cohon, Board. I have a similar line of questioning to John's, but I want to focus on neutralize. And if you could put up the Board's question again? And I want to focus on the question you didn't answer that you pointed out, where we use the phrase completely neutralized, sort of three-quarters of the way down, what would be the potential dose if the waste packages were completely neutralized.

ANDREWS: Yes.

COHON: Now, this in no way objects to what you've done at all. It's very interesting and largely answers the questions that some of us had. But I wanted to give a little more background and talk a little bit about semantics.

You define neutralize, so that's fine, though it's not, I don't believe, what we meant there. So for you, neutralize meant the package has a breach in it, a hole, has a hole. I think neutralized--furthermore, when you say completely neutralized, you meant all the packages have a hole, they each have a hole? Completely neutralized, for you, that phrase meant every one of the 12,000 packages had a hole?

ANDREWS: That was neutralized.

COHON: What did I just say?

ANDREWS: You used completely neutralized. Completely neutralized would have been for me that case of the igneous intrusion event where the whole package surface, I mean, it's almost like you had bare waste sitting in a drift.

COHON: You're right. Sorry.

ANDREWS: That would be completely neutralized.

COHON: Okay. But only 200 packages were completely neutralized?

ANDREWS: 200 packages were completely neutralized, yes.

COHON: Right. Okay, thank you.

All right, so let's do that again. Let's start again. Early what, early breach is one package with a hole?

ANDREWS: Yes.

COHON: Neutralized is all 12,000 packages, each with a hole, just like the early case?

ANDREWS: Yes.

COHON: I think completely neutralized, our completely neutralized was trying to get at understanding the contributions of the various barriers. So if you took the bare waste all exposed, the complete inventory, and you stuck it in drifts with nothing else there, what would happen? I think--now, I'm not asking you to answer the question. But what would the dose be was the scenario I think that was posing.

ANDREWS: It would be 60 times that one curve on--

COHON: Is that right? Okay. 60 times the--

ANDREWS: 200 millirems times 60, whatever--

COHON: Okay, times--for the igneous case.

ANDREWS: So 30 rems. That's completely neutralized drip shield and cladding, too.

COHON: Okay.

ANDREWS: Bare waste in a drift, that's what you asked for, yes.

SAGÜÉS: That's for the median?

ANDREWS: That's for the mean, I think.

SAGÜÉS: Oh, the mean.

COHON: Okay. Separate question, and this probably goes to my faulty memory more than anything else. I thought the last time we saw results from the base case, that even with an early breach, that the dose was zero until after 10,000 years. Am I remembering that correctly?

ANDREWS: For the SR? For TSPA SR?

COHON: Yes, the last time you presented to us. Am I just remembering that wrong?

ANDREWS: I think we, you know, in August, that juvenile package scenario or early breach scenario was presented in the repository safety strategy part of the presentation.

COHON: It showed the same kind of results you showed today?

ANDREWS: Yes.

COHON: Okay.

ANDREWS: We can verify that.

COHON: No, no, I--thank you for clarifying that.

RUNNELLS: Dan Bullen?

BULLEN: Bullen, Board. First, I want to thank you for a very illuminating presentation. But I do have a couple of questions. Could you put up Figure 36? And as we get to Figure 36, it deals with the intrusive versus extrusive volcanic event. And first, I'd like to thank you for, in Figure 36, giving us the unweighted numbers. If you'll recall, last time these were presented to us, adding that probability weighting distribution of ten to the minus four, or whatever, caused a little bit of consternation. And so even though the doses are above the regulatory limit, it's nice to see that we can see those numbers.

And I guess the follow-on question, and I know it wasn't asked in the questions we asked you, was how big a difference is there in the unweighted numbers for the extrusive volcanic event versus the intrusive? I know the extrusive flies the ash up in the air and you have a lot higher dose, but can you kind of give us a ballpark number for where that would be on there?

ANDREWS: Do you want a figure?

BULLEN: Well, if you just looked at the bottom figure, you know, and you've got the intrusive event there, what does the extrusive event look like?

ANDREWS: It's about ten rems, I believe. We have a plot that--

BULLEN: Oh, is it in a supplement? I'm sorry.

ANDREWS: No, no. I mean, somebody asked this question on Friday.

BULLEN: So you're prepared?

ANDREWS: Well, you know, we try to be responsive. But we didn't have a chance to put it into the briefing.

BULLEN: That's quite all right.

ANDREWS: And it requires some explanation.

BULLEN: Mr. Chairman, if we have a couple minutes of time, could you do that for us? That's would be great.

ANDREWS: These are the probability unweighted eruptive. So the probably, remember, is 1.6, ten to the minus eight. The mean of that is about, you know, ten to the fourth millirems for the event if it occurred tomorrow. Well, if it occurred a year after emplacement. That value decreases with time because there's a lot of soil processes and redistribution processes. That also depends on the time that event occurs. The later the time the event occurs, the dose is also lower because the inventory is different as a function of time. So this is taking the contents of those packages, you know, spewing them out and distributing them with the wind.

BULLEN: Okay.

ANDREWS: No probability in there. So you could go from these back to the other curves that we presented in August.

BULLEN: By multiplying by 1.6, ten to the minus eight?

ANDREWS: Yeah.

BULLEN: Okay. Unrelated question, but something that I'm interested in. Since the Neptunium dose is driven by failure area on the waste package, is a patch failure as your first failure overly conservative? I mean, opening up 300 square centimeters on the surface of a waste package kind of drives that dose and causes the cross-over from Tech to Neptunium early on, I don't know, 40,000 years or wherever that shows up, is that an overly conservative assumption? And can you kind of come up with justification for why you picked the 300 square centimeters, other than the fact that it's the size of a patch?

ANDREWS: Well, let me back up. Remember, everything other than the nominal scenario class and the igneous intrusion scenario class are all for insight. You know, all of these other cases, whether it be the early package failure case, the neutralized package failure case, the degraded package failure case, all of that is to gain insight into the contributions of the various parts of the system. None of those do we think are reasonable or realistic. So they're all for insight producers.

We could have gained as much insight by saying it was a crack rather than 300 centimeters squared. We could have gained insight by saying it was 3 meters squared. We picked a single patch to push the system, if you will, and see what that did, and gained those insights. Because it's those insights that help contribute to the identification of the barriers, and their individual contribution. So it's arbitrary.

BULLEN: Okay, thank you. And then maybe just one little fine point. When you finally moved all of the waste to the bottom of the waste package and had it diffuse through a crack that was saturated with water, did the crack length vary with time? I mean, the waste package is getting thinner. Did you just assume it was a 2 centimeter crack?

ANDREWS: Two centimeters thick.

BULLEN: Okay, thank you.

RUNNELLS: Question from Debra?

KNOPMAN: Knopman, Board. There are two barriers that I think--I don't think the Board has spent a whole lot of time talking about with you, and there may be other people here who can answer this question. One is the invert and the invert material, and the consistency with which one can emplace that invert, and the other is the drip shield and the drip shield material, and it's the uncertainties surrounding its performance.

Perhaps you could just walk us through, if you know the numbers off hand, what happens when you don't have the invert performing as you anticipate. I mean, these pictures now look like, to me, a platform as opposed to sitting on a metal, on a steel, some kind of steel pallet of some kind.

ANDREWS: Yeah, there's a little pallet.

KNOPMAN: And then there's ballast material. I'm just not--I don't think we're real clear on what that whole part of the system really is and how well it can be engineered.

