UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

2004 SPRING MEETING

Tuesday, May 18, 2004

Embassy Suites Hotel
1250 22nd Street, NW
Washington, DC 20037

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen, Afternoon Session Chair
Dr. Thure Cerling
Dr. Norman Christensen
Dr. David Duquette, Chair, Executive Committee
Dr. Ronald Latanision, Morning Session Chair
Dr. Priscilla P. Nelson
Dr. Richard R. Parizek

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
Linda Coultry, Management Assistant
Alvina Hayes, Office Assistant

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P R O C E E D I N G S

8:00 a.m.

DUQUETTE: Please take your seats. Otherwise, I'm going to play boots and saddles next.

Good morning, and welcome to the Spring meeting of the U.S. Nuclear Waste Technical Review Board. For those of you who don't know me, I'm David Duquette, and I chair the Board's Executive Committee.

Many of you have been at these meetings before, but I'm going to give you a little of the background on the Board, for those of you who are new to the meeting. The Board meets three to four times a year as a full Board. It also meets several times a year in panels for specific topics. Most of our meetings are held in Nevada, because obviously the Yucca Mountain site is there, and about once a year, we try to meet in Washington. Many of you have travelled a long distance to come here, and we appreciate your efforts to be here.

The Board was created in 1987 by the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of activities of the Secretary of Energy related to the disposal of commercial spent nuclear fuel and defense high-level nuclear waste. This includes reviewing DOE's work on the packaging and transportation of the waste. We're required to report our findings and recommendations at least twice a year to the Congress and to the Secretary.

The members of the Board are appointed by the President from a list of nominees submitted by the National Academy of Sciences. It's a multi-disciplinary group with a wide range of expertise and experience, including materials scientists, geologist, hydrologist, biologist, and so on and so forth, most of the sciences and engineering disciplines that are of interest to the site. Normally, the Board consists of eleven members. There are currently three Board vacancies. We're waiting for the White House to make those appointments. And, there are four members of the Board who will be rotating off the Board and we'll be expecting a relatively new Board in about a year or so, or perhaps sooner, depending on what the White House does.

I'd like to introduce the Board members, and I'd like to ask them to put their hands up as I introduce them. In my own case, I'm Professor of Materials Science and Engineering at Rensselaer Polytechnic Institute, and I head the department there. And, my expertise is in physical, mechanical and chemical properties of materials, with a specific emphasis on corrosion properties.

Mark Abkowitz is a Professor of Civil Engineering and Management Technology at Vanderbilt, and he's director of the Vanderbilt Center for Environmental Management Studies. His expertise is in transportation, risk management, and risk assessment. Mark chairs the Board's panel on waste management systems.

Dan Bullen, until recently, was Associate Professor of Mechanical Engineering at Iowa State University. He's recently joined the firm of Exponent with offices in Chicago. His areas include nuclear engineering, performance assessment, modeling, and materials science. He chairs the Board's panel on repository system performance and integration.

Thure Cerling is a Distinguished Professor of Geology and Geophysics and also a Distinguished Professor of Biology at the University of Utah in Salt Lake City. He is a geochemist with particular expertise in apply geochemistry to a wide range of geological, climatological, and anthropological studies.

Norm Christensen is a Professor of Ecology and former Dean of the Nicholas School of the Environment at Duke. His areas of expertise include biology, ecology, and ecosystems management.

Ron Latanision is Professor Emeritus of Materials Science and Engineering at MIT. He's also Professor Emeritus of Nuclear Engineering at that school, and he's the former Director of the Ulig Corrosion Laboratory at MIT. He is currently a Principal Engineer and the Mechanics and Materials Practice Director with Exponent in Boston. His areas of expertise include materials processing and corrosion of metals, and other materials in different aqueous environments. Ron chairs the Board's panel on engineered systems.

Priscilla nelson is a Senior Advisor to the Directorate for Engineering at the National Science Foundation. Her areas of expertise include rock engineering and underground construction.

Richard Parizek is Professor of Geology and Geoenvironmental Engineering at Penn State. He's also President of Richard Parizek and Associates, Consulting Hydrogeologist and Environment Geologists. His areas of expertise include hydrogeology and environmental geology. He chairs the Board's panel on natural systems.

Over to my right is our staff, which is directed by Bill Barnard. One of the really nice things about sitting in this particular position, is the tremendous amount of support we get from the staff. I don't think I've ever worked with a better group of people in my life.

Let me turn to the meeting agenda. I'll be as brief as possible, because we have a really busy agenda this morning. First, this morning, we're going to hear from Dr. Margaret Chu, Director of the Office of Civilian Radioactive Waste Management. She's going to update us on the status of the Yucca Mountain Program.

Following her presentation, Gary Lanthrum, OCRWM's Director of the Office of National Transportation, Office of Strategy and Program Development will present an update of the transportation-planning activities. Since the Board's January transportation panel meeting in Las Vegas, the Department of Energy has announced a decision on the selection of the Caliente corridor. We look forward to additional information related to the planning and development of the transportation system.

John Arthur, Director of the Office of Repository Development for the project, will present an overview of project activities, including long-range plans and project priorities for science and engineering. With eight months to go before the DOE planned submittal of a license application, the Board is particularly interested in hearing this overview.

Mark Peters, Manager of Science and Technology Project, Bechtel SAIC Company, whom we haven't heard for some time, will provide an update of science and technology activities. As always, we look forward to hearing from Mark.

John Ake, Geophysicist with the Bureau of

Reclamation, will provide an update on seismic design. Some of you may recall that the joint Site Characterization and Repository panel meeting on seismic issues held in February 2003, that the Department of Energy establish ground motions estimates for pre- and postclosure. We look forward to hearing more on these seismic issues.

After a brief break, we'll move to the main focus of today's meeting. In May of last year, the Department of Energy provided a series of in-depth presentations describing the thermal aspects of the current repository design and operating mode. Now those aspects have been analyzed, and the results of those analyses will be discussed at this meeting. The Department will also provide additional information on related topics at the September Board meeting last year. The Board used information from these meetings as a basis for a Board letter and a technical basis report sent to Dr. Chu last year. That letter is posted on our website for those of you who haven't seen it. The focus of the letter and report was the potential for localized corrosion of waste packages during the period of high temperature in the repository tunnels after closure. This high-temperature period is called the thermal pulse.

The session on waste package corrosion during the thermal pulse immediately follows the break, and will be chaired by Mark Abkowitz. The goal of this and subsequent corrosion related sessions are to provide the Board with the opportunity to review recent new data and analyses related to this subject. We look forward to an open and comprehensive exchange of views among the meeting participants over the next two days.

To save time, I will only outline the session topics and presenters in very general terms. The session chairs will cover the session topics in more detail and fully introduce the presenters. Let me begin by saying it's unusual, but not unknown, but Board members to make presentations during our own meetings, and we intend to do so here. Three Board members will make presentations. The purpose of these presentations is to summarize the Board's views, particularly for some of you who have not seen a letter or haven't looked at it for some time. It will be a very brief summary of what is basically in the letter to Dr. Chu and our subsequent backup document.

Ron Latanision will open the first session with an introduction and overview, followed by a presentation by Thure Cerling on the evolution of the environments in the repository tunnels to which the waste packages will be exposed. I will conclude our series with a presentation on corrosion. A question and discussion period will allow meeting participants to ask additional questions or comments on the Board presentations, the letter and the report.

After lunch, staff from the NRC and the Center for Nuclear Waste Research and Analysis will present their views and recent research on the potential for corrosion during the thermal pulse. Subsequently, over the course of the afternoon, the State of Nevada, followed by the Electric Power Research Institute will make presentations on the same topics. At the end of each group of presentations, time will be made for questions and discussions. I will warn you now, however, it's such a busy meeting that that discussion period will probably not be long after each presentation.

On Wednesday, the DOE will present relevant views, data, research and analysis. Priscilla Nelson will chair this session and introduce the presenters and presentation topics. Dr. Chu will make the first presentation of the day, followed by the DOE project staff. Priscilla Nelson will also chair the afternoon session. DOE presentations will continue through the afternoon until approximately 4 o'clock. A short wrap-up session will provide meeting participants with the opportunity to make brief final comments. This will be followed by a final public comment period.

As I've just indicated, we have a lot to cover in two days, so to make sure we hear from everybody, it's important that meeting participants pay particular attention to the ground rules, by including staying on time with their particular schedules.

Before we begin, we need to take care of several business items. First, the Board values public participation, and, so, we have set aside time for public comment at the end of the sessions today and tomorrow. If you would like to speak during those times, please add your name to the sign-up sheets at the registration table where Linda Coultry and Alvina Hayes are seated at the table located at the back of the room. Linda and Alvina, please identify yourselves for those of you who need to register for public discussion.

Most of you that have attended our meetings know that we try to accommodate everyone during the public comment period, but with this tight an agenda, there may be people who won't get a chance to speak. We always welcome written commentary. If you have any question that you'd like to have the Board ask related to topics being discussed, please give them to Linda or Alvina. Session chairs will, if time permits, address your questions, however, it may not be possible to answer all of the questions that are asked, or even ask all of the questions that are submitted.

As always, I must offer our usual disclaimer for the record, so that everybody is clear about the conduct of our meeting and what you're hearing, and the significance of what you're hearing.

Our meetings are spontaneous. That's by design.

Those of you who have attended our meetings before know that the Board members speak quite frankly and openly about their interests and opinions. I have to emphasize that when we speak extemporaneously, members are speaking on behalf of themselves, and not on behalf of the Board. When we have a Board position, we'll let you know, and it will generally be published. Also, when Board positions are stated in our letters and reports, they are made available, as I indicated, on the website.

Finally, I'll ask all of you to take the next 15 seconds to confirm that your cell phones and pagers are switched to silent mode. And, I want to emphasize that because it is, as you all know, very disruptive to have them go off in the middle of the meeting. I have to check my own when I sit down.

I was also asked to remind you that the microphones in this room are very limited in range, and, so, be sure to speak directly into the microphone. And, if I haven't done that this morning, I apologize.

Let's start the meeting by introducing Dr. Margaret Chu, Director of the Office of Civilian Radioactive Waste Management. She will update us on the status of the Program.

Margaret, if you would, please?

CHU: Good morning. Thank you for everyone attending this meeting. It's really a full house here. I'm looking forward to the presentation and discussions over the next two days. 20 years, how about that. Yeah, two days.

As might be expected during this important year of license application preparation for us, our office has made progress in many areas since our last Board meeting. And, I would like to begin by discussing key management topics, as usual, before turning to the more technical items on the agenda.

First, please let me introduce John Wengle. I don't see John. Okay, John Wengle over there, our new Director of Science and Technology and International Office at Headquarters. John was previously with the Office of Science and Technology under the Office of Environmental Management at DOEM. He just came over not long ago, and they were pleased, by filling that position.

Now, staff realignments have taken place at the Office of Repository Development to support improved integration and project management at the Office of Repository Development, which John Arthur will tell you about in a little bit. Additionally, the firm of Hunton and Williams, based in Richmond, Virginia, is now under contract to provide legal services throughout our licensing process.

For many years, the Department has maintained a goal of beginning to receive waste at a licensed Yucca Mountain repository in 2010. Many activities will have to be completed over the next six years for this goal to be achieved, and sufficient funding will have to be provided and sustained to support repository licensing and construction and transportation system development. As you all know, our focus this year is to prepare a high quality license application.

We are looking forward toward a very busy summer to complete the remaining work, but we are committed to devoting the time and effort necessary to meet NRC's requirements and our own high expectations. One recent example of this commitment is the recent reassignment of a fair sized group of staff members to a concentrated review of our technical products for clarity, transparency and sufficiency. We initiated this review with respect to observations that were made by the NRC during its technical evaluation of analysis model reports, AMRs, and then also the review of certain processes and the corrective action program.

At the last Board meeting, I provided details on our implementation of wide-ranging management improvements. Our approach to many of these improvements was defined in the Management Improvement Initiatives you have heard before, which we undertook in 2002. In April of this year, I informed the NRC that we had completed the commitments made in that particular initiative, and had transitioned the continuous improvement goals to day-to-day line management practices. This followed a comprehensive review, conducted by an independent firm, which verified that responsible managers had demonstrated evidence of completion for each of the actions, and we had appropriately made the transition of responsibility to line management. That was really our goal. So, it became a day-to-day improvement.

Through these improved management practices, clearer roles and responsibilities, and a Program-wide focus on principles, such as quality, accountability, and safety-conscious work environment, we have resolved longstanding problems and advanced the program. For example, at the last Board meeting, I told you about our first externally administered safety conscious work environment survey was ongoing. Now, I can report that the survey firm rated our office work environment as substantially better than similar government science and technology organizations, and that we're continuing to do survey on a periodic basis. We have also closed two longstanding, very longstanding, Condition Reports, these are terms in the Quality Assurance Program, on two things. One is data, another software. And, that we are on a path to close the model validation Condition Report, that's another, the last remaining longstanding Condition Report, and we are scheduled to close that sometime in the summer.

We have seen measurable improvements in the implementation of quality assurance requirements, process adequacy, self-identification of conditions adverse to quality, and in the planning, implementation, and verification of corrective action. Overall, I really believe trends are going in the right direction, and I believe we have the ability to resolve our remaining issues and prepare a license application with the clarity, completeness, and traceability required for it to be docketed by the NRC.

The final management topic I would like to cover is program funding. The President's budget for Fiscal Year '05 included $880 million for our office. The main factor driving this request level is the convergence and integration of repository readiness, transportation system development, and waste acceptance readiness. Significant work must be done in all three areas starting in '05, if we are to sustain our longstanding goal of beginning repository operations in 2010.

$880 million is a significant increase over the past funding levels, but it is one that has been planned carefully and understood for many years, and this is only the first of several years of higher funding requirements down the road. We have reached a point where appropriations at historical levels will no longer work. As part of OMB's budget request, this year, we have submitted a legislative proposal that will allow it up to the amount of Nuclear Waste

Fund annual revenue received from utility contract holders to be reclassified from mandatory receipts to discretionary collections, so that they would directly offset appropriations from the Nuclear Waste Fund.

The important point is the amount credited as offsetting collections would still be subject to Congressional appropriations, there's a lot of confusions out there, but, it's still subject to Congressional appropriations, but could be appropriated within the amounts of receipts without reducing the funding that would be available for other federal programs. One of the voids is the competition with other programs for funds. That's really the key. Many Congressional leaders recognize the importance of the repository program and the fundamental principle of using taxpayers' disposal fees for their intended purpose. We don't know what the outcome of the legislative proposal is. At this time, we are proceeding under the assumption that adequate funding will be provided for licensing, planned transportation work, and other activities supporting the 2010 goal.

Now, turning to the agenda of today and tomorrow, I'd like to touch on some of the topics that other speakers will address in depth later.

Right after my remarks, Gary Lanthrum, our Director of National Transportation Program, will provide a transportation update. I am very pleased with the progress we have made in Fiscal Year '04. After several years in which transportation work was deferred over and over again due to funding limitations, Gary has reactivated the program, has made significant accomplishments in a short time. Since the last Board meeting, the Department issued the Transportation Strategic Plan, issued a Nevada rail corridor preference announcement and Record of Decision, issued a Record of Decision identifying mostly rail as our chosen transportation mode, and initiated the EIS process with a Notice of Intent and scoping hearings. The scoping public hearings we just completed yesterday. There were five of them total. Gary will also tell you about an ongoing assessment of existing transportation casks that support the cask acquisition process.

John Arthur, Deputy Director of our Office of Repository Development will discuss our license application progress in detail later this morning. Mark Peters, from Los Alamos National Laboratory, will, as he has done in the past, provide an update on the Yucca Mountain Project's ongoing science and testing program in support of the license activities. I do want to emphasize that we do have quite a bit of ongoing and planned scientific programs.

Also, the Board has had considerable interest in our work in the seismic area, especially in the low probability and the ground motion. John Ake later will give you an update on our latest work in this area, in the low probability, and how we're treating it right now.