But then also, if you can walk through what happens if the drip shield isn't there? Because, to me, it looks like you're getting what you need from the drip shield in the 7,000 to 11,000 year time frame, if I read your graphs right.

ANDREWS: A little longer.

KNOPMAN: Which means they need to stay up that long, and we really haven't seen much evidence presented that that's in fact what would happen. And those are both important components of your case.

ANDREWS: Let me--you've got a lot of questions there. Let me try the first one on the invert and it's characteristics and its contribution. I probably should go back to those conceptual figures, because they become very important. If it's advecting through the invert, so in the case where I have a hole through the package and a hole through the drip shield, that advective travel time through that one meter is not very long. There's no credit taken in these analyses for any absorption, for invert characteristics, no credit taken for any infiltration, you know, of the invert. So in case of advection, there's no invert performance added per se.

In the case of the diffusive transport, there is some credit being given to that invert. However, the diffusive characteristics are driven by the saturation in the invert, water saturation in the invert. That saturation in the invert is driven by how the invert and the rock hydraulically communicate, if they do communicate hydraulically. Right now, we are summing they communicate very well, so it becomes an equilibrium with the conditions in the rock. Bo showed you some pictures, conceptual pictures of cases where they weren't in hydraulic communication with the rock at all.

That's a pretty conservative assumption. So there's not much of a diffusive barrier in the invert itself, even in the absence of there being advection. That is, however, one of the unquantified uncertainties, and we're going to examine alternative ways of looking at diffusion through that invert.

One of the important aspects of it is, you know, what I alluded to on one of those slides, is when I get to the base of the invert, do I diffuse into a flowing fracture, or not? And as Bo pointed out, you know, 99.something per cent of the rock mass is non-fractured. So 99 per cent of the time, you would think it would diffuse into a solid rock matrix, with some saturation, not into fracture. That's a big difference. We're going to examine that conservatism as part of these unquantified uncertainty tasks. That's the invert, and its transport and contribution to the overall system right now, and what we're examining in terms of those conservatisms.

The drip shield itself does several things. One is it keeps there from being any advection into the package until such time as that drip shield is considered to degrade. And it does degrade. I mean, the titanium does corrode, just as the package materials corrode. And we have those degradation characteristics and models in there. So when it is still functioning as a water shedding device, I don't have any advection through the package, even if my package happens to be degraded, whether it's degraded at receipt, as in the case of those early package failure scenario, or whether I happen to have a package that fails at a stress corrosion cracking, you know, prior to the time that the drip shield fails.

That contributes of shedding the water away and its significance is somewhat a function of the diffusive characteristics and the assumption, those other assumptions I was talking about. Everything becomes kind of, you know, linked once you get inside the drift.

If I have that one representation that I had towards the end of the unquantified uncertainty, which is fairly, or perhaps a more reasonable diffusion barrier through the package, then the drip shield is buying you a lot. If I have a more conservative representation of diffusion out of the package and through the invert, you know, the drip shield doesn't buy you that much as a performance barrier. So it kind of then is more of a defense-in-depth kind of barrier, adding margin in the cases of some particular assumptions.

KNOPMAN: Thank you. A real quick followup. The assumption of 15 per cent dripping, 85 per cent non-dripping, carries through all the way through? I mean, I guess I've always been concerned about when the drip shield is still there, but you're already in a cool-down period, you've going to have condensation in the inside of the drip shield, in which case, they could all be dripping. They could be dripping on all of the packages, even with the intact drip shield.

ANDREWS: There is no--I mean, the seepage part occurs after the thermal period. We had a long discussion in

August, remember, about some assumptions we were making about how we got seepage during the thermal period. It's probably not useful to go down that path again here. But once I have seepage, then it's diverted around. The condensation under the drip shield is not considered--the thermal analyses that have been done, you know, the package drip shield combination, say the drip shield while it's cooler than the package, is always warmer and a lot warmer than the invert. So the possibility of there being any condensation under the drip shield for any reasonable period of time during--whenever I have a thermal gradient which lasts for a long time, is zero in the analysis. So we have no condensation underneath the drip shield.

RUNNELLS: Last question, Alberto?

SAGÜÉS: Okay, a question of clarification quickly on that picture. I presume that that scenario does not consider the drip shields in any way; right?

ANDREWS: The drip shields are removed, as well as the package?

SAGÜÉS: Okay, so that would be really the full neutralization?

ANDREWS: Yes, for 200 packages.

SAGÜÉS: Right. But basically that's what happens if you take away then most of the engineered barrier?

ANDREWS: All of them, the drip shield, the package and the cladding for those 200 packages.

SAGÜÉS: What would you say to the--if someone asks you then does that mean then that you tested the system for redundant barriers and found it to be wanting?

ANDREWS: Well, I think this event, should it occur, has a very low probability. If it does occur, it has consequences on the order of a few hundred millirems per year.

SAGÜÉS: What I mean is if you remove the waste package completely, then the mountain is not enough to contain the waste, because you will be getting doses that could be like 30 rem after 10,000 years?

ANDREWS: I think you have to look at what would be the doses if there was no mountain and no saturated zone. And we haven't presented those here. I think the repository safety strategy presented those, and they were like--somebody is going to have to correct me--but like ten to the twelfth rems, or something like that.

SAGÜÉS: But you still get like 30 rem even with the mountain, and that would exceed grossly--

ANDREWS: Yes, without any engineered barriers.

SAGÜÉS: Right.

ANDREWS: That's right.

SAGÜÉS: Okay, very quickly one other issue. This assumes absolutely that the whole approach doesn't take into account any possibility of biological action in the repository; is that correct?

ANDREWS: Any excuse me?

SAGÜÉS: Any possibility of biological action, like for example, mold growing inside after the breach in the package.

ANDREWS: You know, the in drift chemistry representation includes some biological component. You're getting outside my field, so--

SAGÜÉS: Transport, you know, like if you have some mold or something in the system, then in that case, the transport could conceivably be a lot faster than just diffusion. That's not conceived of?

ANDREWS: Not on the transport itself. On the chemistry, it was considered. I don't think it was considered on the transport. I could be corrected by someone who's closer to that part of the system.

RUNNELLS: And with that, we'll close the questions.

Thank you, Bob, and thanks for being so responsive to the Board's question. I appreciate it.

Our last presenter or responder is Paul Harrington, a project engineer in the Site Characterization Office, and he's responsible for overseeing the work on the repository design.

HARRINGTON: Before I start, I want to point out that the copies of Sheet 13 in the handouts were generally fairly light, so we had additional copies made and they're on the back table there, should someone not have picked them up yet.

Question 5 was fairly straightforward. What are the design objectives? What are the relative weights between them? And what are the trade-offs between them? (See Question 5 in its entirety in the Index.)

I'll address that. Given that we have not developed the answers to the extent that I think the Board was anticipating when they asked the question, we included some other information, some stuff that we had done in LADS that talked about relative weighting, and also some low-temperature scenario work that we have just completed that talks about trade-offs that we had to make between competing objectives. So we'll go through the objectives, relative importance, considerations, talk about flexibility, trade-offs, low-temperature, and that always brings up utilization of capacity. Do we have enough space to accommodate these different scenarios or schemes that we might need to use?

The objectives that we do have are relatively high level. We need to manage the uncertainty in postclosure performance, recognizing near field affects waste package corrosion rates. Recently, we came up with a change to the repository layout that would allow free drainage to try and reduce some of the concerns about potential water intrusion into the drift, manage the thermal effects on host rocks.

There's certainly uncertainties associated with that also.