Now, most of the time allocated to our Department at this Board meeting will be devoted to the topic of potential waste package corrosion during the thermal period. I have read and understood the Board's letters and its report on this topic, and I hope that tomorrow's presentations from our office will show that we are giving very serious consideration to what the Board has to say. Our senior management and key members of our technical staff are here to listen to the Board's views, as well as views and research by the NRC, the State of Nevada, and the Electric Power Research Institute.

After receiving the Board's technical report on waste package corrosion in November 2003, I provided the Department's preliminary views in a letter dated December 17. We as a Program have spent significant time in analysis of your letter and report. I would like to start by acknowledging the effort and time the Board has made in analyzing and explaining in detail the issues and concerns you have associated with waste package corrosion, especially during the thermal period. This report really helped us to better understand how our logic, data, and presentations could be enhanced to address your concerns. I personally have worked with our staff to determine how to address these concerns, and have been directly involved in focusing new work to get to the heart of resolving our differences. We have done additional tests, additional analysis, many that are directly focused to answer Board questions.

In our presentations and briefings tomorrow, you will see additional data and further evidence that we believe that substantiates our previous position that corrosion will not only not be widespread, but also very unlikely. Senior scientists from BSC and Lawrence Berkeley Lab will provide detailed technical presentations on our analysis of likely repository conditions. That's tomorrow. And, my advisor on corrosion science, Dr. Joe Payer, who is a well-recognized expert in corrosion from Case-Western Reserve University, will discuss the corrosion behavior of the waste package material, Alloy 22, again, tomorrow.

I want to emphasize that although our positions may differ, I believe this open scientific interchange is extremely valuable to us, and we are here to listen and share and to discuss. I thank the Board for devoting its meeting to such extensive consideration of this important topic. In addition to exploring the individual processes that would occur in a repository, we must also consider the probability, consequences, and uncertainties associated with these processes, and integrate the analyses of individual processes into a total system view. This is what NRC's risk-based regulatory framework requires, and that's what we are, the whole Program, is working toward. And, it is what DOE must provide to NRC to demonstrate a reasonable expectation that the repository will operate safely. This is a very important point that I want to emphasize, so tomorrow, I will make a short, ten minutes, presentation on this specific topic tomorrow morning.

Thank you. And, I'll be happy to answer any questions.

DUQUETTE: Thank you, Margaret.

Unless there's a burning question from the Board, we're already a few minutes late, this is sort of like an Abkowitz meeting, so I'm going to thank you, Margaret. I think we're going to move on with the program.

With no disrespect meant for the speakers, we normally introduce them and give a short biography. There's so much to do this morning, I think we'll only introduce them, and have them come up, and I, again, with no meaning for disrespect, I'll announce them from here so we don't waste even those few seconds.

The next speaker is Gary Lanthrum, Director of National Transportation, Office of Strategy and Program Development for OCRWM, and he's going to give us a transportation update.

LANTHRUM: In the interest of maintaining the schedule, I will forego the humor this morning, and jump right into the presentation.

Since the last time we met, one of the things I started off with was a discussion of major milestones that we were going to be pursuing. At the last time I gave an update to the Board, we already had a number of these done. The first three of these had already been issued, the creating a transportation management approach that was focused on projects rather than just on ongoing work, developing a transportation scope based on the available budget, and issuing the Transportation Strategic Plan.

What we've done since then is we've begun working with state regional groups on specific targeted projects. In the past, our relationship with state regional groups, for those of you that may not be aware, to facilitate more appropriate transportation planning in dealing with the states. We have individual state relationships, and we certainly will maintain notifications on a state by state basis for any shipments that are done, but, to do really good planning, you have to do it in a regional context. So, where a route enters and leaves a state, connects with entry and exit points in adjacent states. And, so, we have state regional groups that combine a regional focus and help us to do integrated planning a little bit better.

In the past, we had just blanket funding that was provided to these state regional groups to provide a cross-cutting look at our programs, and advice. What we would challenge them to do this year is to come up with specific projects that they are interested in that would facilitate their ability to address concerns they've got, and at the same time, help move the transportation planning process forward.

Our fiscal year for the state regional groups runs a little bit different than the federal fiscal year. The contracts for them run from July through June. We are working closely with the state regional groups, and expect to have some of these specific projects that they have asked to focus on in place before the July update to their cooperative agreements. We met just recently at the Transportation External Coordinators working group in Albuquerque, and the representatives from the state regional groups, as well as from industry and several tribal representatives were there, and we talked about this focused project approach, and it received considerable kudos from the assembled audience, and from the state regional groups, because it helps them more directly address the things that they are concerned about, rather than staying more general in their approach.

A fine example is there's a significant difference between state regional groups on their thoughts on barging operations to get from sites that don't have rail access to a rail head. States in the midwest are adamantly opposed to barging on the Great Lakes, however, states in the southeast that have plants along river sites that may not have rail access are very interested in barging. And, so, the southern states, and now the northeast states, have expressed a significant interest in doing a barge study on the viability of that as a way of getting rail sized casks from shipping sites that don't have rail access to a rail head. And, so we are able to accommodate the needs of the northeast and the southern states without impacting adversely the midwest states that are opposed to it. We've got a number of other projects, and I can talk about those in more detail a little bit later.

We've also begun building up the transportation infrastructure that's going to be necessary. I'll talk a little bit more later about the actual cask development effort that we've got underway. We received a number of questions and some concerns have been raised by the Board about the time it will take to get casks in place to move the contents that we've got. I think when we get into the detailed slide about our cask project, you will have a better appreciation for what we've done in working both with the industry and with our customers to make sure that we will have the assets necessary when shipments start in 2010.

We did announce our record of decision, as Margaret indicated, on both our mode of transportation, which is now mostly rail, and our corridor selection for where to build the rail line within Nevada. And, in parallel with that, we issued a Notice of Intent on development of an EIS for alignment of the rail line within the Caliente corridor, which was selected.

Where are we going from here? We're going to be busy. We've got a lot of questions from the Board about the basic project planning and desire to see Gantt Charts, for example, that define both the actual tasks that will be necessary to be successful in our transportation planning, the resources required to support those tasks, and the schedules for executing them.

We have to be careful about not putting the cart before the horse. What we're working on right now, what we've done in a lot of detail, is we've developed a list of significant milestones that have to be achieved. A prime example is on the Nevada Rail Alignment. We know that we've selected rail, mostly rail, as our mode of transportation. We are just now, as Margaret indicated, completed our scoping meetings. The scoping period extends through June 1. So, in addition to the scoping meetings, we are still taking written comments, and for a number of stakeholders, were able to come to the scoping meetings. In some cases, a scoping meeting is a way for individuals and organizations to kind of gel their ideas about the transportation system a little bit more. They can see some of the displays, some of the alternatives, the layout. Many of them give comments at the scoping meetings. Other individuals will go home and think about it, and then submit comments later on.

Out of all this, at the end of the scoping process, those scoping comments will go into helping define the scope of the EIS itself. That's going to determine the duration of the EIS. Right now, we don't have the scope marked down in stone, and it won't be until the EIS process itself is completed, and we've issued a Record of Decision on the rail alignment, and on the other issues that are raised as part of the scoping process, that we will be able to develop a performance specification and a detailed baseline for the actual construction of the railroad. And, so, we've got milestones. We know where we want to be at given points in time along the way.

What I can't do is say here is the exact schedule for building a railroad, because I don't know the scope of it yet, and I won't know the scope of it until we complete the EIS. The EIS is going to say where exactly within the corridor that we've selected the rail is going to be constructed. And, so, there are a lot of unknowns now, and it's important that we've identified the milestones that we're working towards. But, as we get more detailed definition of a scope itself, and a more detailed definition of the resources required to execute that scope, the schedules associated with executing that scope are going to change, and that will generate the kind of Gantt Charts that were requested by the Board.

We're also working on project execution approval for our acquisitions. Within the Department of Energy, there is an order that defines how we manage projects, and that's what they call a CD process. It's a Critical Decision Process. The first Critical Decision along the way is basically the approval of the project itself, and you enter that with a ball park duration and scope definition that bounds what you think the project is going to be. Once you get approval based on that broad definition of the size of the box the project is going to fit in, you go off and do a lot of detailed analysis and you come back at a later point for what's called CD2, Critical Decision 2, which is actually the authorization to do the final design, and then CD3 is the authorization to build whatever the project is.

What we're going forward with is the CD1 permissions to allow us to develop the more detailed analysis that would be presented in the CD2 context. And, we've got a fairly good set of background information, and details on talking to the energy system's Acquisition Advisory Board, who actually gives us the approval to proceed with the project.

The big ones that we're working on right now are the Cask Acquisitions, the Support Facility decisions, and some decisions on moving forward with Nevada Rail, just again on that overarching size of the box, what is the general size of the project, and what's the general duration of the project for execution.

We have begun development of the EIS process, as I've already discussed. We've been through the scoping meetings. We've had about 400 people attend the meetings. We had three meetings along the Caliente corridor within each of the counties that the rail line passes through. The first was in Amargosa Valley, the Nye County, the terminus county. The second meeting was in Goldfield in Esmeralda County, and the third meeting was in Caliente and Lincoln County, the starting point for the Nevada Rail Line.

We were requested by the State of Nevada to add two additional meetings, which we did, and we extended the scoping period also in deference to the Nevada request. The additional meetings were added in Reno and in Las Vegas. The Reno meeting was held last week. Surprisingly, there were fewer people at the Reno meeting than there were at the more remote meeting locations along the Caliente corridor itself. We only had about 45 people show up for the Reno meeting.

Last night, we held a meeting in Las Vegas, and as you can well imagine, it was well attended. We had about 125 people attend the Las Vegas meeting, and we got lots of good comments, lots of good discussion I guess is probably a better characterization.

Not everybody, as you can well imagine, is in favor of us moving forward with this project, but we got lots of good comments. And, interestingly enough, even the folks that were opposed to the project itself, were favorable of the format where we held the scoping meetings. It was not a construct where there was a podium and presentations given. There were people allowed to wander through an area where they were able to collect technical information about the scope of the project, and the basic approach and the process for getting the EIS in place. Then, there were four folks that wanted to give written testimony. There was a number of court recorders available there to give their written testimony--or, their verbal testimony to, and if you wanted written testimony to turn in, there was a basket for that. Then, there was just a lot of people there available to do question and answers with.

So, it was a successful format, and I think all the people that participated appreciated the fact that it was a format that supported open and frank discussion. And, so, I'm hoping that out of all of this, we'll have some good comments that will shape the conduct and the scope of the EIS itself.

And, I've already talked a little bit about increasing the focus of the institutional collaboration on specific transportation projects that they themselves want to pursue, and that's moving forward nicely.

The four main projects that we have, and I've talked about this with the Board before, we have four projects. The first is the Fleet Acquisition Project. It's buying the rail casks, buying the rail cars. There will be some truck casks that we will need, because even under the mostly rail scenario, there will be some truck shipments, some possibly from sites that don't have rail access, and choose not to use either heavy haul or barge shipping to get from the site to a rail head. And, in that case, they would have the option of using legal weight trucks for the shipment all the way.

We issued a supplement analysis back in the early April time frame that addressed the possibility of putting legal weight truck casks on rail cars, and transporting them to an intermodal facility located somewhere, and then doing a legal weight truck shipment from that intermodal facility to the repository. And, that is an option that's available, and it was actually analyzed in fair detail in the original repository EIS. The supplement analysis just validated the fact that that had been one of the activities that had been studied, and that the impacts of that possibility had been taken into consideration in the original EIS, and, so, just letting folks know that that was something that was being looked at as a possibility, if in fact rail was not completed by the time the repository opened.

There's an Operational Infrastructure Project. We've got a lot of interesting work going on here. The Operational Project, a lot of folks see a dichotomy between the term operation and project. There's usually a split. Operations are operations, and projects are projects. Well, since we don't have an operational system in place, the operational project is the effort to build the infrastructure necessary so we can get to the point where it transitions to operations, per se.

And, some of the things that are involved in this are security planning, developing the concept of operations. A number of the studies that we are doing are being supported through the operational project. We are supporting the NRC's package performance study, and I think some of you might have seen that this week, the Nuclear Regulatory Commission came out with their selection of their test plan. They are now developing the test schedule and resource requirements for their effort, and we are supporting that. I've had some discussions with Dr. Papereillo and others in the NRC's research and development arena. What they're looking at is whether or not they would possibly be able to accelerate their testing program if we were able to provide support to them this year. They're taking a serious look at that currently. I've got my fingers crossed and hopefully by providing support early and maintaining that support, there may be a chance of accelerating their schedule, which right now calls for completion in the 2009 time frame.

On the security front, there's been a lot of interest in that arena. I can't go into a lot of details, but I can tell you that we've had meetings with the Department of Homeland Security, with the Nuclear Regulatory Commission, and with the Department of Transportation. As you are probably aware, the Department of Homeland Security has required development by federal agencies of critical infrastructure protection plans, and they have a critical infrastructure protection plan for each sector of the economy. And, the nuclear sector of the economy, the NRC, is responsible for the plan. That plan includes nuclear plants, nuclear materials, nuclear waste, and all aspects of dealing with those contents and those sites.

In the earlier draft of that plan, Transportation was not included. In our last meeting several weeks ago with DHS, the NRC and DOT, a decision was made to include Transportation in NRC's plan, particularly for category 7 hazardous materials, which is, you know, the radioactive materials.

DOT also has a critical infrastructure protection plan that they are working on, and their sector of the economy that they're focused on is transportation, and they will be addressing all other hazardous cargos. And, in fact, Rick Boyle from the Research and Special Projects Administration within DOT is helping craft the language of the Transportation piece of the NRC's plan to make sure that there's no split between the approach in the NRC plan and the DOT plan.

In addition, we've worked with our own Office of Safety and Security. What we're developing now is a transportation specific design basis threat. We've got a lot of time to work on that, and I would fully expect the design basis threat would change possibly significantly between now and the time that we actually start our transportation operations.

The important thing to note, though, is that we are working with the security world in looking at both the national impacts of our small piece of work in the overall context of transportation in this country. We are a very small drop in the bucket overall compared to the number of hazardous goods that are moved around this country every day, and yet there's going to be a lot of focus on our shipments.

What we have to make sure of is that the security approach that we take is consistent with the security approach that is being advised by the Department of Transportation and the NRC, and it melds well with the Department of Homeland Security's expectations.

DUQUETTE: If you'll please take your seats. And, Gary, let me turn it back over to you.

LANTHRUM: Okay, thank you.

I was just talking about the institutional project when somebody decided that was not a subject that I needed to dwell on, so I think I'll jump on next the Nevada Transportation Project, which is one that everybody should be pretty familiar with. That's what's been getting most of the attention here lately, and is driven by the fact that we've made the decision to use mostly rail as our transportation mode, and required the selection of a corridor within Nevada to build a railroad. And, now, we are deeply enmassed in the scoping process for the EIS that will define exactly where within that corridor the rail line would be constructed, and all the other details associated with that, the design, the construction, the operation, and possible eventual abandonment of that rail line, since the transportation requirements for actually bringing waste in would be concluded after 24 years.

I've got a list of some milestones here, but we've already gone over these, the fact that we've made the decisions that support where we are in the EIS currently. This is a little bit more important chart to see, perhaps. It shows the basic organization of the transportation program office. Transportation here, there are, again, the four main projects that we've got, the institutional project, operations, the fleet acquisition, and the Nevada Rail project.

What informs how these projects get executed is a couple of things. One, is on the waste acceptance side, and I know there have been a number of questions raised about waste acceptance itself, and the interactions with the utility community, the Department is in the process of trying to define or update information about what utilities would be desirous to ship when. We're expecting some updates here in the not too distant future. But, there's obviously a very clear driver from my perspective over what's going to be shipped when. With the fact that even when we get updates on what's going to be shipped when, the utilities have the opportunity to change what they're going to be shipping as early as six months prior to the shipment itself.