We need to obtain reasonable assurance of a postclosure performance margin. We need to be successful should we do a site recommendation, should we try and make a site recommendation, we want to be successful in the licensing event that would follow that. So we want to have a high probability of that. That will be driven heavily by whether or not we can show it to be protective of public health and safety. That will be driven by whether or not our pre and postclosure exposures are acceptably low. And we need to have adequate flexibility to accommodate changes in the future.

We're all aware that our scientific understanding of the mountain, of the natural system, has improved over the past few years. You have seen changes to the design. To accommodate that, we can expect that we can continue to learn information. We have some time prior to a site recommendation. Following that, more time prior to a license application. And should there be a repository, there is quite a long time for performance confirmation. So we're looking for a design that's flexible enough to accommodate that.

Cost and schedule, it has to be affordable, be able to be built on a schedule that can accommodate the total system. It has to be constructable, operable and maintainable.

As we have scientific work yet to do, also a lot of engineering work yet to do, it's premature to try and identify right now specific objectives. The thing I'm really referring to are some sample objectives of whether or not we would focus on an 85 degree C waste package. Until we get a somewhat improved understanding of the mechanisms that would cause waste package degradation, of the environment that the waste packages would actually see, to try and choose that or some other specific value as a hard design objective at this point is premature. So we haven't chosen those sorts of hard ones. We're still using flexibility and the overall approach.

Now, I did want to bring up that at LADS, we have really had to ask ourselves many of the same questions, the LA design selection exercise from a couple of years ago. We looked at a number of different potential repository redesigns, ultimately selected one. We did that on the basis of several criteria. We ended up sending the Board a letter, and gave this ranking of those criteria from LADS.

Public safety was really paramount. Postclosure performance, licensing, demonstrability, preclosure worker safety. Now, in this--this is verbatim from the letter. At this point, if we were to redo this, obviously we would incorporate preclosure public health and safety. We can't ignore that. Flexibility and cost. That was the relative ranking from a couple of years ago.

The influences that will drive our determination are going to define the relative importance of those objectives. We haven't decided upon a specific decision process. Russ Dyer will talk to that tomorrow morning. We're still evaluating different approaches that we might take, and until we have the process itself defined, we can't provide the scaling or other parts of that decision process. But we are really focusing on acquiring new information, and making sure that we have a design that can accommodate reconsideration of objectives that have been important to us, be able to reassess decisions that we may have made.

These are some considerations that I believe we've shown to you before, I wanted to go over them again fairly quickly, that drive operational flexibility in the design.

Within the fuel itself, thermal content is driven by the enrichment, the exposure that it received in the reactor, the time from discharge, the individual--those all contribute to the thermal output of the assemblies.

Also contributing to that are the number of assemblies that we include in the waste package itself, the mix of assemblies, whether or not they're relatively fresh, relatively high burn-up, that would cause them to be hotter or older, that would cause them to be cooler, spacing of the waste packages. All of those drive the thermal loading within an emplacement drive.

The distance, the spacing between the drifts, the extent of time that we keep a repository open prior to closure, and the ventilation flow rates combine with that thermal loading to drive the near field thermal response. All of those are really features that can be adjusted, design parameters that we can adjust to achieve whatever the ultimate set of objectives are, down to a specific temperature, for example, on a waste package or a rock.

General observations. The lower temperatures we believe also would reduce uncertainties and localized corrosion, some of the rock alteration processes, coupled processes. There's some value to doing that.

Conversely, higher temperatures allow us to have shorter excavations. That would arguably improve preclosure worker safety issues.

Aging before emplacement, if we have very long ventilation periods, that doesn't play a significant role. That effect becomes very minor. Shorter emplacement durations, shorter preclosure periods, aging plays more of a significant role.

If we do leave a repository open for longer periods of time, multiple centuries, for example, certainly that introduces some concerns in the licensing process, just how that might be addressed. But also there are some introduced modeling uncertainties. Thermal profiles, for example, we think we can probably predict preclosure thermal responses more accurately for shorter terms. As that period gets extended, it gets maybe a little more difficult to do that.

If we looked at a relatively short preclosure period, 100 years or so, what you really need to do to get that is to space the waste packages out fairly wide, or have an appreciable amount of aging. Conversely if you go with surrogates like smaller waste packages. But those are the factors that really drive that.

If we go with a higher areal mass loading, that would allow us to consolidate the waste in a smaller footprint, and potentially use more advantageous places within the host horizon.

What is it that we're actually doing to address these uncertainties? Using low thermal loadings as one method of achieving lower uncertainties. We can get to that through several ways. We've addressed and defined a number of different design concepts to get there.

There are a number of scenarios that we could potentially use to achieve even an 85 degree C waste package temperature, both pre and postclosure. So in the SR, we will include, as a representative low thermal case, a design for that.

In developing those several scenarios that we reviewed, and this review happened over the last several months, and about a month ago, we went to the Plant Operations Review Board with a proposal for a recommendation for one of those to be the SR representative scenario, and that was accepted. That doesn't exclude the rest of the things from consideration, though.

But what do we have to meet? First of all, whether or not that particular approach would satisfy regulatory release criteria, whether or not it would achieve an average 85 C, or lower, peak waste package surface temperature, or maintain relative humidity lower.

In this discussion of waste package temperatures, what we have here really conservatively is looking at 85 as being the average of the waste package maximum temperatures. By extension, that means that some of them exceed 85 as a maximum temperature. We've done some other thermal analysis that says that the average is lower than 85. What I'm going to put up here, just consider that as 85 or lower as the average, or the maximum temperature of the average number of waste packages.

So we also want to limit rock wall to 96 C or less. And, yes, a comment was made earlier about that means that the rock would eat the waste packages up. No, what this is really trying to focus on is our interest in staying away from the concern over coupled processes, and the introduction of above boiling temperatures in the host rock. So these are not exclusive.

Achieve both of those criteria two and three with no more than 300 years worth of ventilation. That can be either using forced for the whole time, or passive, or some combination of them. Accommodate at least the 70,000 MTHM regulatory limit on waste material, looking at both the upper and lower blocks.

Most of the layouts that we present generally show the upper block, but remember that there's an adjacent area also that can be used. That's referred to as the lower block.

Also, to limit the surface aging of the fuel, try and minimize the amount of a facility that would be required to do that aging, and to maintain the areal mass loadings between 85 and 25 MTHM per acre. Those were the limits that we had established in the EIS for bounding purposes.

Given those requirements for development of scenarios, they each have to possess these attributes. They have to satisfy the criteria certainly. Also, they would need to lend themselves toward consideration of criteria of approaches from other scenarios. If one is more flexible in terms of being able to be used as a base for evaluating other thermal scenarios, that would rank it higher.

The next several slides are representative of different considerations that we looked at in these various criteria that we can adjust. This one happens to be a 70,000 MTHM emplacement, but spaced out at a lower thermal load than the referenced design. So that's 70,000 takes up most of the upper block.

We put in a schematic of the cell just to show for the natural and forced ventilation. Just as a reminder, we're doing these in sets of panels. Each panel has a series of emplacement drifts supplied by an intake shaft for that. It goes out and distributes across the two headers, goes in from each header through the emplacement drifts, down the down-comer to the exhaust shaft, then collected and taken out.

Now, given the height differential between the exhaust shaft and the emplacement areas, and even the intake shaft, we think that will work as a natural ventilation feature also. The primary thing to take out of this is that cell as a way to approach it.

Now, one comment I would make on natural ventilation, the thing that I think we really need to focus on in looking at long-term ventilation is the thermal characteristics that we're trying to achieve and, therefore, the flow rates that we need to achieve those thermal characteristics. Whether or not we're actually 50 or 150 years out, able to achieve that naturally, or if we have to turn on a fan, I think the real thing to focus on is the temperature criteria, not whether or not we can achieve it simply by natural ventilation.

I put this in here to talk about waste package temperatures, as the 1.45 kilowatt per meter reference case versus the 1.0 kilowatts per meter, spreading them out to achieve a 1.0, you can do that with smaller packages also, would achieve lower than 85 degree average maximum waste package temperatures. With the 1.45, it goes up about to 160. So that's another mechanism we can use to accommodate that.

This one is to look at the relative effect of aging versus spacing of waste packages. For the spacing, that doesn't exceed about 70 degrees. But if we do aging on the surface prior to emplacement to bring the at emplacement thermal load down to the same 1.0 kilowatts per meter, because it's continuing to generate heat, it ends up at 94 C, which is very near to what the reference case was. So our mind is with the longer term preclosure period, the aging has very little effect relative to spacing.

Now, this table I'm going to put up here also. These are the several scenarios that were evaluated. There's the reference case, and then we did six of them. And go to the next, please. This is really the major attributes of those. The first one is--

RUNNELLS: Paul, just a warning, about five minutes.

HARRINGTON: Okay. There's really a combination of a number of different variables with a relatively small adjustment to each. The next one was looking at smaller waste packages, an appreciable difference. This one takes the waste package, or the drift spacing out from 81 to 120 meters. The fourth one spaced the waste packages quite a bit further apart, six meters. The fifth did the surface aging of waste prior to emplacement to achieve the 30 year average. And the sixth said let's just leave this thing indefinitely open to take advantage of whatever natural ventilation flow would occur to remove both heath and humidity.

Now, the TSLCCs, or the cost estimates, are kind of interesting. In the interest of time, let me jump to the next one.

We did end up selecting Scenario Number 1 as the representative case. The reason for that really came down to a philosophical discussion between do we take something that, as in the case of Scenario 1, made a number of perturbations, but is a reasonable starting point for evaluation, not only of that, but also that can include the salient features out of the others, or if you're looking truly to do comparisons, we only vary one parameter, and look specifically then at the effect of that. So what we ended up doing was selecting the

Scenario 1 to be the reference case for the low-temperature approach within the SR, but we will also do evaluations of the significant features from Scenarios 2 through 5. We'll do that as modifications to Scenario 1. Scenario 6, because of the indefinite closure period, was not going to be considered any further.

The thing to take away from this, though, this isn't a specific choice between this specific lower temperature design and the current reference design as being the hot/cold decision. This is what will be evaluated in the context of both it and the representative features out of 2 through 5. We use that as the basis for the higher versus lower temperature considerations.

We've shown you this sort of curve before. That was based on 96 degree rock wall temperature. This is based on 85 degree waste package average maximum. It's similar, but the spacing has increased substantially.

That brings up can we make it? Is there enough inventory there to actually accommodate it? The answer is still yes, even looking at four meters, the 70,000 regulatory inventory would still fit. It would fit at two meters, even strictly in the upper block.

So, we reviewed what we do have in terms of the objectives for where we are with the development of the additional scientific and engineering testing work. We think it's appropriate. We haven't yet established the relative importance of those, or more specific criteria.

We do think, though, that we can come up with a design that can accommodate both thermal considerations. Whether or not the current reference case can be shown to be acceptable or if we do need to ultimately change to a cooler case, we think we can do those. And we need to retain flexibility to accommodate information we learn in the future.

So, with that, I'll take questions.

RUNNELLS: Thank you, Paul. I think you did an excellent job, especially of pointing out to us the trade-offs that are involved. Without the specifics of the design, you nevertheless gave us a very nice overview of the trade-offs that are involved.

With that, we'll open up questions from the Board. Priscilla first, and then Jerry.

NELSON: This is an easy one. Just for clarity on Number 20, Slide 20, can you indicate--this is Nelson, Board. Sorry. Can you indicate your design that you're going to go forward with for the low-temperature on that chart?

HARRINGTON: Well, it has zero years of aging, and it had two meters of spacing, if I remember. So it would be, oh, let's see, it's actually not represented on that chart, is the best way to say it. This chart is forced ventilation.

Okay? And what we're taking forward as the base representative case was 50 years of forced and another 250 of natural. So it doesn't really follow on the chart.

NELSON: You know, what I was going to guess was--this is what I was going to guess, based on all the discussions and my understanding of the chart, was that it would be 50 years of forced ventilation, two meter spacing. And because it takes 25 years--or it takes an amount to load, there is aging involved in the loading.

HARRINGTON: That's true, but only for the first set of fuel. Most of these things occur, when we talk about aging, that's really after emplacement of waste. So as we do the thermal analyses and say 50 years of ventilation, for example, that's after the last package goes in. So, yes, you're right. The first package effectively has had aging for the emplacement duration. But the last package doesn't.

NELSON: So this case is not on there?

HARRINGTON: No. No. We talked about how to incorporate the natural ventilation on there, and it would be a whole stack of slides. So I did this really as an update of what we had shown you before. But the constraint on this is it is only focusing on forced ventilation. I didn't do one that would have the 50 forced, plus a period of natural.

RUNNELLS: Jerry Cohon?

COHON: I'd like to go to Slide 3, please, which is the list of the objectives. I have two comments or suggestions about this. One is that I would suggest that the first two bullets, that's manage the uncertainty, manage the design to obtain reasonable assurance, are really one, and it's basically have a design with an acceptable level of uncertainty. Now, reasonable assurance may be an expression of that, but I don't see that they should be thought of as differently.

HARRINGTON: There's a lot of truth to that. However, if I knew with absolute certainty what the performance of each feature of the facility would be, I would still want margin. That's why we had them separate. But, yes, they're very related.

COHON: I see. Okay. Well, if you knew absolute certainty, then I guess the margin wouldn't be so important, because you'd be absolutely certain. You'd be able to absolutely predict the future. So they are variations of the theme.

But that's actually interesting, and maybe--I think it's actually informative to combine them and maybe make them subsets of an overarching one about an uncertainty, so that there's some acceptable level of uncertainty, and there's some performance margin.

The third one, high licensing probability/protective of public health, certainly license ability is an objective or criteria, and I wouldn't dispute that. But combining them with protection of public health is probably not a good idea, because, I mean, public health protection is part of the licensing process, no doubt. But licensing includes more than that, and we know that you care about protecting public health. That should stand by itself.

It also invites cynicism to present it this way, because if someone were to grant you a license right now, poof, say by act of Congress, that doesn't say anything about protecting public health, yet we know you care about that as a separate objective.

I'd like to just move to Number 5 where you present the criteria from the last LADS process.

HARRINGTON: Yes.

COHON: And simply observe that except for uncertainty, you've got it; right? This is basically the same as--these correspond nicely to the objectives we just talked about, except you mentioned the point about preclosure public safety as well as worker safety. The only thing missing from that list is treatment of uncertainty?

HARRINGTON: Yes, that's right.

COHON: Thanks.

RUNNELLS: Dan Bullen?

BULLEN: Bullen, Board. Could we go to Slide 14, please? And at the risk of not being consistent, I could ask

Paul what's my question?

HARRINGTON: Why is the ventilation underneath the emplacement drift? Would that be it?