And, so, even when we get updated information about long-range plans, when the actual execution comes around, there's still a fair amount of uncertainty. So, what we're doing to bound that uncertainty on the Fleet Acquisition, we are looking at procuring casks and rolling stock capability to bound the majority of what shipments could be requested initially.

So, we are going to be relatively impervious to the decisions that are made overall on the left-hand side here in our ability to support some shipments initially, regardless of what those shipments are. So, again, our goal is to build a very broad based capability with the casks that we procure, with the rolling stock that we procure, and ultimately, with the contracts for operations that we procure, and, thereby, somewhat mitigating the impacts of last minute decisions that can be made from this point.

We are also impacted by the repository, and what they're capable of receiving, what they're geared up to receive, and the mix of receipts that they would like to see. And, again, the same basic approach of a broad based capability down here will serve whatever decisions are made and whatever changes are made, again, both on the repository side and on the waste acceptance side.

The final external driver, and it's one that we had a two-way relationship with more than a one-way, is with our stakeholder communities. And, again, we've got this interactive process going on with the state regional groups, but we have other stakeholders. We have the industrial stakeholders that are actually going to be providing some of the requirements, the casks, the rolling stock. Ultimately, there will be operations contractors that we'll be dealing with, and there will be a fair amount of two-way negotiation with them. We have the states, we have the tribes. There's a whole slew of interested parties that we will be working with, both as we go through the development of the infrastructure itself, and as we do our concept, development of a concept of operations. There's a lot of give and take there, and all of that work winds up informing the actual execution of the projects that we wind up putting in place.

Here is a very high level look at the significant milestones for each of our four projects. They're broken down, National Transportation Project, a Nevada Transportation Project, which is the way that at least OMB sees our funding requests. They see three major projects for the Offices of Radioactive Waste Management. There's a Repository Project that John Arthur is responsible for. Then, there are two Transportation Projects, the National and the Nevada.

Under the National, we've got our Fleet Acquisition, our operations, a Fleet Management Facility, which is actually more broad than that. There's a whole slew of support facilities that will be required to support the transportation infrastructure. And, there's the Institutional Project. And, again, ultimately, the

Institutional efforts will become operational in nature, but we're still building the basic infrastructure and the relationships that will allow us to get to that, the operational mode.

On the Nevada Transportation side, we've got the actual Mode ROD, and that's not a schedule, that is a milestone that was achieved. What we're doing now is working on development of the alignment EIS. We've got a few milestones for that here. And, then, ultimately, that will lead to rail design and construction. We're anticipating that the rail design and construction process is about a four year evolution, but we won't know for sure until we complete the EIS and issue a ROD and know exactly what the alignment of the rail line is and what the input has been provided on how that rail line would be operated and conducted.

And, again, that's more than a little bit of an eye strain here to try and see what's up on the chart. What I wanted to emphasize is the fact that we've done a lot of detailed task discussion supporting milestones. And, to cover a bit of that, I'm going to go into one particular task, and I've provided a number of these in your handouts, and in the presentation materials, and it would probably ultimately be more beneficial for you to spend time looking at this electronically where you can blow it up and see the details. But, I wanted to give you a feel for the level of effort that's gone into each of our projects, and the cask acquisition is a good example.

What we started off doing was back in January of this year, we issued a Notice of Intent, Notice of Programmatic Interest, to the industry as a whole through Fed Bus Ops, and said we're interested in acquiring casks, and if you as a vendor have ever had a type B certificate, which is a kind of certificate that our casks will have, from the NRC, and if you are interested in possibly providing casks to us for our work, come talk to us. We had seven vendors express an interest to come in. We held meetings later that month. They were very good discussions. And, in fact, the discussions we had with the cask vendors gave me a much better feeling about the work we had ahead of us than I had anticipated before they came in.

I had anticipated that our capability to bound our work scope with existing casks was probably somewhere down around the 20 to 30 per cent coverage of the materials that we needed to ship in 2010. The cask vendors assured us that the number was closer to 70 per cent of what we needed to ship could be covered by existing hardware, either through existing certificates, or with existing hardware where the certificates would be modified to add additional content.

And, so, what we're looking at now is three basic paths forward. There are casks existing hardware, where there is an existing certificate, and that would allow us to ship some--changes with the NRC. We could actually load those casks up, and depending on whether it's a rail cask or a truck cask, put it on the appropriate conveyance and move it to the repository.

There are some casks where the hardware is sufficient, but the certificate does not adequately bound our needs, and it's a, relatively speaking, a relatively simply approach for the vendor to add additional contents to their certificate, make an application to the NRC. The NRC at that point is not reviewing the whole design. They're only reviewing the application of that design to a specific content. And, so, the turn around time for that kind of an application is far quicker than the application of a new design completely from scratch.

The third option is that there would be a need in some cases for completely new designs, and clearly, the timeline for completing a design, submitting it to the NRC, to have the question and answer process resolved to the point where the NRC could issue a certificate of compliance, that clearly is the longest line process for any of the options out there.

Now, the meetings we had with the cask vendors were one on one meetings where they discussed fairly openly with us what they thought they could do, and we anticipated that those discussions would in many cases be seen as more of a sales pitch than anything. So, the next step, rather than take everything on face value, is we worked out with some procurements to try and buy cask capability reports, and what those are is essentially getting the vendors to put in writing what they had communicated to us verbally. And, what we're doing is we're asking the vendors to take a look at all the materials that will be available to be shipped in 2010, and map what they currently have to those contents, map what they currently have and think could be made more broadly acceptable by changing just the certificates to those contents, and show what contents we will have in 2010 that would require completely new designs on their part to be able to support.

The procurements for those reports are expected to go out here in the very near future. We did get all the applications in. We have edited them. We'll be making the awards here in the not too distant future, and we're expecting the actual reports themselves to come back this summer. That will help give us a very clear framing of what's going to be needed to make sure that we have that broad based capability I talked about in 2010 to accommodate any last minute changes in shipping plans that are made by the vendors, exercise the options that they have under the Nuclear Waste Policy Act.

We're expecting in the 2005 timeframe, based on the information that we get from these cask capability reports, to again look at possibly expanding the capability of some existing casks by authorizing some vendors, actually procuring design services to expand the capability of their existing casks with revised certificates.

And, possibly some additional design work would be authorized if we have very long lead tasks that we would anticipate would be needed initially, and the implications we got from our meetings in January with the vendor community was that we would be able to provide that broad-based capability without any new from scratch designs. But, if after the final written reports come in, we feel that we should have some new designs in hand to start shipments in 2010, we would also start that process in the 2005 timeframe, and start initiating cask fabrication in 2006. Again, it's a phased approach. It let's us look at what the options are currently. It let's us make advances without major commitments of funds as we look at certificate modifications. Again, all the time expanding our knowledge of what the utilities desire to ship before we commit ourselves to actual fabrication of casks.

And, the fabrication, again, we're looking at in the 2006 time frame. We would expect deliveries of Category A casks, and the Category A is the existing designs with existing certificates, possibly as early as the late 2006, early 2007 timeframe. Those early deliveries would be to do training exercises with some of our stakeholders. We also have a fairly significant scope of work in developing rolling stock that meets the Association of American Railroad Standards for moving spent nuclear fuel and high-level waste. There is a very detailed dynamic testing program required for cars certified to meet that standard, and it would be very helpful to have a couple of casks on hand that could be loaded with dummy product for that testing. But, we would like, rather than having just a completely dummy load, to actually use an actual cask, even though the weight in the cask may not be actually spent fuel, but actually have a cask loaded on the cars for the dynamic testing. And, so, those procurements would support that.

We'd expect delivery of the Category B casks, which are the ones where we've had additional mods done to the certificates, in the 2008 timeframe. Deliveries of the Category C casks, if we need any, in the 2009 timeframe, and begin operations in 2010. Again, this is the kind of thing, and a milestone level, that would be revised. Again, we'll have these cask capability reports this summer sometime. That will give us a very clear view of whether or not what we've been lead to believe from the verbal presentations is accurate. We'll be able to make course corrections, and adjust accordingly. But, we've got a lot of work that's gone into developing our capability for looking at casks.

A similar scope of work has gone into the following slides. I'm not going to go into these in any detail, just provided that for your information. But, we've got a fairly significant look at the milestones for the institutional program. You can see there's a lot more milestones here. There's a lot more work going on in parallel on the institutional front than there is in the cask front. It's a much broader scope of work, a much broader set of stakeholders we have to deal with.

We have the Operations Overview. This captures some of our security planning activities. It captures our operational planning activities, where we are in developing a concept of operations. We've got rolling stock acquisition activities. Again, this is more on the level of number of milestones of the casks, because it's a very focused activity. We're looking at procurement basically of three types of rail cars, an actual load bearing car to put the casks on, a security car to cover our security requirements for these shipments in transit, and a buffer car to go between the load bearing cars and the locomotive, or between the load bearing cars and other cars that may be in the train.

And, then, finally, a support facilities plan.

And, again, more of the details are down in the discussion here. Again, there's a fairly finite number of facilities we're looking at right now. Based on the comments we get through scoping, the number of facilities may change, and there may be activities that we would anticipate being performed in a single facility that based on scoping comments, we get during the EIS. They may be broken into multiple facilities. There are a number of things that can be co-located or split. We're expecting to get lots of input on those kinds of activities. In fact, we encourage our stakeholders to give us that kind of input during the scoping process.

The Nevada Rail Transportation Project is one that we've already talked about in a fair amount of detail. And, again, we know that we want to have rail available as early as possible, but I can't put together an actual performance baseline for constructing a rail line until we complete the EIS, and we've identified where exactly within the corridor the rail is going to be aligned. We know a lot of input about what the operational constraints of the rail line is going to be, how our stakeholders have asked us to consider, or actions they want us to consider in a design process. All of that will inform the performance baseline that will frame the actual requirements for final design and construction of the railroad.

What we've got in terms of upcoming decisions, we have begun the Environmental Impact Statement process. We are working hard to get the Environment Impact Statement contractor on board. We've already issued contracts for some of the technical work that will be done out there. Regardless of the comments that we get from the scoping process from our stakeholders, there are some things that we know we have to do. We have to do the geotechnical work out there. We have to do the hydrological work on the site. We have to do the cultural and environmentally sensitive species of plants and animals. We have to do all of that. And, so, contracts for that technical data collection have already been let in some cases, and will be let soon in others. And, parallel with that, we are trying very diligently right now to get the EIS contractor itself on board to have them help shape the data collection and incorporation of the public scoping comments that we received into the actual scope of work that will ultimately result in our EIS.

I've told you a little bit about where we are in our rolling stock acquisition and our cask acquisitions. Again, we are taking a phased deliberative approach where we're pulling the industry in. We're getting comments from our stakeholders. We're taking all that into account before final decisions are made that would be irreversible, like actually going out for fabrications. We're maintaining a fair amount of flexibility before final commitments are made, and yet we are still looking at the requirements of making sure that all of the tasks that have to be completed to be ready to support shipments in 2010 have been thought of and are included, at least in a milestone schedule right now. And, as we complete milestones that develop enough detailed information to do performance baselines, we will do that.

One of the criteria or actions that all of our state regional groups expressed a significant interest in from the stakeholder perspective was developing routing criteria, and the process for selecting routes. That's one of the things that we will be providing funding for to the state regional groups, and we'll be working with tribes on. Routing is clearly one of the issues that they are interested in, and we will start work on routing criteria and selection methodology in the near future, hopefully having, as pointed out on the institutional timeline, the actual preferred routes established sometime late in 2006 that would support development of our emergency response planning activities, because that has to be focused along where the routes themselves are. And, again, we're integrating the planning between our different projects to make sure that what's done on the institutional side supports our technical development.

Also, last week was a meeting of the state and tribal government working group in Sante Fe. Our office was there. We have said on a number of occasions that our expectation is to work with the tribes on a government to government basis. But, just as we will work with individual states on state expectations, for overall transportation planning, it's necessary to pull a number of states together to do a regional approach.

The EPA has been very successful in working with tribes on a regional approach and yet maintaining the individual government to government relationships that are important to the tribes and to the Department. We anticipate the same kind of approach being implemented by DOE that was discussed at this meeting in Sante Fe last week, again, encouraging the tribes that they would not lose any of their sovereignty in joining together in regional groups to address transportation issues efficiently and effectively, and I'm waiting to get feedback about how that meeting went and how we would move forward in establishing the definition of the regions in which tribes will be participating.

In conclusion, we've got a challenging set of projects, and I think that many of you may see as an under statement, but we have done a significant amount of work in developing the milestones that are necessary to execute those projects. We are working where we can on development of detailed project baselines, doing the resource worrying for the activities that we know that we have to do, and making sure for the scope of work that has been defined, that we have a fairly good appreciation of a schedule required to execute that scope.

We've got a lot of work to do on the Nevada Rail construction, on emergency response training, and on fleet acquisition. I went through that as we discussed the individual activities. I think probably at this point, it's best to go ahead and say that I think we can conclude all of this and be ready to ship by 2010, particularly if the indications we got from the cask vendors and from the rolling stock vendors is accurate in saying that if we had to start shipping tomorrow, we have the capability in place to safely and securely move spent fuel from utility sites tomorrow if we needed to.

And, so, knowing that we have that base capability in place now gives me great confidence that we can expand that capability to be the broad based offering that I intend to have in place for a broader scope in 2010 when the repository starts operations.

With that, I'll open myself to questions.

DUQUETTE: Thank you, Gary.

We're running a little late, and I'm going to ask the Board to keep their questions to a minimum, and perhaps we can optimize that by having Mark Abkowitz make some comments as Chair of the Transportation Panel, and then have some questions after that. Mark?

ABKOWITZ: Thank you, David. Abkowitz, Board.

Gary, first of all, thank you for your information that you presented today. I think that this has been very helpful, and I wanted to commend you on the progress that the Department is making in transportation planning, and in particular your laying out the schedule that you're working within. I recognize in our Board letter that we were asking for this type of schedule to be produced, and it's an incremental process that involves continuing levels of detail. But, I think it's very important that you've been able to lay out in each of your project areas the milestone schedule, because that's certainly the first step, and is much more commensurate with the kind of information that constitutes the strategic plan, at least in my personal opinion. So, I wanted to thank you for that.

There will be a Transportation Panel meeting. It's being planned right now to be held sometime this fall, and at that juncture, we can get into some of this information, and other new developments in greater detail.

There are a couple of things that I did want to raise, and if you would like to comment on them, that's fine. First of all, it's becoming apparent, as you know, that this is a very ambitious activity, and a number of concurrent planning activities that are going on, and their interdependencies, and the timeframe that you're operating under are really going to necessitate a closely coordinated, well-managed overall effort.

So, one of the things that I did want to bring to your attention is that at some point, these project milestones and ultimately schedules will need to be interfaced into one grand schedule, and that there be an identification of the interdependencies between those projects, because there is a critical path that will be emerging from this, and there are certain steps that will not be able to be accomplished very well without other steps having been accomplished previously. I'll give you a couple of examples just to illustrate the point.

One is in the area of cask procurement and fleet acquisition. It's difficult to imagine how well the system can be put together before waste acceptance and access egress infrastructure issues are fully understood and agreed upon between DOE and the utilities. Similarly, in the area of emergency response planning, absent route selection, there's only so far that you can go with emergency response planning.

So, in iterations of this planning process, it will certainly be helpful to get a better understanding of how these projects interface with one another and when certain things can be operated in sequence, and when they have to be operated in succession.

The other sort of over arching comment I wanted to make is in the Nevada Transportation Project area. I notice that there's the absence of the word truck anywhere in the Nevada Transportation Project slides, and I recognize that there's an emphasis right now on trying to establish rail access into the facility, and that, you know, the EIS and other activities around rail design and construction are sort of foremost on your mind. But, I think it's becoming more apparent to more people that the likelihood of having rail access directly into Yucca Mountain by 2010 is certainly far less than one, although somewhat greater than zero.

And, so, consequently, I would encourage that there be more comprehensive and explicit attention focused on truck transportation planning within Nevada. And, some of the issues that come up when one gets into that area are issues about intermodal transfer facilities, upgrades if necessary to road infrastructure, and what particular routes would be used, and even issues in the licensing area, such as are truck casks licensed for rail use, if in fact that's what's going to happen. So, I would just encourage that truck be a card carrying member of the modal planning that goes on in the Nevada transportation project.

Thank you.

LANTHRUM: Can I give you a little bit more feedback on that? We are aware that there is a need for good integrated planning, and, in fact, the work that I've done so far in developing the milestones, we do have tasks below the milestones, it's just that they aren't tasks that have been completely vetted by the information that they're going to ultimately need.

In doing the resources for the tasks that we do have, certainly there is a sharing of resources across projects. And, so, I have to make sure that the resources are available, as well as the decisions that one project affecting another project, and your example of having the routes selected before you implement the YVC I just mentioned during the slides that we expect to have our final routes, or at least our preferred routes, designed and selected in the late 2006 timeframe in working with our stakeholders, and that would be in adequate time to support the YVC implementation and doing the training along those routes.

So, we do understand that there are significant interdependencies between the projects. We are working on those.

To your point on Nevada Rail not including truck, right now, I don't see truck as part of the Nevada Project, the Nevada Transportation Project planning. Truck is part of the national planning. To the extent that there would be a possible need for an intermodal facility in Nevada, we did include that as one of the questions we asked our stakeholders as part of our scoping process for the Nevada Rail EIS. Should we include the intermodal facility in that EIS? We were looking for input. We're waiting to see the results of all the comments that we got. But, the facility aspects of that would certainly be part of the Nevada Project.

But, the overall planning for the use of trucks is part of the Operational Project, because there's a continuity there that's part of the operational planning, how you look at the security, how you look at the planning, how you look at all the aspects. I see that more as a national activity than a Nevada specific activity. But, we are taking a close look at the possibility of trucks playing a significant role in the early years of our operations.

DUQUETTE: Thank you, Gary. Unless there's a really burning questions, especially since there's going to be a Panel meeting in the fall, I'd like to move the meeting along, because we're running a little bit late.

The next speaker is John Arthur, who is Deputy Director for Repository Development in the Office of Repository Development.

ARTHUR: Good morning. I'm very pleased to present to the Board here in Washington today.

What I'd like to do is summarize our project progress since the meeting in January, also talk a little bit about our path forward on the license support network certification, development of a license application, and then other continuing ongoing improvements in management and quality assurance.

The first exhibit is just an organizational chart. We've made some final alignments in April of this year, and this is the one I'll move ahead with towards the license submittal. Our main area is the one I emphasize as we've just recently hired employees, Concerns Manager, it's a vacancy I've had for about nine months, and I'm very pleased. We have a lady joined us from the Hanford site, Julie Goeckner, in July of this year. Great experience in employee concerns.

Then, I also moved Mark Van Der Puy of my office, who you've met before, up to the Safety Conscious Work Environment Coordinator to keep a focus on that critical activity as we move ahead towards NRC licensing.

Also, we're looking well past 2004 to the kind of organizational, the structure, and the contract management that's required as we go through the multiple phases of this important project.

I now want to move on to the next exhibit, talk about our management progress towards the license application. If I could have the next slide, please. This is a summary that I've shown consistently in previous meetings. This is out of our April monthly operating review. Again, the license is being prepared in accordance with 10 CFR 63, as well as the Yucca Mountain Review Plan. Right now, we estimate that we're at 68 per cent, and that's the progress at the time we reported out in the meeting. It also shows what I reported to you at the last meeting in January, 54 per cent weighted. I talked about before, so I'm not going to repeat it today. I just want to emphasize a few areas.

I'll talk in a few minutes about KTIs, Key Technical Issues, but as far as the physical development of the document, the license at 33 per cent, every day I'm seeing new chapters, sections of the license coming through in varying levels of detail. The goal is by the end of July, to have all those chapters internal to the whole review process within the Department of Energy.

The Preclosure Safety Assessment has advanced to 62 per cent, daily interface with the design, going back and forth actually hourly, not just daily.

The design itself has progressed significantly to 79 per cent complete. And, again, when I say that, that's not 79 per cent of the final design. That's the amount that's necessary to support a license application.

I might state that the subsurface, as well as the waste package design, for the license application is fully complete, and the surface, as I'll talk about a little bit later, is proceeding real well.

Current plans, we've talked in the past, the waste package prototype, the procurement was awarded earlier this year. We hope to have that prototype developed in June of '05, and then integrate that in with the welding processes in 2006. So, that's moving along very well.

I want to next move to Key Technical Issues, since that's an area of discussion. This is a summary chart right out of our monthly operating review. Just at the bottom, a summary that shows where they are in various stages as of the end of April. Of the 293 Key Technical Issue agreements, 214 have been submitted to NRC, and 99, as of this time, have been deemed complete by NRC. There's another 124, they're either in review by NRC, or we've got to provide to them for review.

The next area shows a little bit more of the workloads ahead of us. This shows for March to the end of August, our commitment is we would have all the Key Technical Issues addressed prior to the license application submittal. But, internally, we're trying to work that by September 1. What this provides is a color coding that shows high, medium and low risk as done by an NRC risk ranking. So, it shows the workloads we've got to complete. We've submitted I believe seven out of the eight, and we're trying to actually move in that. We realize for regular, that creates a big peak for review of about 45 in the July timeframe, so we're trying to move some of that in. Right now in our offices in Las Vegas, we have 40 under review, so we're hoping to get a jump start on some of those and exceed the schedule in May, but again, we want to make sure it's a quality deliverable before we send them over for NRC review.

The next area I want to talk about before I get into design is license support network certification. We are on target for our June 23rd LSN certification. As of two weeks ago, we started early indexing. It's also known as crawling, where we're providing documents across to NRC, and that process is underway right now. So, again, it's not just the license, it's also to have all the necessary documents available before discovery in the electronic courtroom.

The next area here just shows a little bit about the license application. I don't know if I've ever showed this one before. It's just a hierarchy of some of the documents. We estimate the license itself is going to be at about 5200 plus or minus, I mean, as we go through final reviews that will go up or down. There will be 5200 pages. You can see, about 400 pages will be in the sections on the left, physical protection plan, site characterization summaries, general description and layouts. Most of it is going to be in the safety analysis, both in the preclosure and postclosure safety.

And, then, below the license, we have supporting plans, analysis and modeling reports, and the whole architecture of documents that will be required to support something of this magnitude.

I want to next move into current surface facilities. Paul Harrington of my office I think gave a brief within the last six months to you, a little bit about the design, and we're making very good progress there. We have design inputs from Cogema based on the operations over at Le Hague, and extensive experience is being applied to our dry transfer facility.

What you have here, and, again, it's color coded. If you go over on the right, purple would be infrastructure readiness. That would be the development off to the south of the site, which will be initiated first, followed by the green, which would be the initial supporting facilities, as well as bare fuel handling facility. And, then into the red, which is a canisterized facility operations. The red, the green and the purple would be the first phase of development for the repository, and then you can see in the green, the dry transfer facility, that's the larger facility that would be constructed from Time Zero, but will continue while we initiate our first operations.

So, we're planning--I know you asked Gary a little bit earlier about schedules--we do have internal to the project, a fully integrated schedule where you look at the transportation, as well as infrastructure and repository, two key areas, we're continuing to mature that schedule. I'm owed by Bechtel SAIC a detailed engineering and construction schedule that will come in in late June. As we get that integrated into our master schedule, we're going to have technical interfaces with NRC, in the July/August timeframe, not just to look at design, but also construction schedule.

The next area I want to talk for a few minutes, and this isn't our color blindness test, this is a very busy slide, but it's important to make a point. First of all, this is a summary of the analysis and modeling reports, which many of you have been briefed on various aspects through the years, about 188 of those documents.

As many of you are aware, the Nuclear Regulatory Commission did a vertical, cross-cutting review of three of these back starting late last year, concluded that, issued a report, a report out on that on April 10th to the Department of Energy. And, as NRC noted in the technical evaluation reviews, DOE had continued to make significant progress in these products since the time of site recommendation, however, there were significant challenges still in the areas of transparency and traceability, as well as the corrective action program to alleviate the improvements in some of these documents.

As we relayed back to the NRC in a meeting just two weeks ago, we take their findings very seriously. We have since March, started an integrated effort in Las Vegas to actually take a look at all of the AMRs prior to putting them into TSPA. And, this really shows some of the challenges, because out of about 188 documents, we had well over 90 different authors located at five different institutions in different geographical locations around the U.S. For the final production of this license, that's all being done by a team in Las Vegas.

If I could move to the next slide, please? This is what we've called our Regulatory Integration Team, the centralized production of the license as it relates to analysis and modeling reports. We'll all go through this team. It brings together nine different teams of some of our best throughout the national labs, as well as Bechtel SAIC and other offices from Quality, Engineering, Project Controls and Operations under a single project manager to make sure each analysis and modeling report goes through the same level of review.

Some of the areas we're looking at in this team is the technical accuracy and validity of models and analysis, traceability of inputs and outputs among the models and analysis, considering the integration across and among AMRs, taking a look at each one for the appropriateness of assumptions and consistency between each AMR. So, it's a very detailed look to ensure that all of those are done consistently. Some are data models and software utilization. It's a very intensive effort.

The four step process will be completed by the end of May. Our teams have been working on this since late March, and I'm pleased to say that they're finding some of the similar areas that the Nuclear Regulatory Commission found. They'll come up with an action plan, and then what will happen, we've already started on that, the analysis and modeling reports will be revised between now and the middle of August, and then fully utilized for the TSPA.

So, that's just a summary. We are going to respond back to the Nuclear Regulatory Commission within two weeks with our response to their report. It reflects some of these processes, and I have high confidence it just won't be technically sufficient, each of those AMRs, but it will have the same level of quality and transparency on each one.

I want to now transition into another phase. Many meetings before, I know Mark and others have asked me about my confidence in the Quality Assurance, is there competition between schedule and quality, and where do we stand in the project. And, I feel we've made very good strides. We still have issues, challenges ahead, which I'll talk about. But, in this project, as I've said to the Board and to others many times, it's not just important to have a quality license application, but also to achieve and maintain management processes and a quality program conducive of an NRC licensee, and we take that very seriously.

I want to share with you, this is similar to a lot of other nuclear plants around the country, each one might present a little bit differently, but safety conscious work environment, and really four pillars. The first one on your left as you look at it is can employees go to their supervisors and raise any concerns without any fear of retaliation? On a survey we did last year, it showed 76 per cent had a favorable position towards that.

The next one in the red was the corrective action program, could people use the corrective action program. This is one of the ones that scored the lowest in our internal surveys, and this is across 2500 employees in the project, about 67 or 62 per cent, I believe it was, return rate. 58 per cent felt at that time, and that was about a year ago, that they had positive things to say.

The next area was if a person can't use one of those other methods, could they use the employees concerns program? The numbers came out to 76 per cent.

And, then, the last one was did we have effective methods to detect and prevent retaliation? We didn't have questions in that survey, so after that time, we've come back and we've set as a leadership counsel, a series of analyses and goals for us by the end of this year, which is reflected in the next slide.

Our goals, and this will be based on a survey that we do later this year, is to try to have that number for employees that raise concerns without fear of retaliation upwards of 85 per cent. It's a pretty good stride and goal.

Get the corrective action program up to 70 per cent. We knew there was going to be a challenge. We had to make some software changes, as well as enforce the management accountability, which is well underway now.

85 per cent for employee favoritism towards using an employees concern program. And, then, also, we'd want to have 100 per cent effectiveness in ways to detect any retaliation or harassment, of which we would have no concerns substantiated.

So, that's our goals we've set. We've taken a lot of management actions towards achieving that. And, again, these are the four pillars by which we'll move ahead towards the license process.

If I could have the next slide, please? Another area that I've showed consistently at our meetings before is our annunciator panel. I'm not going to, obviously, get into the specifics here, but I want to let you know we've made considerable progress, each of the managers, Department of Energy and Bechtel, as well as the national labs monthly, look at areas from schedule, quality, where we stand on all aspects of the projects. The areas that we've some significant improvements since last time is we closed out a data management corrective action that was open for over 322 days, as well as a software corrective action that was open for 1033 days, just, you know, about three years.

And, the importance of these are that this is the efforts of the project to move all these key areas into conformance with NRC requirements. The areas you'll still see on the top, which is work execution, still red, is the analysis and model reports. Until we have those reports revised and the Department of Energy has accepted those, that will stay in the red.

Model validation, we have a plan to have our model corrective action closed out in July or August of this year. So, at that time, it will move up into the red. So, this is a summary. We consistently look at that, as well as all the management processes down below.

I have a few others that I want to just talk about, detailed metrics below this, if I could have the next exhibit. If you drilled down in something like corrective action program, this is the one I showed you that had the biggest challenges, there are a number of measures that continue to improve. What this says is the adequacy of the quality assurance requirements description, requirements in all of our implementing documents, plans, and it shows you that consistently, we've had improvements occurring, less than our goal of 5 per cent, ever since about May of last year. So, that says that when our QA independent reviews look at these document, they found the necessary requirements inside of the plans.

The next area talks a little bit more about implementation, and that's how adequate is our corrective action plans. In this particular area, we've set a goal, which is pretty aggressive, about 85 per cent would be adequate on a once through review. We're still running below that. We're just running about 78 per cent. We have a six month rolling average, so it takes away the monthly peaks and variances there.

So, I guess in summary, what I'd like to say is the license is proceeding well. We have a number of challenges. Issues are coming up every day. We continue to manage those, but right now, we're about 68 per cent complete towards the December date. I feel that the quality assurance, and when I say QA, not just the technical products in the license, but also the management processes across are moving in the right direction. And, again, our goals right now are still certification of the license support network, June 23rd, and license submittal in December. And, as I told the Nuclear Regulatory Commission in our management meetings in Las Vegas last week, if anything gets off track and we find an issue there that's significant and we can't make that date, we'll make the proper notifications. But, right now, things are proceeding well.

So, with that, I'll end my presentation.

DUQUETTE: Thank you very much.

Dan Bullen?

BULLEN: Bullen, Board. Could we go back to the annunciator panel, Slide 12?

The two that jump out at me are the AMRs and the Model Validation Report issues. I guess the question that I have is that if TSPA is going to be a very integral part of a license application and you need time to, say, turn the TSPA crank, if those issues aren't resolved until August, will that pose a real problem with respect to the time to meet a December license application deadline?

ARTHUR: Dan, as far as the TSPA, we've continued up until recent to make runs, and most of these changes we're making aren't affecting the technical adequacy of those AMRs. The technical content overall is staying pretty much the same. It's the transparency, the level of detail, the quality in those. So, right now, we don't see an issue. It's most important to get all those done in August, and then we'll continue another run of TSPA. But, right now, things, at least in our schedules, look like that can be done.

BULLEN: Thank you.

DUQUETTE: Mark Abkowitz?

ABKOWITZ: Abkowitz, Board.

I just had a couple of very quick questions and comments. The first one has to do with Slide 11, I believe. And, I understand the aspirations are high, and I appreciate that, but I have difficulty with any goal that's 100 per cent. It's kind of like the person who says, well, we're going to have a zero accident policy. And, that sounds great, but, you know, the expectation of having 2500 out of 2500 people tell you that it's effective, you know, sort of engenders some doubt on the part of people's minds as to whether or not that's really realistic. So, I'd like you to comment on that.

And, then, my other question is that as you're charting this progress that you're making across lots of different areas of the project, I was curious as to what role, if any, third parties are having in the review and audit of that. Because from my familiarity with chemical plants, internal management tends to have a different view of the progress they're making than an external third party that doesn't have a bias.