BULLEN: That's exactly right. Since you showed us this slide, I just have to ask that question. Why is the exhaust main below the drift instead of above the drift if you want to take advantage of the natural convective forces?

HARRINGTON: The simplest answer to that I think is the head difference between intake and exhaust. And what else goes on around here right--the last time we had looked at this, we still had the performance confirmation drifts above the emplacement drifts, and there was a lot of rationale for that. It's easier to come down and observe from the top rather than trying to do it through the invert and that sort of stuff.

The loss of efficiency--well, backing up to the PC drifts, if we had the ventilation shaft above there, there's some interferences. It was a little more difficult, not impossible. Also, there was a concern that having the ventilation drift above would provide a conduit for water to collect in that ventilation exhaust, and then enter the emplacement drifts. If you had it below, you wouldn't have that problem.

Having the exhaust main below, yes, arguably might have some reduction in efficiency of the ventilation, of natural ventilation, and that might be why I made the comment I did about let's not focus just on natural ventilation. The real key is not whether or not this thing can work all by itself without a fan, but whether or not we maintain the thermal goals.

BULLEN: I agree, and I'll just consistently ask the question as long as I keep seeing the same figure.

One other quick question--

HARRINGTON: Actually, tomorrow morning, you might hear something that it's going to be re-assessed.

BULLEN: Can you go to the next slide 15, please? These are very intriguing calculations and I'm very pleased with the effort that you've made to take a look at trying to maintain the waste package surface temperature at some threshold for whatever reason. I guess the question I have is based on the information that you've got from the drift scale heater test, for example, and the integrated energy analysis of where the heat goes, how much confidence do you place on these kinds of calculations that this would indeed be the temperature that you'd see?

HARRINGTON: Moderate. Another thing we've been saying today about uncertainties applies to this, and these were some fairly rough calculations based upon 2D ANSIS models, and that's why we're going off to do the additional work, is to try and scrub that. That's why I said it appears that we can come up with some designs that could accommodate 85, but all of the uncertainty issues that we've been talking about will drive whether or not that's ultimately possible.

BULLEN: Okay. I guess the follow-on question to that is how conservative are these calculations? Did you push it to the max, or are these essentially going to be as hot as it would be? Or do you think that the fact that you can't integrate 20 per cent of the heat of the drift scale test might lower these temperatures some?

HARRINGTON: With respect to this, because it's 2D ANSIS, that's conservative we think relative to what a 3D case would be, relative to what NUF shows. Typically, the NUF shows cooler temperatures. 3D, where we actually look at the effect of even distribution of heat down the emplacement drift, of the ventilation, all of that would say this is conservative. But there are some other potentially non-conservative things, like thermal conductivity, and we're reassessing that, and especially the wet conductivity may well change.

So at this point, it would be real tough to say that is wholly enveloping, if it's bounding. I think it's best to say it's representative, given what we know now. And it may go either way, depending upon how all the conservatisms sort out after we do the additional work.

BULLEN: Thank you.

RUNNELLS: Any other questions? Yes, Richard? Hold on. Debra was first.

KNOPMAN: Knopman, Board. Slide 17, Paul. I realize these are very preliminary numbers here, but let me just focus a little bit on cost, because what you show consistently at the bottom line here is increased cost for all of these lower temperature scenarios.

Somewhere in your material, in the program material, I saw the suggestion that perhaps a drip shield would not be needed in a low temperature design. To what extent are you actually thinking about differences in operations, beyond just changing one parameter at a time here, so that you'd actually get a different picture of costs? That also goes for the question of the 81 meter spacing.

Now, there may be a reason to keep that for flexibility purposes, or in the event that there was something that went wrong with ventilation and you ended up with higher temperatures and still wanted to take advantage of getting between pillar shedding of water, but it would be useful to just hear you explain a little bit about how you're thinking about these cost estimates at this preliminary, very preliminary stage of the analysis.

HARRINGTON: That was kind of a two-part question, three really. Part of it was should drip shields remain in, especially if they're a significant cost driver. And are we doing something to reassess that? Part of it was just kind of what drives these costs. Let me address the second part first.

This one, Scenario 2, with the smaller waste packages, there's more of them. So even in net present value, that one goes up appreciably. This one, Scenario 4, the much increased area and length of excavation is really what drove that. This one with the 30 years of aging and the facilities needed to do that and the handling and stuff, I think that's primarily what drove that. A lot of that's near-term stuff.

The others are relatively low because they're kind of operational changes, not heavily different than what the current base case is. Yes, we did space them out more. Yes, that meant we had to go to some additional drifting. But it's not a great deal. There's also the extension of the preclosure duration, up to 300 years, versus the shorter duration that had been in there earlier. That's kind of what drove the cost.

As far as things that are contained within there that we could remove and reduce it, such as drip shields, we're continuing to assess whether--well, what the contribution of drip shields are, both from a performance perspective, defense-in-depth perspective, and cost perspective. So drip shields specifically are something that are being continued to be assessed.

PARIZEK: Parizek, Board. There's a figure, again 17 is dealing with costs. There's obviously a length of drifts that vary, and my question relates to this. I mean, obviously, you could pick drift spacing to shed water. You can also do it to kind of reduce the loading, thermal loading. But other than, say, offsets to major faults, which is a place that I guess you won't go, if you get to a known big fault, you're not going to mine into it and have a drift cut into one. There's a set-back requirement for major faults?

HARRINGTON: Right.

PARIZEK: Is there any other reason to reject any part of a tunnel which you don't have yet, and some of the block, you know, not tunnel obviously, but if you come to something you might not want to use, and as a result, the length of tunnelling goes up and, therefore the risk to workers goes up again because more tunnels, more risk, but at the same time, it adds to the cost in a way that you couldn't really say right now. Is there any intention at all to say there's a fatal flaw in this piece of the repository, therefore, we're not going to use that section?

HARRINGTON: We're trying to not get into that situation by doing the characterizations that define where the faults are, and then define a block to fit within those. And that's really the definition of the east side, was the Ghost Dance. The west side is Solitario. The south end was overburden, and the north end was the rising water table. So within that, we're looking at the individual faulting.

At one point, we had a standoff requirement, I think it was ten meters, or something, from large faults, just so you would not have a waste package right there. Other than that, I think the expectation is that given that we've bounded the perimeter within problem areas, we think the resultant area is probably pretty good and we shouldn't have too much in the way of difficulties.

Now, these sorts of layouts also are counting a 10 per cent contingency, just to accommodate that sort of surprise, should we have some local area that we did think was problematic, didn't want to put a waste package there. They layouts, the utilization of capacities always allow 10 per cent for that.

PARIZEK: I didn't realize there was a 10 per cent. Again, to the extent that you know the block, it's one thing. When you actually get underground and there's kilometers and kilometers of tunnel, who knows what you're going to really see in some sections there.

HARRINGTON: Right.

RUNNELLS: Priscilla, do you have a short question?

NELSON: Yes, just short, and I think it's a follow-on to Debra's hope that this is a similar table to the one we've seen before that related the outcome of the LADS exercise. And at that point, we had the concern that a scenario would be selected, but not really be designed for performance under the different conditions that represent your design goals. So that there might be several things different about a low temperature design, even if you fix the spacing, that advantages that could be taken in that case that wouldn't be taken in a hot design, and it's a real thinking from a blank sheet of paper about how you'd use the best qualities of the rock in that environment, that we're I think hoping would actually happen, and develop a rationale so that it would truly be a design, not just a change in temperature.