ARTHUR: Good point. First of all, a clarification required. The first three are based on employee surveys. I'm glad you brought that up. 100 per cent is we have 100 per cent detection. Right now, we're actually doing a survey. We didn't do any surveys in the first go around for that pillar. So, we actually did a pulse survey recently. I think it went out to roughly 400 employees randomly. I should have data back on that one real soon. So, that wasn't 100 per cent favorable comments employees. It was to have 100 per cent methodology of detecting any retaliation. So, I want to clarify that one, and we'll share those results. They should be out in another two weeks from that first survey on that area.

Your next question had to be about independency. First of all, a couple areas. All the surveys are done by an independent firm. We knew there would be a distrust if we did that within the project. It's done by an independent firm. They've done similar surveys for other federal agencies, Fortune 500 and others who have a credible process.

The next area on an annunciator panel, we do have independent quality assurance reviews from our Quality Assurance office on that particular area. But, also we benchmark, I benchmark on a quarterly basis we many of the chief nuclear officers from industry. We sit down and look at our processes. We compare. So, we try to apply lessons learns. In fact, we have some of those people that have, you know, looked at our metrics and given us advice. So, I believe right now, they're very credible and I know there's been a lot of different interpretations, including by GAO, and I just say let's look at the facts and what the numbers show, and I will continue to have independent evaluation of that.

DIODATO: Diodato, Staff.

Thanks for your presentation. I liked your Slide 8. I think that's really a helpful way to organize the information. One of the things I noticed on that is that the muti-scale thermal hydrologic model I guess shows it in four different places and four different columns. I guess that reflects the utility of those analyses in the overall scope of the analysis; is that correct?

ARTHUR: I'll have to have some assistance from our folks. I think the answer is yes. But, one of the areas as we moved through I didn't mention, this was about 188 here, and this was an earlier one. Right now, as it comes through the regulatory integration team, it looks like about 104 of those are going to be used to support the TSPA. So, there is some integration. Some have been covered in multiple areas, as you said, so that's the purpose of this team, is to really make sure everything shored up to support the TSPA.

DIODATO: Well, that's helpful.

The other thing that I noticed on Slide 6 of your safety analysis report, you've got Item 4 there on the Performance Confirmation Program. Who had expressed interest over time in the Performance Confirmation Program? We're just wondering what the status of that is, and if the, you know, broad structure, if it's been outlined--

ARTHUR: We've had a number of meetings inside the project. We're in the process right now of revising the Performance Confirmation Plan to make sure that it ties directly to the design, the TSPA, so you can really look at not just what's going to ultimately be elements of the Performance Confirmation, but to make sure there's the necessary ties from the other programs. So, we've recently direction back to Bechtel for expectations on that Performance Confirmation Plan.

DIODATO: Do you have any idea when they're going to respond to you on those expectations?

ARTHUR: By July, late July timeframe. Claudia, is that about right? My boss tells me yes.

DUQUETTE: Priscilla Nelson?

NELSON: Nelson, Board.

This relates to Gary's presentation as well, and it's a question about how the AMR appropriation and the operations appropriations, and all these things that are feeding into licensing, are also being looked at to develop an understanding of what R&D or S&T needs could really be important here, and feedback into Margaret's Science and Technology Group. So, that connection is very often missed, and what are you doing in this timeframe to start to generate that flow?

ARTHUR: We've had, first of all, one of the areas I wanted to make sure is we had clear criteria, and excuse my definition of criteria, but I wanted to make sure there was real clarity, and the regulations drive pretty clearly what goes into Performance Confirmation, and then I believe Mark is going to talk after me on some of the tests that are underway right now and the test program, and then as well as the Science and Technology. So, we're looking right now to make sure we have clear criteria in the future, and probably maybe in the next meeting, it would be good to show you what some of the various types of test elements that go in each of those three programs, but there is a lot of work underway right now to define that.

NELSON: Nelson, Board.

I guess, as you build the AMRs, there's bound to be some places that some aspects could be enhanced or relatively weak, and making sure that that feedback to the Science and Technology people to keep an eye--technology is changing so fast that when the opportunity is missed, unless that's a real low friction interface.

ARTHUR: And, the other point I might add is we have recently authorized Bechtel to perform a certain amount of additional work, which will go out to some of the labs, as well as as things come out of the Regulatory Integration Team, we're keeping a checklist. I think in the next two weeks, I'll be briefed, and some of my staff, on what those are, and then make decisions on needs to go future into S&T or other immediate needs that we have. But, that is being well integrated in one master list in Las Vegas.

DUQUETTE: Thank you, John.

I'd like to move things along. Next talk is by Mark Peters, the Project Manager of Los Alamos National Laboratory, giving us a science update on the program.

PETERS: Thank you all for having me back.

Since it's been a year, I've got 200 slides to go through. That was a joke, Dan.

I want to first start, stay on the title for a minute. I want to tell you what you're going to hear about and not hear about today. I'm going to focus today on the ongoing science program that's being done as part of the repository program, which you've heard from me many times before. I'm not going to be talking about ongoing work in the Science and Technology Program. If that's confusing at all, we can talk about that maybe in questions and answers. But, this is focused on the ongoing science programs for the repository, and licensing activities.

Again, just to provide a status to you, I'm going to focus on the field program. What I'm not going to talk about today is any of the additional testing and data collection that's gone on in the area of, I'll call it, in-drift environment and corrosion. You're going to hear about a lot of that new information tomorrow, so you will not see that in this presentation. I'll leave that to the folks tomorrow.

Also, waste form, ongoing waste form work at Argonne and PNL primarily isn't in this presentation. That's primarily became of time constraints.

I'm going to start walking through the unsaturated zone, focusing again on the field program, the ongoing field program in the ESF, drift scale test, very brief on chlorine 36 validation, some of the USGS work on secondary fracture minerals. Moving to the cross drift, and review the work that's being done primarily by the Bureau of Reclamation on the geologic aspects of the Topopah Spring in particular, then move into hydrology, the Alcove 8, Niche 3 drift-to drift test, update on that. And, then, recall the bulkhead investigations in the cross drift where we have the back half of the cross drift, about a kilometer of that tunnel, bulkheaded off with no ventilation, looking for evidence of seepage or condensation.

Still staying in the cross drift primarily, but we're doing some additional work in some parts of the ESF on rock properties, thermal-mechanical properties. A brief update on work in the saturated zone. The Board just had a panel meeting in early March where they talked about this extensively, so this is a very brief update. And, finally, an update on the work that we're doing to look at volcanic probabilities in Crater Flat.

A diagram of the ESF shows the exploratory studies facility, the U-shaped tunnel, as well as the red cross drift that cuts across the repository block. In green here is the Solitario Canyon Fault. North is in this direction, so to the lower left, shows the various test locations. Again, I'm going to talk primarily about Alcove 5 drift scale test, and the work from Alcove 8 to Niche 3, the drift-to-drift test. Then, focus a lot of my discussion on work going on in the cross drift where we expose the deeper parts of the proposed repository horizon.

First, the drift scale test. It's a coupled processes test. We're looking at primarily evaluating the coupled processes in the rock. This was not set up to look at the details of the processes within the drift, but again, it's focused on coupled processes in the rock. I don't think I need to dwell on this slide too much. It's a large scale thermal test. We heated for four years, we're now about two plus years into a cooling phase. It's planned to go for a full four years. The heaters, both wing heaters in boreholes, as well as canister heaters in the heated drift itself, we've got boreholes drilled all through the test block monitoring temperature, pressure, relative humidity, as well as active measurements of various moisture movement, as well as collecting water and gas for chemical analysis.

This is really just a more detailed review of what I just went through briefly. Again, we're after the coupled processes, and this is a list of all the sorts of things that we've done, both as we characterize a test block prior to the test starting, that was characterization data, as well as detailed predictions, model predictions of what we thought we would see in thermal hydrologic mechanical chemical processes. And, then, during the heating and cooling phase, the measurement of the physical parameters. And, as I mentioned, periodically active testing using various geophysical techniques for moisture movement and air permeability measurements and also collecting the water and gas for chemical analysis.

Again, we're a little over two years into the cooling phase at this stage. Heaters were turned off in mid January of 2002, and as you can see, this is a representative sensor along the crown of the drift about halfway down the heated drift. It shows that we are well below the boiling point of water at this stage, approaching 70 degrees C. at the drift wall. Power had been completely turned off in mid January of '02, we basically turned the power off. We did turn the power off. We've let it cool naturally since that time.

I want to show a few representative slides of some of the cooling phase results. Again, you're going to hear a lot more about this test and how it's used in model validation tomorrow from Bo and Carl. So, today, I'm going to focus more of just a few snapshots of the sort of data that we're collecting, and leave the validation piece until tomorrow's discussion.

This happens to be one borehole ray that's halfway down the heated drift. This shows a cross-section of the drift with the boreholes, and what we're showing here is three different time slices after the heaters were turned off for three different boreholes. Temperature is a function of distance from the drift wall to depth in the borehole for both this up borehole, this inclined borehole, and this horizontal borehole, showing predictions in the solid lines, and the actual data in the symbols. Reasonable matches from the predictions relative to the data, there is some differences and we can explore maybe that in the questions if you'd like. We feel there's a reasonable prediction of the temperature within the rock as this test cooled.

We've also gone in and drilled a few additional holes. One of the things that we were interested in is what was going on chemically and mineralogically in the rock as it was heated, and then it started to cool. So, we've drilled a couple of additional boreholes, the so-called ChemSamp boreholes that were drilled from the observation drift, and we collected core and we've done both water extraction for moisture content measurements, as well as pore water analyses, and also mineralogical analyses to see if we see any evidence of significant dissolution or precipitation in the fractures due to the influence of the heat.

This just gives you an idea of what we've done with some of the core from that borehole. I'm going to show you in a minute some preliminary results on moisture content measurements for some of that core, and then also make the point that we've done detailed predictions of the moisture saturation changes, particularly in the matrix, and how that compares to the actual moisture content measurements in the borehole.

This is a representative prediction. This is for about a year and a couple months after we turned off the heaters. The contours are temperature, so this is the observation drift, the heated drift going into the page, this is that ChemSamp-3 borehole that was drilled from the observation drift. Again, the contours are for temperature at the time of April '03, and what's plotted here is the predictions of matrix saturation. That's what's shown in color codes. So, the boreholes start about here in more ambient area, went through relatively high saturation area, and then barely skimmed through the dry-out zone, and then back out into the wetter areas.

Next slide, please? This is the results, some preliminary results of some of the moisture content in some of those core. Moisture content is a function of distance from the front end of the borehole as you go down into the borehole. Again, these are actual data points for moisture contents of the core, and they're color coded according to their space location according to that prediction map that I just showed you. So, in general, we show a nice comparison of the actual moisture contents with what we would expect them to be based on the model matrix saturation values.

Switching now to Chlorine 36, again, this is strictly an update. We've told the Board in the past, and there was an extensive discussion of this work in the last meeting, or the meeting before that, we had Jim Paces, Bob Roback and Bill Boyle up here talking about the update on that. I just want to make the point, reemphasize the point that we do have an independent study going on of Chlorine 36 systematics. It's being lead by folks at UNLV and New Mexico Tech. They have a scientific investigation plan in place. They've laid out sample locations in the ESF, and the sampling should be starting imminently here.

They will be having quarterly meetings. There was one held in November, I believe, and those will be held on a regular basis once we get going with the field sampling effort. So, we're hoping that that will progress and we would like to see the results later in fiscal year '05 of this study.

Switching now to secondary fracture minerals. The USGS, Zell Peterman's folks in Denver, have an ongoing program looking at the secondary fracture minerals and what it tells us about a whole host of things, percolation flux, long-term variation in percolation flux, how that ties to climate change. Also, John Ake is going to talk some about seismic. There's been some interesting work done on what the minerals might tell us about the evidence of seismic shaking in the past as well. There's some interesting things they can do there. But, I'm going to focus today on just a brief update on some of the ongoing work we're doing, again, looking at time percolation flux to climate change.

We're starting to do a lot of, we'll call it, micro-analytical work. Instead of taking wholesale calcite grains and doing stabilized analysis, they've started to use micro-perp techniques at Stanford to look at detailed profiles of carbon and oxygen isotopes in the calcites, and also doing detailed geochronology on some of the coexisting opals, and that's allowing us to do an even better job of typing the details of how these fracture minerals grow in time back to the climate signal that we expect regionally, typing to things like the Devil's Hole record, and things like that.

Implications, we do think we see variation in growth rates based on drier conditions during the recent times, transition back to glacial, more wetter conditions during the tertiary, and that the sampling resolutions allowing us to see differences in growth rates and how that might correlate with changes in climate over time.

A lot of what I've already said, some interesting results. There's actually a fairly significant range in oxygen isotope composition of some of these calcite grains, 3 to 4 per mil is a fairly significant variation within a calcite grain. And, again, that could reflect variable climate signals, but we're working on getting H framework. That, you have to use primarily the coexisting opals to get that H framework. And, you can see that I've already said that.

There's some in situ microdigesting techniques that the GS is developing, and that's going to allow us to get some very detailed geochronology on some of the opals. So, we're going to another level of detail in looking at the stabilized tops of the radiogenic isotopes to tie to climate through both changes in water composition, changes in volume of water, and also time.

So, additional work that the USGS is doing, Jim Paces at USGS in Denver is heavily involved in this, we're looking at both fracture sets, samples from fracture sets, samples from faults, and samples from more matrix, and looking at the U-series isotopes, and those provide a geochemical indicator of percolation flux, not only amount, but also character as a function of geology, let's say.

So, basically, the degree of disequilibrium in the U-series tells you something about whether there's been uniform percolation flux, and ultimately low over time, versus focused flow.

At the bottom there, you can see when we look at the fracture sets and the matrix samples, the preliminary results suggest that you basically have very little in the way of any disequilibrium between uranium and thorium isotopes, which suggests that there's been basically long-term, fairly uniform percolation flux through the UZ. We're seeing some disequilibrium along the Bow Ridge Fault, and we're going to continue to look at the faults to see what that can tell us about focused flow along the faults as a function of time.

Switching to the cross drift, this is a diagram I've used many times before. It's color coded I hope in the same way. It's again showing the bottom of the north ramp of the ESF, the main drift of the ESF, as well as the cross drift. Let's talk a little bit about the code here. The test locations that are shown in regular font, in bold, are existing test locations in the underground where we've either got ongoing work or we've completed the work.

Those in the blue Italics are planned locations. So, do not yet exist. There's not yet testing going on in those areas. Also, along the cross drift, recall that the ESF actually does not get into too much of the lower lithophysal unit, which is the majority of the proposed repository horizon. The cross drift, we benefitted tremendously from doing that, in my opinion, because we were able to see the deeper parts of the repository horizon, particularly a lot of the lower lithophysal, and we've taken great advantage of that.

I should point out that this Board was instrumental in driving us towards digging that tunnel. I think we've gained tremendous benefit from the work that we've done in there.

But, what I've shown here is also the contacts as they're exposed along the drift. So, in code, this is the upper lithophysal of the Topopah Spring. We've got the middle non-lithophysal of the Topopah Spring, a significant portion of lower lithophysal, and then a little bit of the lower non-lithophysal before we get to the Solitario Canyon Fault. And, I'm going to talk primarily about the geologic data collection that's gone on throughout the cross drift, a little bit about the hydrology at the crossover alcove, the drift-to-drift test, and finally, something about the bulkhead investigation.

Another way of looking at the section, this is just a cross-section of Yucca Mountain, west to east, with the cross drift coming across, and it shows basically what I just said. This is the actual geology as it was observed as we mapped it prior to the mining.

First, the geology. We've done a whole host of detailed panel maps, traverses, detailed fracture mapping. Again, this has primarily been done by the U.S. Bureau of Reclamation and the U.S. Geological Survey. We've done, again, fracture characterization, also looked a lot at lithophysae abundance, character of lithophysal cavities. That's important for a whole host of reasons that I don't think I have to tell this Board how they influence the hydrology, how they influence the rock mass, thermal properties and mechanical properties of the rock. And, I'm going to get into that a little bit more.

I will not dwell on this. This is just a non-geologist guide to all the words that I'm throwing around. I talk about lithophysae. That's the holes in the rock. If you walk through the underground, they vary quite a bit, the abundance. That's where you get the non-lithophysal versus lithophysal character. You get a lot of different characters of fracturing. Some of the fracturing is from the cooling of the unit, some of it's from tectonic activity in the area, and you also get horizontal partings that are also from cooling of the unit. Again, we're mapping the character of all those, understanding the timing, and how they influence the rock mass properties.

I don't think I need to dwell on this. It's a lot of what I've already said. Again, a lot of panel mapping, five of them, a lot of traverses, and also focusing quite a bit, particularly in the lower lithophysal, this code here is a section of the tunnel that we're talking about. So, that's 1700 meters, 2500 meters down the cross drift, and that's primarily where the lithophysal unit is exposed. That spills over a little bit into the lower non-lith, but, again, focusing on lithophysal character and abundance.

We've also compared those results to some observations that we've made from video down as well as core from some of the surface based boreholes. In this particular case, we mentioned WT-2, which is down south. And, it's important to mention that the results are consistent, and again, it's a good type of the borehole geophysical log data that we have a wealth of in the surface based boreholes.

This is a summary slide that I'm not going to attempt to go through in detail. But, it's intended to show as a function of distance from the entrance of the cross drift, all the way down to the end of the cross drift, the different sorts of geologic data that we've collected in the cross drift over the past several years. Again, I talked quite a bit about a lot of the geologic observations that we've made. I should also say what's shown on here is the contacts. Again, this is in code, upper lith, middle non-lith, lower lith, and lower non-lith.

It shows the major faults that we've mapped, as well as the green lines shown the locations of the bulkheads that we have in the cross drift. And, again, it just shows the areas where we've collected data, where we've also done thermal properties, thermal mechanical properties tests at the rock mass scale in the cross drift in this case. We've also done a few tests in the ESF as well.

Just an example of some of the results. This happens to be as a function of distance along the cross drift, the abundance of lithophysal cavities, and then down here is a calculation of the actual area of the lithophysal cavities, just to give you a sense for the sort of data that we've collected, the coverage that we do have, particularly of the lower lithophysal in the cross drift.

A little bit about fractures. Again, the fractures are of different character, so the cooling fractures, some are tectonically related. The important thing is when you look at some of the detailed fracture surveys, they match up very well with the look that we did as we were mapping. You'll recall, we did line surveys as we were mapping, and we've compared these small scale fracture studies to those results. And, just again, reemphasizing the point, the areas that we've studied.

Switching to hydrology. You recall we've got the cross drift crosses over top of the ESF. There's about 18 meters distance between the two. We've taken advance of that geometry and put in a test alcove called Alcove 8. It's over top of ESF Niche 3, and we're doing a large-scale flow and transport test in the UZ, taking advantage of that geometry.

Just a schematic of the test. Again, here's the cross drift, ESF, you have Alcove 8, Niche 3. Again, this is about 18 meters. I'll show some pictures of the infiltration plot in a second, but we have both down looking and up looking boreholes. Those are primarily for active geophysical measurements to monitor the travel to moisture front.

A picture of the test bed. This is a picture from the back of Alcove 8 looking out towards the cross drift. This is Niche 3. You see the collection trays in the roof of Niche 3 that we used to collect the water that might seep, and also shown here is a fault. There's a fault in the back of Alcove 8 that we did some additional testing on. I presented those results already in the past, and now we have a large, a relatively large infiltration plot broken up into twelve sections, where we're doing a larger scale of flow and transport experiment.

It's also important to point out here that the actual contact between the upper lithophysal and the middle non-lithophysal is exposed about two-thirds of the way down to Niche 3. So, we're actually travelling through two different sub-units of the Topopah in this test.

What I've already said, again, we tested a fault, in the back, that's exposed here, that trench, and we're now doing a large-scale, a larger scale infiltration plot. Actually, you can see the white part of that plot right there just beyond that water container.

Some representative results. This happens to be from about a year ago. Plotted in blue are the actual infiltration rates in Alcove 8 as a function of liters per day, and then in red are the actual seepage results in liters per day as collected in Niche 3. There's a delay. We see the development of distinct flow paths.

Here, the last month or so, we also introduced a set of tracers. This was just water with lithium bromide. We've also now introduced a set of tracers, and that will allow us to get more information on transport phenomenon within the UZ. I believe those were started in March and turned off in April, so we're still waiting for arrival. We have a set of predictions on what we think we're going to see. It will be interesting to see how those compare.

Moving to the bulkheads, again, we had a whole back half of the cross drift that had been mapped and our testing plans didn't have a lot of activity going on back there, so there was a decision made to basically bulkhead them off, not ventilate, and look for evidence of seepage.

We monitored back there for liquid water, and we've talked about this several times in the past. We have seen evidence of water back there. It's due to condensation, but that's where we're at right now. I'm going to show a little bit of review of some of the results, a few pictures. I've got a lot of pictures in the backup. That test continues. We continue to monitor what's going on behind the bulkheads.

I should also say there's a very detailed slide that you probably need, it's going to challenge your eyes, but this has been a very long test in terms of how the bulkheads have been opened, closed, when and what-not, and, so, there's a slide back there that shows that chronology. I don't intend to go through it, but it may be useful for some of you all who are interested in the details.

This is just a picture to show the character of some of the moisture that we've seen collected back there. We've seen it collected. This happens to be a picture looking up at one of the ventilation ducts, and we see droplets forming on the ventilation ducts. And, then, what you're looking at here is a picture looking down on the floor. We had some plastic collection sheets, and this is a puddle of water that gathered up over one of those plastic collection sheets. We see it gathered on the conveyor belt, and when I say we see it, that's because we periodically open the bulkheads and enter and walk through and do observations, empty our sample bottles, do chemical analysis, et cetera.

Again, this is a summary of the observations. There's not uniform moisture distribution when you walk the tunnel after you've opened these bulkhead doors. So, this just gives you a sense for how it's variable. We think that's primarily attributable to the presence of I'll call them heat sources back there. Early on, we still had power running to the TBM, tunnel boring machines parked at the back end of the tunnel. That was driving, we think, a lot of the condensation. So, if you look at that area back there, it's dry and it actually remains dry, but as you walk through the tunnel, again, there's some variability in the moisture distribution.

So, we've also been monitoring relative humidity in the tunnel, and also near the rock, in the near-field rock, as well as temperature changes. And, as soon as you close the bulkheads, it's clear the humidity rises very quickly. I mean, there's clear communication between the rock and the drift, no surprise. Spatial variability in temperature, again, and also moisture distribution is likely due to heat sources, very low power heat sources, actually. It's amazing what sort of temperature gradients drive some of these phenomena, which I'm surprised.

But, multiple lines of evidence, we've done chemical analysis of the water, the character of the water, the volume of the water, the way it's distributed within the drift relative to the heat sources all show that they absorb moistures from condensation. It's from temperature differences within the drift, and between the drift and the surrounding rock.

Let's switch now to thermal properties. Dave, how much time do I have? I'll be okay.

Thermal properties, again, we've done a detailed laboratory field program. I'm going to speed up a little. I've talked about the laboratory and field program in the past. This is really just to bring up that we're now conducting two additional tests, Tests 4 and 5. Those happen to be in the lower lithophysal and the upper lithophysal, but now is exposed in the ESF down by the south ramp. Similar layout, single heater holes, with two holes with thermal couple strings in them with, again, drying out a small volume of rock to get rock mass thermal conductivity.

This is a review slide. You may not recall, but I've used this before. This is thermal conductivity in watts per meter K as a function of porosity of the sample. This shows the results of all the lab experiments that were done by Nancy Brodsky and coworkers at Sandia over the past few years. And, also plotted on here at what we call an arbitrary porosity, meaning that it's not the actual porosity of field scale experiments as it's shown here, but it just shows how the field experiments compare to the laboratory work that's been done. This is a well integrated laboratory field program, very similar to what we're doing in the mechanical properties area.

Representative results for Test 4, this is showing results from one thermal couple hole as a function of time. We've also added a component now looking at the water redistribution as we heat the rock. So, it's also showing the neutron logging data. So, the heater runs perpendicular, so the temperature swing is running towards the heater. You see the bump in temperature, and then it runs to the other cool end. You can use this data to do some inverse modeling, and come up with thermal properties, thermal conductivity, and other thermal properties.

This is an updated table. You've seen this table before as well, showing the five tests now, and how those compare to the ranges of thermal Ks that we use in the models. Also, down at the bottom here, I've shown the range of values that are used in the thermal hydrologic models. You may hear more about these tomorrow from Bo primarily, in his presentation.

Tom Buschek and his folks at Livermore have also done an analysis of the first thermal conductivity test using NUFT, and the bottom line with that is they get results that agree quite well actually with Nancy's work, and clearly show that the thermal hydrologic effects on the test were negligible. So, we really are getting reasonable rock mass thermal conductivity values.

I won't dwell on this. This is the results of Tom's simulations showing how he's matched the data for, again, Thermal Test 1.

Moving now to the mechanical properties. Again, similar program, looking at in the ESF in the cross drift, combined with the laboratory program, scale effects, lithologic effects, lithophysae effects on rock mass properties. We did a lot of large diameter coring, taking samples, doing laboratory work. We've also done some in situ flat-jack tests where we press on the rock to get at strength parameters. And, the field tests are complete, and we continue to do some laboratory measurements on some of the samples we took.

The laboratory program, we've presented results. I've had representative slides. Mark Board has talked to you in the past about strength and other parameters as a function of lithophysal porosity and strain rate, et cetera. This is a couple slides on some ongoing work that we're doing on creep, creep failure of some of the core. So, this is work ongoing, corroboration of Sandia in an external laboratory, again, relatively small diameter samples, and we've completed twelve samples to date. And, the next slide is going to show some representative results. Again, these are creep tests, so what we are showing here is--I don't want to get into the details, we can talk about it maybe in the questions if you're interested, but it's a creep stress, and a way of representing creep stress relative to time to failure of the sample due to creep. And this is in seconds, this is in a percentage because it's been normalized to the overall strength. But, the bottom line is the relationship is consistent with the work that we've done in reference to in drift degradation model that can support the LA.

Next, please? Saturated zone, Nye County. I'm switching now to the SZ. Lots of water, as opposed to the UZ, very little water. Nye County has an ongoing program. This simply shows the locations for the Phases 1, 2 and 3 for their boreholes that were drilled. As you all well know, we've done cooperative work with Nye County in terms of sharing samples, and we've done a whole host of measurements and modeling and used, I think, the results of their program to great advantage for the program.

Next slide? This is another slide just to show the location of the three additional boreholes that were drilled for the Phase 4. They moved up Forty Mile Wash, so Yucca is up here, so we're basically moving up Forty Mile Wash.

Next slide? I want to focus again, the Board heard a lot about this in early March at their Panel meeting. I have a few slides here that talk about some of the work that's been done, additional work that's been done on hydrochemistry. Gary Patterson and folks at the USGS have done a lot of this work, again, using the hydrochemistry to validate the SZ model.

Next slide? Updated slide. This is a map view of the area in Yucca Mountain. Up here, Crater Flat, Amargosa Valley. This is a summary plot that uses the hydrochemical data and ties it to different I'll call it hydrogeologic, to a hydrogeologic framework at the different facies. So, the different components of the flow system. This is, again, an interpretation that's been made by using the hydrochemistry data. It's interesting to compare this to the actual model results when that's done in our AMRs that are being prepared for LA.

Next slide? You also mentioned in your letter about the sonic core. I believe you saw the Nye County facilities when you were out on your tour. They've done one hole with a sonic core technique, and the nice thing about that is it provides us very coherent samples of the alluvium. The alluvium is not easy to sample, and that's an important part of our system downgradient. So, we are working cooperatively with Nye County. One of the things that we're doing is we're taking hydrochemical samples from that core, and we have experiments underway to do detailed inorganic as well as trace element, inorganic trace element, major element, minor element, as well as isotopic analyses of those waters. Hopefully, in future meetings, we can talk about some of those results.

We are also doing flow and transport, planning flow and transport experiments with some of those core as well, which will be very interesting.

Next? Finally, igneous, your letter from December commented on some of the stuff that we had done in the past on igneous. As you're aware, one of the things that we have ongoing is looking at some of the additional anomalies that have been identified in the area, and have been identified as potential buried volcanic centers. And, so, it's important that we better understand that to refine our volcanism probabilistic analysis if necessary.

So, we're doing a detailed aeromag survey. Recall Nye County, in cooperation with USGS at Menlo Park, did a detailed survey back in the '99 timeframe. We are now doing some additional surveys.

The next slide shows just a map of the area. In blue is an earlier version of the area that we were going to do the detailed survey. My understanding is that we are now planning on filling in this area so that we will also survey over in here. And, we're also extending the survey to the south. What's shown on here in red triangles are the actual volcanos. The circles are the anomalies that were identified during the 1999 survey. Then, there's also shown on here planned drill holes and contingency drill holes. After we do the survey, we'll interpret the results.

In the plan, it would allow us to go and drill some of those anomalies if warranted, to do some detailed geochronology on some of those centers. That would be very important to get the age control. Again, that's only in the plan. We've got to evaluate the survey prior to deciding what we're going to do. So, that's ongoing.

And, then, I think the final slide is just a picture of the helicopter pulling the tool, it's about 60 meters out to--that's out in Crate Flat actually, looking out towards Death Valley.

And, finally, summary. Sorry if I had to go a little quick, but I wanted to try to give you all a feel for the ongoing science program in support of licensing activities. We continue to address uncertainties and build confidence in our models as we move forward.

And, I'll take questions.

DUQUETTE: Thank you very much, Mark. You always amaze me how much material you can pack into about 30 minutes.

PETERS: Hopefully it wasn't too hard. Hopefully, it wasn't too hard to get.

DUQUETTE: No. Priscilla?

NELSON: Nelson, Board. Thanks, Mark, as always.

There are a couple questions. One deals with the minerals, mineral studies in the UZ. Part one is are you controlling these two, cover all block units, spatial control lithophysae size, or are they being controlled within the database? And, then, secondly, is the drive percolation rate information being used as a way of testing Alan Flint's model for percolation rates expected to vary across the mountain?

PETERS: Okay, let's take the first one first.

The samples are taken within a geologic context. They're oriented. I mean, Zell could probably stand up here and tell you a lot more, but, yes, they're taken from different characters, low angle, high angle fractures, lithophysal cavities, where they occur. So, I think we've got that controlled, and documentation on how they're current geologically relative to what they're telling us chemically. I think I answered.

NELSON: Yeah. Nelson, Board.

Just what I'm looking for is the connection between what you're observing on the mineralogy relative to size.

PETERS: Right. Yeah, I don't know, I'm probably not going to be able to tell you if there's something systematic about the character as a function of size of the lithophysal cavity. I'll say this, that in the cavities, I think you're aware of this, they tend to be focused spatially along the sides and bottom as opposed to the tops.

But, in terms of variation and size, maybe we could talk to Zell about that later, and I could get an answer.

NELSON: Okay. What about the ability to use the inferred percolation rate. You're calculating or inferring a percolate rate based on rates of deposition. According to Alan Flint's model, that would be expected to vary across the mountain. Is your data showing that or supporting that?

PETERS: No, I'm with your question. I'm just trying to remember if we see the spatial variability. I'll say this. As a multiple line of evidence, it's always given us great confidence when you look at those long-term growth rates. It typically corroborates a percolation flux of 1 to 10 millimeters per year, which is what we see from other lines of evidence.

Now, Zell might have to answer. Do you see spatial variability across the block in terms of the percolation flux? In terms of what you see in the character, is there spatial variability across the block?