HARRINGTON: That's why we're looking at not just this Scenario 1, but the features out of 2 through 5 to see how they affect performance and whether or not it would be appropriate to include some inclusion of that attribute in a final design.

NELSON: I guess, and I don't mean to cause a response, but at one point, there was a discussion, for example, about characteristics of the invert, and using certain materials in the invert that might actually be, what were they called, I was going to say--but kinds of materials that may be functional at lower temperatures that would not be functional at higher temperatures, that may do some other things.

HARRINGTON: We can use that as a segue into tomorrow.

NELSON: That's fine. But, I mean, just from the standpoint you've got a couple of really physically defined variables, and if we're going to try to include everything, we're never going to get a design that's really tuned to the possibilities for low temperature at that site. And it deserves to have a chance to be tuned.

RUNNELLS: Paul, with that, we're out of time. I want to thank you very much for your presentation and answers to the questions. Thank you.

I want to apologize to the Board staff. We have run out of time all day long, haven't given them a chance to ask one question, as I recall. Let's hope we do better tomorrow.

I want to thank very much all of the people who have presented today. I think the intense preparation shows, and I think by and large, folks were very responsive to our questions. So, thanks very much to everyone who gave a talk.

COHON: Thank you, Don.

We turn now to the public comment period. We have five people signed up. Let me just confirm this time so that I don't have names that I shouldn't have and make sure that we didn't miss anybody. I have Corbin Harney, Leuren Moret, Judy Treichel, Bill Vasconi and Sally Devlin. Correct? Did we miss anybody?

(No response.)

COHON: We can be a little more casual, because this is the end of today's meeting, casual on time, I mean. And I would ask each of you, though, so we can end at a reasonable time, to try to limit your remarks to about ten minute. We'll start with Corbin Harney.

HARNEY: My concern is always about my land. I still own the land that we're talking about under the Treaty of 1863. I never have been compensated for it, like some people are saying, but I've been asking people show me the documents where you own the land. This is really a concern of mine because my forefathers lived on this land for thousands of years.

What you guys are doing is showing a good picture, a good picture within the framework of that good picture, but it seems to me like that we're not concerned about the life that we already have taken. It goes into millions and millions of lives that's been taken by radiation, but we continue to talk about it, how good it is, but we're not concerned about anything, it seems to me like.

I don't know whether we're here to destroy this mother earth of ours, what's on it, what survives on it. It seems to me like we want to destroy the whole life on this earth. So we're doing a good job so far that I see. I think most of us know that. And today, some people making their living on this earth of ours, trying to take care of it as much as they can, because this is where their bread and butter comes from.

Today throughout the country, I see in all cafes milk is already contaminated with radiation. Our food today is contaminated with something else. So what now are we going to come to, or aren't we going to ever come to? Are we just going to be the guinea pigs for the Nuclear Energy Department? So far, that's what it looks like. This is something that we the people are going to have to talk about it. Tell us, the Nuclear Energy Department should tell us that they are using us as a guinea pig.

The more we talk about those things, it seems to me like we're getting into more dollars, trying to keep this Yucca Mountain open. We're not sure of what we're doing. We're going from day to day thinking about we're going to change it here, change it there. The only one making money at it right now is the contractors, digging into your pocket to make it work. Whether it will work or not, we really don't know. I don't think anybody knows.

Somehow, somebody should start telling the truth, not do a guesswork at it thinking it might work, and it might not. Those are things that I hear from the people that's employed by the Nuclear Energy. This is something that we've got to think about. The nuclear waste here is another problem, a big problem. It's going to come from throughout the world here. We already have accidents. There's no 100 per cent guarantee. It might be 50 per cent guarantee.

But like I say, my concern is the life on this earth. We should be the ones that really take care of this earth of ours because we all survive on it. It gives us our food. It gives us plenty of water, clean water at one time, clean air, and so forth. But today, we're contaminating everything on it. So far, everything on this earth today, the life has been taken by radiation.

Ladies and Gentlemen, think about it. Think about what can we do to make it better. There is a cleaner way for energy, power. The more we use nuclear power, it's going to contaminate more. It's going to accumulate more waste. Where are we going to put it. This Nevada state ain't big enough to carry all the nuclear waste.

So, people, think about it and see what we can do together and talk about it. Let's not say you're different than I am. We're all here together. Let's all work together as a people, because this is what this earth put us here for, to take care of it, take care of our water, think about our young people, younger generation, and so forth.

I hope to see you guys again, and make it better than what it is today. Don't do more guesswork. Don't say if it will work, and if it don't, it's too bad.

Thank you.

COHON: Thank you, Mr. Harney. Leuren Moret?

MORET: Thank you. I'll just finish my open letter, and it is on the web at http.www.native web.org/pages/legal/Moret.html.

COHON: Will you also leave a copy, though?

MORET: Yes.

COHON: For us to put in the record.

MORET: Yes.

COHON: Thank you.

MORET: "This is regarding Rosalee Burtell and her estimates on cancer. In her estimates of fatal and non-fatal cancers, they are more than doubled if skin cancers are included. This indicates that elevated skin cancer rates at the Livermore Lab are just part of total cancers for lab workers, and that the lab is under reporting cancer rates. Politician, government experts, scientists, and the radiation protection industry are telling us we have nothing to fear. Dr. Burtell's book, "No Immediate Danger, Prognosis for a Radioactive Earth," revised 2001, reveals how the nuclear industry massively under estimates the real cost to human health, and hides the victim with restrictive definitions of radiation caused illnesses.

Poor bureaucratic solutions to high level radioactive waste will increase the numbers of victims of the nuclear age. The transport of high level waste is also a critical issue, particularly after comments from the audience at an NRC public meeting on packaging and transportation of radioactive material held in Oakland, California on September 26, 2000. During the discussion a man in the audience wondered if anyone had information about a lost railroad shipment of fuel rods. Another woman spoke up about a lost railroad shipment of fuel rods in casks, which had been missing for one week last summer. She said it was finally located in Sacramento. The man said he was talking about a lost shipment in Nevada. And the other night in Pahrump, I heard there was a lost shipment in Texas.

Neither Bill Bracht from NRC, nor Fred Ferarti, Department of Transportation, had knowledge of any lost fuel rod shipments. With 100,000 shipments over the next 30 years, further unnecessary exposure of citizens will occur when the responsible agencies are not even informed, and cover-ups preclude developing better tracking methods. Citizens will be exposed and never know it.

The 2000 World Conference against Atomic and Hydrogen Bombs was held last August in Hiroshima and Nagasaki Japan. Thanks to Judy Treichel, I was invited to speak at the plenary session about Yucca Mountain and high level waste issues. The title of my talk was "Yucca Mountain, Moving the

Goal Post." It was a new and rewarding experience for me as a scientist. I was invited to visit communities in Japan where their Yucca Mountain will be forced on unwilling citizens. We had town hall meetings, visited city officials, and held press conferences, talked to activists and visited proposed siting facilities.

When I was leaving, the citizens told me you are the only honest scientist we have met. That was very sad for me to hear, especially after I had seen how they were able to use the scientific facts and information I gave them to challenge their elected officials in order to make better decisions for future generations. I have sent a binder of my trip through Japan, speaking about Yucca Mountain to Congresswoman Shelley Berkley, and hope that she will feel energized and encouraged to continue her fight for the citizens of Nevada.