PETERMAN: There is spatial variability. And, I guess the best example is under Drill Hole Wash, which in that section of the ESF, that's the greatest abundance of the secondary minerals, and that fits Alan Flint's infiltration model in the sense that he would say that under the present climate there isn't much infiltration. The water transpires back out before it can get into the bedrock, for the most part. But higher than 10,000 years ago, very likely, there was, and that's certainly consistent with the abundance of calcite in that interval. Elsewhere, you're sort of restricted to, you know, what's available in terms of depositional sites. You have to have, you know, someone open five cavities--there are large intervals where there aren't such figures.

NELSON: Nelson, Board. It just seems like that's a real interesting thing to follow up on. It's such a fundamental premise of the way the mountain operates.

PETERS: Good point.

NELSON: And, just to hit one more thing. When you plotted rock mass thermal conductivity information, there's been a lot of accent on water content, moisture content, as a function, but I was really looking for something that also includes volume tested, because the sensible rock mass, a lot of the tests were run on core.

PETERS: Right.

NELSON: And, the sense of the volume of rock measured, rather than a wider content, which is necessarily itself a point measurement. The volume is going to be very important.

PETERS: Agreed.

NELSON: So, if you have plotting versus volume would be very interesting.

PETERS: Okay. We can certainly do that.

PARIZEK: Parizek, Board.

I had similar comments as what Priscilla asked about in terms of episodic flow. If one gets from the various dating of calcite growth, for instance, where you really do get some evidence of not a long-term average percolation flux, but variability with it, then how that might fit into that modeling.

Then, as far as Page 29 on the cross drift seepage experiments, was anything done here with colloids, either adding them as microspheres or just capturing water from below to see whether or not anything is coming through as particles.

PETERS: There was intent, but did we add microspheres this time? No, we still have not yet added microspheres into the tracer mix. That's in the long-term plan for the test, but we haven't yet done that.

PARIZEK: So, that's still being scheduled?

PETERS: Yes, and whether we do it or not, I can't stand here and say we would absolutely do it, but it's under consideration for the long-term future of the test.

PARIZEK: Right. Then, as far as the heater experiment, there's dryout zone shown in the one diagram, and you had two figures, which I guess you could overlay one with the other. One showed the model forecast of dryout, and the others are the points where actual measurements were taken. So, am I correct I could overlay those two figures?

PETERS: Except that the predictions are saturations, and the data are moisture content.

PARIZEK: Okay.

PETERS: So, I can talk to you separately, and we can dry to do that conversion.

PARIZEK: Also, Parizek, Board, again, looking for some evidence of this drift shadow development, it seemed like there's sort of a symmetrical dryout.

PETERS: Right.

PARIZEK: And, we're not getting a tear drop look to it yet, or maybe it shows in other datasets. Can you comment on that, whether we see evidence of the drift shadow? Any funnel tests, for example?

PETERS: Right.

PARIZEK: Right now, it seems symmetrical as a dryout point.

PETERS: And, I would say from my perspective, we haven't probably laid that test out well enough to really look for the geometry of that shadow. You know, if we were really to go after the drift shadow, we would have to conceive of a very different--I don't think you could really say much about the drift shadow from that, at least the way the test is laid out.

PARIZEK: And, Page 47 is the chemistry, which is really like a collaborative evidence of modeling, and I guess these are not new data points. These we probably would have seen in the March panel meeting?

PETERS: Yes, you probably saw this data. I presented something like this in the past as well, but this has been updated with the new data. But, Gary probably presented it.

PARIZEK: One can almost see the green as being sort of a shot straight south, versus the southeasterly path, and, so, this is multiple lines of evidence to support a southeasterly southerly flow has to be kind of dealt with. And, the chemistry is just one of those independent lines of evidence that you folks are using, but it's worth commenting on.

PETERS: It tends to be more southerly as opposed to southeasterly? Right, this particular dataset.

PARIZEK: Thank you.

DUQUETTE: Leon Reiter?

REITER: Leon Reiter, Staff.

Mark, I noticed that in your cross drift, you have a--planned thermal outgo. There's been a lot of times discussing whether or not the conditions are right for deliquescence, and at least localized corrosion during the thermal pulse--

PETERS: For that thermal test?

REITER: Yes.

PETERS: As currently conceived, it's not going to go after conditions inside of a drift. It was conceived as a coupled processes rock test. That's not to say that we couldn't try to set up a test. If I was to go--if the details would go--just localized to me are much more amenable to more controlled laboratory experiments at this stage. We could certainly try to go after some of those objectives in that test. One of the things I would go after in that test was seepage before I'd go after deliquescence inside of a drift. We can certainly talk about that. It's on the books, but it hasn't been fielded. It's been reevaluated this summer as to whether and if we conduct that test. And, so, I think we can put that in as one of the possible objectives. But, my first inclination would be that would be tough, deliquescence inside of--the controlled manner.

DIODATO: Diodato, Staff.

Mark, thanks again for your usual excellent presentation. Very informative. Slide 8, this is the drift scale test, and you've got temperatures here on the ordinate and centigrade; right?

PETERS: Yes.

DIODATO: So, then, for Slide 10, there's the cross-sectional image now. So, are these temperatures also then in degrees C?

PETERS: Yes. It's a contour map.

DIODATO: Right. So, looking at, you have the saturations plotted down to as low as 80 per cent, and then I'm looking at the, say, 100 degree boiling isotherm, I still see saturations there between 80 and 90 per cent above that, and then it for some reason drops off to zero. There's nothing plotted below the 80 per cent number.

PETERS: Right.

DIODATO: Is that normal that there be liquid water still in the zones above boiling temperature in this experiment?

PETERS: You mean--we have relatively low saturations.

DIODATO: Oh, these are 80 to 90 per cent saturations.

PETERS: Yes, I'm not going to be able to speak to the details of that probably standing up here.

DIODATO: I was just wondering about it, because it really--

PETERS: It's a good question.

DIODATO: It's very fundamental. And, the other question was on Slide 50, you got the volcanic centers and the plans for the drilling. I don't know, I was just wondering, you've got drill hole locations planned. There's the observations of the anomalies. Is there some reason they're not in the same location?

PETERS: I think it's probably just so they didn't overlay the symbols.

DIODATO: The graph?

PETERS: Yes. The drill holes would be intended to drill the anomalies if warranted. I want to be clear, though, we're not saying we're going to drill all those anomalies. We've got to evaluate the aeromag data before we decide what we're going to do.

DIODATO: Got you. Thanks.

PYE: Pye, Staff.

Slide 37. This data shows thermal conductivity all tested below 100 degrees. Is there a reason why?

PETERS: It's because it's the first phase of the test. We're in the process of heating it up to go above boiling now.

PYE: Okay. And, Slide 38, you've indicated some test data here, and then at the bottom of the page, you've indicated a range for thermal hydrological models for, for example, lower lith from 2.14 watt meter to 1.3. How do you justify that range?

PETERS: What I did here at the bottom was simply take the ranges that are used in both the drift scale seepage model and the multi-scale model, their means and their plus or minus standard deviations, and simply wrote them down as a range there just for your information.

PYE: So, the 1.3 is a mean minus some standard deviation?

PETERS: Yes, the means are basically what you see here.

PYE: Okay.

PETERS: Close to it.

PYE: All right. I remember in SSPA, we looked at a lithophysae range of extreme value from zero to 25. Well, field data clearly shows now that the mean lithophysae porosity is around 25 per cent, and can be as high as 52, 56, if you include the large lithophysae population as part of the general population. So, again, I'm sort of intrigued as to why you bounded it just at 1.3 watt meter K, when if you do a simple volume averaging model, it would indicate that it would be, in fact, lower.

PETERS: Even lower.

PYE: Yes.

PETERS: I think Tom, the multi-scale model, he's done sensitivities probably maybe even down below that, John. I'm not going to be able to defend the details of Tom's sensitivities, but he's done a lot of sensitivities probably looking at even lower thermal Ks.

PYE: Okay.

PETERS: You'd have to look at his AMR. They're also looking at--one of the things that they're doing as part of the regulatory integration effort that John mentioned is they're looking at the details of the lithophysal porosity data relative to the thermal K data, and possibly doing some technical adjustments. I'm aware of that as well.

PYE: Well, I just finished reviewing the drift degradation report, and, again, from a regulatory integration point of view, it seems like you're using the old thermal conductivity data.

PETERS: They're probably ironing out some differences in what parameters they're using with thermal properties. I will not disagree with that.

PYE: Right.

PETERS: Consistency is important, as you well know.

PYE: Right. And, again, from a repository design point of view, all things being equal, thermal operating mode, ventilation, duration, et cetera, the implication is if you hold the thermal criteria as they currently are, it would indicate you need a larger repository, based on thermal conductivity decreases.

PETERS: I'm not going to agree with that.

PYE: Well, I'm saying if you hold all the parameters and the thermal criteria as they currently exist, it would require a bigger repository.

PETERS: I'd like to see your analysis of that.

DUQUETTE: Ron Latanision.

LATANISION: Latanision, Board.

Slide 43, could you just remind me of the point of this work, the objective?

PETERS: It's intended to look at how rocks may fail to creep.

LATANISION: Okay.

PETERS: And, it's important for long-term drift degradation primarily, once you have an opening, how it might creep as opposed to the instant failure or it's basically the rock's creep to failure. So, after you make the opening, they creep over time, function of temperature, and ultimately fail. That's a very important parameter for understanding long-term stability of the opening. Does that help?

LATANISION: Well, it helps, and I realize that the test that you've identified, and according to the previous slide, twelve samples have been tested at this point.

PETERS: Right.

LATANISION: And, this is at 125 degrees Centigrade, so they're dry. But, is there an issue associated with moisture in--the static fatigue of ceramics in general, is dependent on their environment. Would moisture make a difference, and is that important to you?

PETERS: I think, yes, I think it would make a difference. It's important. Separate from the creep test, we've done some of our other mechanical tests as a function of temperature and strain it in other parameters. I can't speak to how this would change as you went up in saturation. But, that variable has been taken into account.

LATANISION: Is it on the radar screen in terms of exploring it?

PETERS: I'm not sure what future creep tests we would do at higher saturations, but it's certainly something we have to discuss in our basis, so that we understand the effects of the lower temperatures.

DUQUETTE: Thank you very much, Mark. I think you're done.

I'm going to call for a very, more of a stretch than a break, for about five minutes, just so people can get another cup of coffee and stretch a little bit. And, I'd like to get us back on track, as we're about a half hour late at this point.

(Whereupon, a very brief recess was taken.)

DUQUETTE: I want to make one announcement. Because we're running so late, we're going to take an early lunch breach. We're going to break at about 11:45, and come back at about 1 o'clock for the afternoon meeting, so that we can run the corrosion session concurrently, sequentially.

The next talk is by John Ake, the geophysicist from the U.S. Bureau of Reclamation, and he's going to update us on the seismic studies.

AKE: Well, thanks for the opportunity to provide an update to the Board on where we've been going for the last year or so in the development of seismic inputs at Yucca Mountain.

I'd like to spend the next period of time talking about a very brief recap of some of the information that you have presented last February in the Board meeting in Las Vegas, with a particular emphasis on the rather problematic low probability seismic events. And, then, based on that, I'd like to walk you through where we're going, where we've gone in the last few months, and where we see ourselves going in the next few months, in our effort to try and develop more realistic low probability ground motions for the Yucca Mountain site.

A bit of background here. Because our regular code of requirements are for us to use a risk-informed approach to repository performance, that requires that our seismic design inputs be cast within a probabilistic framework. With that in mind, back in mid Nineties, the project undertook a detailed probabilistic seismic hazard analysis, PSHA, for the Yucca Mountain site.

This was a very structured and detailed evaluation that followed a well developed sort of procedures. That particular methodology has been reviewed by the National Academy, and previous accepted by the NRC in other nuclear facility licensing processes.

One of the real advantages of the PSHA process is that it allows a very good framework for the inclusion of both scientific knowledge based uncertainties, as well as aleatory variability in all of our different input parameters and outputs.

An important point I'd like to point out here that we're going to come back and talk about again in a couple moments is what we call the aleatory variability in ground motion attenuation functions in the current PSHA are modelled as unbounded lognormal distributions. It's a very important point.

Another couple of issues I want to point out here as well. At the time of the conduct of the study in the mid to late 1990s, we anticipated that the region of the risk frame, if you will, that we would be interested in were generally on the annual frequencies of exceedence in the range of 10^{-5} to perhaps 10^{-6} based on previous experience at nuclear facilities.

Subsequently, 10 CFR 63 was issued, and in particular, Subsection 114 of that particular document requires us to at least consider events that have probabilities of occurring of one part in 10,000 within the 10,000 year regulatory compliance framework. So, in other words, that opens the door to at least consider events that have probabilities as low as 10^{-8}.

Another important point here is that it's our requirement to use the mean seismic hazard in our design and performance confirmation.

A quick recap of the PSHA. The PSHA consists of two basic elements, source characterization, and ground motion estimation. The source characterization is just the development of the inventory and characterization of all the fault sources or seismic sources that could provide vibratory ground motion or fault displacement hazards of engineering interest at our site.

It involves developing estimates of the slip rate, or how often earthquakes occur on a particular source, the maximum magnitude that might occur on that source, and the geometric considerations of the sources, the geometric attributes.

I should point out that all of the inputs that go into that part of the source characterization model, maximum magnitude, slip rate, et cetera, are all represented in the PSHA framework as bounded distributions. The map view here, we point out the proposed repository shown here in pink. I only show this to point out a couple of things. One is the existence of Solitario Canyon Fault along the western margin of the Yucca Mountain block here, and the other is the Paintbrush Canyon/Bow Ridge System on the eastern block, boundary of the block.

The source characterization was supported by lots of very detailed studies, including trenching. Once we defined all the seismic sources, the next step in the PSHA process is to, for a given magnitude and distance on a source, is calculate the ground motions at our site. To do that, we availed ourselves of the available empirical data, of which there's, for our site, type of site, not very much. And, we supplemented that with a large number of theoretical ground motion estimates, calculations.

And, again, I point out that we used in the source characterization, bounded inputs by the ground motion attenuation functions that we get out to calculate the ground motions, given those sources, unbounded lognormal inputs.

So, after we have done all of that, the output of our PSHA calculation on machinery is a set of what we call seismic hazard curves. They're produced for a range of vibration of frequencies. In this case, we show three, the high frequency, or peak ground acceleration, the lower frequency portion of the vibrational spectrum, ground velocity, and intermediate one here.

There are three things I wanted to point out on these particular curves, the first and most obvious is the very large ground motions predicted at the low annual probabilities, are below 10^{-6}. You can see we predict for peak ground accelerations, very large values, six or seven GUs here, and maybe as much as 12 GUs here for 10^{-8}.

The second thing I'd like to point out is the shape of these curves. And, again, keep in mind here that we're focusing on the mean curve here. Notice the change in shape of these curves as we progress down through lower and lower decades, and probabilities face here. The mean and high fractile curves here almost become asyntotic to the X-axis. This is, of course, troubling to any physical scientist because this implies for arbitrarily low probabilities, we would predict arbitrarily large ground motions, which doesn't intuitively make any sense.

And, the third thing I'd like to point out is notice the extreme asymmetry in these probability distributions. Just what you're looking at here is for a particular ground motion value, what is the distribution on that? Notice the strong deviation of the mean from the median at the low probability level. That's a function of very large values being included from the unbounded attenuation function inputs.

Next slide? An alternative and important way to look at the ground motion hazards is to look at what's called the deaggregated hazard. In this case, we show the deaggregation by magnitude, distance and epsilon.

The thing I'd like to point out is we have to do the hazard for a particular vibrational frequency. In this case it's 5 to 10 hertz. And for given annual probability exceedence, and in this case, it's the example we showed here, for the 10^{-7} hazard.

What we can see here is that virtually all the hazard at this level, annual probability level, is coming between magnitude 5.9 and about maybe to 6.8 earthquakes. And, it's all coming within 10 kilometers of the source. This is the contribution to the hazard. This level is arising from the Solitario Canyon and Paintbrush Canyon Fault systems.