The Japanese people are in solidarity with Nevadans. You made a comment in your report about the need for a scientist to step forward and speak out on issues. Recently, I have read three books which reveal the demonization of scientists who act with ethics and integrity and the politicization of science on nuclear issues, "The Woman Who Knew Too Much, Alice Stewart, and the Secrets of Radiation," by Gayle Green, 1999, "Making a Real Killing, Rocky Flats in the Nuclear West," by Lynn Ackland, 2000, "Fire in the Rain, the Democratic Consequences of Chernoble," by Peter Gold, 1990.

These books are insightful about the public policy and ethics of nuclear issues, and the need for scientists to take personal responsibility and act in the best interests of the citizens and communities who are most affected by irresponsible bureaucratic decisions.

I hope that we can work together to bring this message to scientists through scientific society participation at GSA next fall, and encourage scientists working on nuclear issues to take personal responsibility. The article in the May/June 2000 issue of the Bulletin of Atomic Scientists by Robert Alvarez, formerly of the DOE Office of Public Policy, sums up DOE priorities.

In the fall of 1995, I found myself in a hallway facing down an angry senior Energy Department career officer, after I blocked a deal that would have allowed some 10,000 tons of radiation contaminated nickel from nuclear weapons operations to be recycled into the civilian metal supply, where some percentage of it would inevitably wind up in stainless steel items such as intrauterine devices, surgical tools, children's orthodontic braces, kitchen sinks, zippers and flatware. However, that confrontation was not to be the end of the scrap metal gambit.

He describes more politics before a decision by

Richardson. In February, Energy Secretary, Bill Richardson, put a hold on releasing the contaminated metal from Oak Ridge and proposed a moratorium on releases at other sites. It looks as if regulated landfills will be the next stop for the contaminated metals, and that the Energy Department will have to eat a few hundred million dollars in disposal costs.

A postscript. The Oak Ridge manager who orchestrated the BNSL recycling contract received a presidential meritorious rank award in 1998, which cited his efforts to recycle the metal. The award carried a $10,000 honorarium. He retired in the summer of 1999 and is now leading a BNSL subsidiary, Westinghouse Government Services, which secured a contract to run Oak Ridge's Y-12 plan.

Minimum cost is the bottom line DOE concern, not the children of tomorrow.

Thanks for your careful study, serving the community interests, and presenting a model for responsible government and democratic decision making. It is about ethics and personal integrity. And these are the words of the peace maker, founder of the Iroquois Confederacy, Circa 1000 A.D. Think not forever of yourselves, Oh Chiefs, nor of your own generation. Think of continuing generations of our families. Think of our grandchildren and of those yet unborn whose faces are coming from beneath the ground."

Thank you.

COHON: Thank you, and thank you for your willingness to give your letter in two installments. And do leave us a copy, please.

All right, next, Judy Treichel.

TREICHEL: I can do it tomorrow.

COHON: Okay, Judy, thank you. Bill Vasconi?

VASCONI: Bill Vasconi, construction worker. I've lived in Nevada for 37 years. I have six grandchildren, three kids, live in Las Vegas, Nevada. I worked the Test Site approximately 17 years as a radiological technician and monitor. The rest of those years was as a construction worker, electrician by trade, and as a general foreman, probably participated in some 100 events at the Nevada Test Site.

The Nevada Test Site has had a long history of repositories. We have 928 nuclear devices detonated at the Nevada Test Site. Of those, 828 are underground. 24 of them was with Great Britain. This was their test area. Not all of them detonated, not all of them were out of the water surface, approximately one-third of them was below the water table. They say it was a closed water aquifer. That gives me some relief.

Now, the reason I'm up here this evening is because I want to address a comment, maybe it was a question by one of the Board members. The terminology used was monitoring.

You know, we've been looking at this Yucca Mountain project for approximately 15 years, and throughout that time, I've been a part of it in one way or the other, because I, irregardless of what you read in the newspaper, I'm one of the Nevadans that see Yucca Mountain as a viable solution to this nation's nuclear concerns, and if it's scientifically proved sound, I'm in favor of it.

But back to the comment that was made on monitoring. You know, in the beginning they were going to concrete Yucca Mountain and plant natural vegetation on it and walk off and leave it. But had I been sitting in this audience this afternoon, I would assume that's what we're going to do again.

Now, monitoring, let me break it down into three quick things; reason, research and resolve. The reason for monitoring? Well, it's assurances of health and safety. Environmental concerns of not only the people of Nevada, but the citizens of the United States. Research, consider this if you will. Research, we can put probes in there. We can have diagnostic facilities. We can call it a mini-lab if you want to. But for generations to come, we know the temperature inside. We know the water content inside. We'll be able to look at it and analyze that there's fluctuations in radioactivity. Studies. The resolve? What if the case shows that the resolve shall be extraction, removal? That capability must be maintained, not only for the reason that we may have troubles with the canisters, but, you know--the system will have a lot of credit.

But what we're doing today we assume is going to last for 10,000 years. Hey, I have three kids, they all have a college education. I'm an old construction worker. They're all smarter than I am now. They all have college degrees. The worst thing of that, they all turned Republicans. I can't justify that. They learned to work with their hands or their brains. I can't tell them what to do. I can spoil the hell out of six grandchildren, and believe me I sugar them up before they go home, they get candy bars, soda pop, I'll get even with those kids.

But the bottom line is what we're doing with today's technologies does affect our future. Right, Nevadans feel maybe they're not a part of the problem, but they may well indeed be the solution for generations to come. And our educational system I give more credit. You know, three or four years down the road, they might have a lot better idea to know what to do with that stuff. It may well be a renewable energy source. I want you to convince this old man that coal and oil is going to be around for the next 200 years. You can't do it.

You had a man from France speak a little while ago, or this morning. I believe France has 59 nuclear reactors.

Apparently France is an exporter of nuclear energy. We heard about high temperature reactors, mutations. Well, maybe the test site is the place for that. Maybe it can generate a little electricity over there. I think that California would well receive it. They'll take that electricity if we generate it at the test site. May have a problem with water. You don't want to talk about that.

At any rate, I'm an old country boy, but realistically, you know, we've got the mountain, we've got the management, we've got the manpower. We've got 50 years of expertise working with nuclear to do the job right, health and safety, scientific issues.

I want to thank you folks for coming here. I want to thank you for having an opportunity to address you, while I'm not near as technically involved as the rest of you are. Hell, I can't remember some of the terms you use, let alone what they meant. But you give me a chance to welcome you as an old country boy and tell you, hey, there's folks that believe in what you're doing. And I use you, I use the National Academy of Sciences. I hope the NRC is listening. I hope EPA is listening. Let's get it right. But beyond that, let's get it done for the sake of the nation.

Now, one other thing before I leave, because I always do this. You know, we have rural counties out here. Now, you see a lot of empty space. But, believe me, folks, those rural counties are for real. A lot of them believe in what you're trying to do. You say, well, how serious can they be about this? There's not that much of a population involved. No, some of those rural counties don't have much of a population, but keep in mind that that rural system, that road system goes directly through that community and affects 90 to 95 per cent of their population.

Don't be afraid to say we suggest more funding to your rural counties, which I'm from Clark County, they don't need any funding, they've got the industry down there, the gambling industry to take care of them, but the rural counties, they could use that money. Impact studies, environmental studies, don't be afraid to suggest it.

No, I don't live close to Yucca Mountain. Again, I live in Clark County. Realistically, we're pretty safe there. We only have a murder every other day, a rape every nine hours, a car stolen ever 40 minutes. Why would I want to move out here where it's dangerous?

My biggest concern is crime, school, water, transportation. About 14 on the list is a place called Yucca Mountain. At one time, I was talking in a group and I said, well, what does YMP stand for? They knew. I said what does NTS stand for? A guy raised his hand right away. That's easy, no to smoking.

Folks, 50 per cent of the people out of 1.3 million people in Clark County have been there less than ten years. I've been here for 37. I believe in what you're doing. I believe it will work. One more time. Let's get on it for the sake of the nation.

COHON: Thank you, Mr. Vasconi. Sally Devlin?

DEVLIN: Thank you very much. Thank you, and I love to look at everybody. That's why I stand here as a good toast master. And I want to thank everybody again for coming to Nye County, Nevada, to Amargosa, and I hope you enjoyed the beautiful sunset in this beautiful area, and that you see how lovely we are. And I cannot tell you how delightful this meeting was. I thoroughly enjoyed all the modeling and all the lab stuff and all the update on all this that I haven't heard for quite a while.

The only problem is, and of course I have to yell at you, as I always have for the last eight years, is that we're having two repositories. I didn't hear anything about the second repository. This is in all the papers and the Congressional, everything. And remember you're saying 70,000 metric tons, and I am saying 77,000 metric tons, and 14,000 of those are DOD, and you cannot put classified waste in my mountain. I didn't hear anything about that from anybody, and I must remind you and yell at you, as I always do, on such a serious omission.

Now, the third thing is, and I think it's just absolutely wonderful, that roads are going to go into the test site, 8.9. Right, Russ? 8.9 million? You're supposed to know these things. It's in the transportation report that I just got. Anyway, this is very nice.

Of course, you know my whole field is transportation, and what Bill said, you know, get on with it, and so on, my numbers to you and my report to Wendy, of course, were because a trillion dollars for the roads, it would cost $50 billion for the canisters, and God knows how much for the other stuff. So we're probably talking $200, $300 billion in the next few years.

And, of course, my lover, Abe, he is going to protect all the current population and we don't worry about the future. So it's only money; right? That's kind of funny.

But I really came here for one thing, and as I told you about becoming our own assembly district and county, and this is terribly important, and I want you to know how this came about, because I am not a native Nevadan, but I've spent most of my life here, and it came about because the CDC, the head of it, Dr. Johnson, said we're going to have bio terrorism and pandemics everywhere in the world. And we only have twelve states in the nation in the telecommunications loop, and of course Nevada is not one of them. We have no intra or inter telecommunications, and so this prefaces all my remarks, and I'll leave you my papers with you.

What I am saying to the State of Nevada, you're 20 years behind the times now. You've got to do something about it or we'll be 40 years behind the times. And I thank Dr. Bullen especially, and many others who have given me papers which I have presented to the legislators, because they're the ones that do this stuff on virtual schools, virtual medicine, and virtual libraries. So that this is the world that we're going to be living in, and we'd better be prepared for it, and I just bought a computer and I'm enjoying it. I push all the buttons and goof it up and do all kinds of terrible things. But somehow we get through, and I hope to get e-mail from everybody now that I have two sites.

What is amazing to me is that the reason that I'm really here, and that is to talk to Bechtel. Are they still here, Mr. Hess? Are you still here?

Good. All right, stand up and take your licking. And the reason I am saying this is in all the years, and it's been a very long time, Nye County has never gotten anything from Bechtel. And I did a little homework because as you get older, you get wiser, it says, a little bit. Not very much, but just a little bit, a little pregnant. And what I learned was you've done many contracts with many areas, and you've always given them something, particularly EEL and Idaho Falls.

Well, you know what Pahrump has gotten from Bechtel? One April--one Christmas in April, you did fix up my girl friend's house. That was charming.

And that said, now, we have the world's worst roads. I've done a million reports on it. We have all that stuff. But the most important thing, and I just did the demographics for the State of Nevada, for Mark Hemmings, because we're going for a certificate of need for a hospital, because I hope you all are aware that there is absolutely no medicine in Nye County. We have a private hospital in Tonopah, which was given to them for $100,000. And when they had the accident with the British bus load of people, 41 people in the bus, had they not had this private hospital, they would have all been dead. They took wonderful care of them, got Flight for Life, and all kinds of stuff. Had that same accident happened in Pahrump, they would all have been dead because we have no medical facilities.

Now, I have been yelling at DOE and I've been yelling at TRW, and now it's my pleasure to yell at you. I want an agreement, because you have two weeks before your contract gets through, and I will personally take you to court, and I've found out all kinds of--color of office, and what have you, because we need it. There are 18,000 to 20,000 flights over NTS a year. We have a nine hazard road, which is 95, which all this stuff is coming down. We have a seven hazard road, which is 160, and we are supplying the EMTs, the fire, and everything for 2400 square miles.

Inyo County, my friends who are here, are broke. We are supplying the fire, and so on, and they will continue that way because everything is dying. And it's a very serious situation. So I am saying to you in front of God and everybody, and all these guys know I consider them God, they're wonderful, we want your money, we want your impact, and we want 50 million at least, which is what I asked for, because we need it. And we also want you involved with the community, which we have never had. And if we are going to be are own county, it will be from the Tonapah Test Range to Mountain Springs. That's only half of Nye County, and we would be called Mercury County. And it's a real possibility and we're really serious about this stuff.

So I feel you are obligated to us because we have nothing and we're going to have to learn to manage on our own, and we have only one requirement that I have set, and that is that nobody that runs for office could have been appointed by anybody from Nye County, can have served as an appointment for any committee, and can have been elected any kind of public office. So we're going to have people that we'll train and learn and that's who you'll be working with, not politicians who we'd like to have grand juries investigate. So we're having fun.

We also wanted to be specializing in radionuclide poisoning, and so on, and it's got to be a teaching hospital, it's got to be a work together hospital, because our people need the jobs, and we want everybody to remain there. We are going to be, and this is the major number, over 120,000 people in the next 20 years. That's bookoo people, bookoo needs, and bookoo interest in what's going on. And I do live in the shadow of Yucca Mountain, and we need your help and we need to work together.

So you've been properly yelled at, and now you're indoctrinated. Thank you, and I'll give you my card and we'll get together. We're having a meeting tomorrow night at 7:00 at the community center, and I'll expect you and your entire staff. And with me, you're lucky to get 24 hours notice. Right, Russ?

So thank you. And thank you all again for coming. See you tomorrow.

COHON: Thank you, Ms. Devlin. We should all be thankful that the press in Nevada does not engage in selective reporting. Here are some of the excerpts from Sally's comment. "My lover, Abe. Little pregnant. Fixed up girl friend's house. Take you to court. We want your money. $50 million at least." That would make quite a story.

I understand that one of the questions I asked during--after Paul Harrington's presentation, might have been misinterpreted by some, and I want to make sure it wasn't misinterpreted, because it's an important issue.

I asked about probability of licensing as one of the objectives, and made the point that I thought public health should stand on its own. Some people seem to have interpreted my comment to mean that public health protection was not part of the licensing process. That's certainly not what I intended.

I want to thank the speakers very much for their participation today, especially the five who responded to our questions. Those were difficult questions that put substantial demands on the speakers, both in terms of preparation and presentation, and we appreciate your efforts very, very much. I think it was very valuable for us, and we hope it was for the program and for those who listened.

And I want to thank Don Runnells for doing an excellent job of chairing. Recall that we will have coffee and donuts available here in this room at 7 o'clock tomorrow morning, and we hope you'll come and interact informally with Board members. The meeting will start promptly at 8 o'clock.

Thank you very much. We're adjourned.

(Whereupon, at 5:50 p.m., the meeting was adjourned.)