The thing to point out here is these very large ground motions are not coming from extraordinarily large magnitude earthquakes. They are coming from moderate magnitude earthquakes very nearby. It's not necessary to have an extraordinarily large earthquake, one consistent with getting its own mini-series during sweeps weak, or anything, but these are actually moderate magnitude earthquakes.

So, when asked well, why are the ground motions so big, well, the ground motions are so big, as explained by epsilon here, which can be thought of as very similar to sigma, the number of standard deviations away from the median. And, this shows that virtually all the hazard, low probability, is being contributed by contributions beyond +2 sigma. This is where that tail, net distribution comes back to adversely affect our hazard results.

Next slide. So, summarizing the existing results that we came up with under PSHA, and you were briefed on last February, we, for low annual probabilities of exceedence, we predicted very large mean ground motions. Also, asymmetric probability distributions in that low range.

If you, for a moment, accept the premise that these very large ground motions are possible, and try and back calculate what source parameters, what parameters at the seismic source would be required to produce those, you end up with extraordinarily large estimates of things like the dynamic stress drop. Those estimates are far beyond any estimate anyone would postulate, at least in print so far.

Secondly, if you take our seismic inputs and drive our site response model with those very large inputs, you calculate extraordinarily large strains in the near surface rocks. This is an extremely important point here, and we're going to come back to this in detail later. But, this suggests to us that there is a disconnect between what's possible at this site, and the limitations imposed by the rocks themselves.

We were aware of some of these problems, and pointed them out in the February meeting. The Board expressed their reservations about moving forward with these extraordinarily conservative and possibly unrealistic values to the Department in a letter last spring. In reaction to our own concerns, as well as the concerns voiced by the Board, we have decided to move forward with trying to develop some more realistic estimates of the low probability motions, and we're trying to do this within the basic framework provided by our existing PSHA study.

The fundamental assumption we're going to base this on is what I mentioned a moment ago, in that there are very real and definable limits to the strengths of the rocks at the repository elevation, and that the ground motion and the amplitudes that one can transmit through those are fundamentally determined by those strength properties.

And, what we're going to try and do is establish what those shear strain limits are that would produce failure and fracture within the tuff units themselves. We have to keep in mind that this limit or criteria we define has to be consistent with our ability to resolve what that would look like within those rock units at repository elevation, and it has to be consistent with our geological observations. And, once we've defined that shear strain criteria, then we can go back and calculate what peak ground velocities in this case, or ground motions, are consistent with that strain threshold. And, we think by doing that, we will have a more consistent and representative set of low probability ground motions.

How do you go about determining the limits to the ground motions? Well, our assessment thus far is this is a hard problem. It's not trivial. I'd characterize it as a cutting edge research topic. The only place this has really come to the fore have been here at the Yucca Mountain project, and on the PEGASOS project in Switzerland, also a nuclear related facility.

The PEGASOS project has actually moved a little bit ahead of us on this in terms of timeline. The approach they took, however, was somewhat different than what we're going to propose here. They tried to determine the absolute physical limits on the ground motions, in other words, what are the biggest ground motions one could ever see, period. And, they discovered very quickly that this is a hard problem, and that is not necessarily amenable to that approach.

Based on experience the Swiss had, and our own considerations of the data we have available to work with at

Yucca Mountain, we decided to approach this as a more site specific problem, and approach it within a probabilistic framework.

There are a couple of background notes on this. Again, the ground motion amplitudes that we predict for very low probabilities are much larger than anything that's ever been observed worldwide anywhere. That, unfortunately, says that the existing observation database of ground motion reportings in probably not going to give us a very robust handle on the upper limit of the ground motion, that that's partly because rare events happen rarely. We've only been monitoring in this sense for about 30 years or 40 years.

The other thing is is we're going to focus on looking at peak ground velocity as our ground motion measure of interest here. And, the reason for that is is that's the ground motion metric that we use to scale our time histories and evaluate damage to the drifts and to the engineered barrier system.

And, we decided, as I said a moment ago, to evaluate these bounding ground motions on PGV using very site specific physically based arguments. And, in fact, that argument really centers around this, that the very intact nature of the tuffs at repository elevation and the delicate mineral deposits contained within those rocks suggests to us that no truly extreme ground motions have occurred at this site since the rocks were deposited 10 to 12 million years ago. They, in a sense, provide a very low resolution seismoscope that's been there for a very long time.

We choose to focus on a very site-specific approach here because of the fact that I think at Yucca Mountain we're very fortunate in that we have of course driven tunnels into the rocks we're interested in. We can go out and we can look at them, touch them. We've sampled them, taken to the lab and tested them. The geologists have gotten out here with their face right on the rock and mapped this in excruciating detail in some places, and that gives us a real decent dataset to go after this problem with.

The existing geological observations that we're going to try and leverage for this problem have been conducted at a variety of different scales here. A very small scale core and thin section really allows us to develop an understanding of really more of the secondary mineral deposits in the rock mass.

Of interest to us are the detailed line surveys and photo inventory in the ECRB and ESF. In particular, some of this data has allowed us to develop an inventory of the existing fractures and understand that the genesis of most of the fractures, which appears to be mostly related to proven phenomenon, and also look at the lithophysae, and I'll show why that is of importance to us in a moment here.

We're real interested in whether the lithophysae have been deformed, or whether there are lots of fractures around the lithophysae.

In addition to the geological observations, we've taken samples to the lab, and we've tested the samples. We're particularly interested in the large core samples like this, because we think they're the most representative of the behavior of the rock mass as a whole, because they have lots of lithophysae within the rock mass. We used some of these results to calibrate our micro-mechanical models.

An example of some of the stress strain curves where we're going to rely on here, this is an example from one of our large samples here in the lower lithophysal tuffs, and you can see that we define an approximate failure strain here of approximately .34 to .36 per cent. This is in the lithophysal tuff units.

However, this is for surface tested, any axially surface conditions, and we have to make an adjustment for the fact that at repository depth of approximately 250 meters, you have overburden stress to take account of, in other words, part way up this loading curve, so, you have to calculate the strain increments to get to the failure here. And, in this case, it turns out for this sample to be about .2 per cent strain.

This is a summary of some of the large sample data that has been corrected to this overburden depth of 250 meters, and you can see that our shear strain limits now are between about .09 per cent and about .34 per cent here, with the bulk of the data below .2 per cent.

So, we're going to focus now on the lithophysal units, and we're going to do that for the following. We feel that that is our most sensitive barometer of large strain in the system here, and that would be the first place we would see fracturing manifest itself, is within those units.

We're going to try and relate the geological observations and test data together by doing some modeling, and the modeling that I'm going to show here is from work done by Peter Cundalin (phonetic), who is associate to the ITASCA Corporation. And, their data, their modeling efforts originally calibrated to the large block test, the 288 millimeter blocks.

This is an example of some of the results that Peter and his associates got, and this is a 1 meter by 1 meter block here that they've exercised to failure, if you will, and you can see the fractures that develop within the sample here. Basically, the existence of the lithophysae, they act as stress concentration points. In almost all cases, the fractures move between these lithophysae, and you get this very diagnostic shear bending in here. This is for a random arrangement of lithophysae. And, we've also done the same sorts of tests using stencils from the mapping within the tunnels themselves of the lithophysae, and you can see exactly the same sort of behavior in all the tests.

We argue the fracturing of this magnitude would certainly be observable within the existing geological mapping. And, Steve Beason and Dave Buesch and their colleagues indicate that they feel very strongly that if fracturing of this type existed within those rocks, they would easily be able to identify it, would have in the previous mapping efforts.

We define a particular term here for this type of behavior. We refer to this as the onset of systematic fracturing, OSF.

A summary of the various test data corrected, data here, this is from work done by New England Research and Sandia Labs, I believe. But, anyway, the summary statistics here for the mean shear strain limit to produce OSF, if you will, ranges from about .13 to .2 per cent strain. You see the standard deviations are relatively small here.

So, based on the modeling results, the geologic mapping, and the fracture inventory and lithophysal inventory, we have defined a distribution on shear strain that's consistent with the onset of systematic fracturing here. We're modeling that as a truncated normal distribution, with a mean of .2 per cent strain, with a sigma of plus or minus .1 per cent, and the limits on that are .05 and .4 per cent strain.

So, once we've calculated or evaluated a limit on that shear strain threshold that would lead to obvious signatures within the rock mass, we can then go back and calculate what does that correspond to in terms of the ground motion, in this case, peak ground velocity. And, we do that by incorporating the uncertainty in the shear strength threshold itself, as well as we exercise our site response model here to try and incorporate the uncertainties in the density, module reduction and damping, and in the short velocity profile at the site.

So, what we end up with is a distribution on the mean bounding ground velocity, and that's the output of this particular exercise.

So, to summarize that, we're really basing this on one fundamental physical observation, and that is the absence of any geologic indicators of seismically-induced deformation within the repository rocks. And, the framework for that are the original geologic observations, and the laboratory testing, and the modeling. Based on that, we develop a distribution on the threshold shear strain, and once we have that, we do go back and calculate the ground motions that are consistent with those strains.

We feel multiple lines of supporting evidence that really add a basis to this case, and I'm going to spend just a couple moments talking about those in a second. And, again, we're offering this as a probability distribution on the bounds.

So, how is this actually used within the TSPA model now? Well, the way it's used is the following. We have our existing hazard for peak ground velocity, which as I indicated, is our ground motion metric of importance for sampling our seismic consequences, and putting that into the TSPA. It's working right now in the current runs of TSPA, that the TSPA, each realization goes in and samples the existing mean peak ground velocity hazard curve, and at the same time, it goes in and samples this distribution on peak ground velocity, and the distribution we're using on this bounded peak ground velocity is a uniform distribution between one and a half and 5 meters per second. That's consistent with those strain limits we described a moment ago, with that ugalcinon strain.

And, it compares, the TSPA then compares those two values, and if the peak ground velocity bounds, and uniform distribution is less than the PGV sampled from the existing hazard curve here, then it uses the smaller of the two values.

So, there's a little bit of supporting evidence I'd like to talk about just for a moment here. Recall a few moments ago, I described how if we assumed very large ground motions, like 10^{-7}, were real and tried to back calculate source parameters for those, we got really unrealistic values out of that. Well, if you do the same exercise with our range of peak ground velocities between one and half and 5 meters a second, you still get very large stress drops, but they're stress drops that we could maybe associate with plus 3 sigma kind of stress drops, which is entirely consistent with what we're trying to do here. Those are very remote, probabilistically very low probability that that would be the answer, but they are not beyond credibility. They are certainly credible estimates of what the stress drops might be, very large stress drops.

The second is looking a little bit at the question of shattered rocks. The supposition here of course is that large motions will in fact shatter the rocks. And, Jim Brune and some of his colleagues have been working on this for a while, and we think there's good evidence that that is, in fact, a good assumption.

And, some of the work that's been done in addition to just the strength of the rocks, there are existing fractures with secondary mineralization within the repository, the tuff units, and the geologist feels strongly that they can document a lack of offset within those fractures based on that secondary mineralization since the formation of those minerals.

Also, some very delicate crystals I'll show an example of in a moment that seem to support at least qualitatively the lack of any extreme shaking at this site.

This is a slide from Jim Brune at the University of Nevada, Reno. And, Jim has been working in California for a number of years here trying to look at investigating the occurrence of shattered rocks, and he has found some really interesting evidence. He only sees the shattered rocks in a very few places, and those places are on the hanging wall of thrust faults, where we have fairly competent materials. And, you can see that these rocks are fractured at virtually every length scale possible, and if you just go off the slide this way a few hundred meters, and across the fault tip, on the footwall rocks, you don't see any of the same sort of behavior at all. You see relatively competent materials.

And, this, observationally, see this in only these places, and theoretically, we can show, you know, in our ground motion modeling calculations why this is the case, that you have energy trapped in that wedge that leads to extraordinarily high ground motions. And, we don't see anything like this anywhere in the basin and range. It's certainly not in the extensional kind of terrain we have at Yucca Mountain site.

Compare that type of behavior in the rocks with what we see within, this is a panel map, within one of the lithophysal units here. You can see where you have lots of lithophysae, but essentially totally unfractured rocks. Now, keep in mind that, you know, for the probabilistic perspective, that these rocks are 10 to 12 million years old, and based on the slip rate and proximity of the Solitario Canyon, Paintbrush Canyon Fault systems, these rocks have seen somewhere between maybe 100 characteristic type maximum earthquakes on the Solitario Canyon, perhaps as many as 50 on the Paintbrush Canyon system. These rocks have experienced that many ground shaking episodes. Certainly earthquakes do happen in the Yucca Mountain area. These rocks have not recorded any signature now of having sampled, if you will, maybe as many as a 150 characteristic events, no extreme motions seem to have manifested themselves here.

And, this is the last slide. This is actually from some work that Joe Whalen and his colleagues at the Geological Survey have been doing. This is a photo of some very delicate textures that you find sometimes within the lithophysae. These are crystals, very slender bladed crystals with top-heavy overgrowths. We haven't really worked on this in a quantitative sense yet, but we have certainly argued that within a qualitative sense, these structures at least are suggestive of no extreme ground motions, at least in acceleration space, at this site in a long time.

So, to sum up, I'd like to reiterate that we feel that the existing PSHA provides a very solid basic framework for development of the ground motions at this site, and we're currently trying to develop what I refer to here as strength-limited peak ground velocity, site-specific strength limited peak ground velocities to ensure that the ground motions that we use in our structural response calculations, performance assessment, are consistent with the observational evidence of what we see at the site, and specifically that's a lack of geological deformation within the rocks at the emplacement level.

We're continuing to work on various testing and modeling studies to try and refine some of this initial assessment here. I must point out that this issue is still being worked on. We have a goal of completing this in much more detail within the next 18 to 24 months.

And, what I say here is that we are currently completing an analysis report, the document where we are right now, with regards to just the peak ground velocity only. That's the only parameter we're investigating at this time.

And, with that, I guess I'd like to--

DUQUETTE: Thank you very much, John. Thanks for being right exactly on time. You obviously don't teach at a university. Question, Richard Parizek?

PARIZEK: Yeah, Parizek, Board.

You don't mention anything about precarious rocks, which Jim does, in the work we have, and it seems to me that's at the surface of the ground, and again, not knowing how long the rocks have been exposed in that condition, a delicate condition, it seems to me that's a very direct evidence. In your example, we have to kind of go along with all this rock mechanic stuff at depth, and wondering, gosh, I wonder, and so you go to some very active other fault that's perhaps a bigger fault area, or active ground motion area. Has that been tested some other place where you could go, San Andreas or some other place, and say look, the rocks do crack up.

AKE: Well, Jim has been working on this for quite a long time. I had a bullet in there about precarious rocks, and chose not to really speak about it right now. The precarious rocks I think speak perhaps more to our over-estimation of the aleatory variability in our ground motion estimates. In other words, for a single, if you're trying to predict what the ground motions are going to be for a single occurrence, what is the standard deviation read for that uncertainty in a particular event, and Jim and John Anderson have written some very interesting papers with regard to that.

In terms of helping us within the probability framework we're interested here, which is 10^{-6} and below, for performance confirmation right now, the precarious rocks really don't help us that much, because they only record a much shorter period of geologic time.

With regards to the second portion of your question there, Jim has been working on looking at rocks adjacent to the San Andreas with precisely the sort of arguments we're talking about here, which is if you assume very large ground motions next to a major fault like the San Andreas, capable of producing extremely large magnitude earthquakes, and you don't see highly fractured rocks, what does that tell you about what the maximum ground motions can be. And, he's real interested in that question of aleatory variability and what the maximum ground motions are. And, I think he's onto a very fruitful line of inquiry with that one, because he has rocks there that he can document have seen probably many hundreds of magnitude 8 earthquakes. And, that's a sample, is the sparses, the data, and the seismic realm, is usually a heck of a sample to look at. So, I think he's got some real good ideas there.

PARIZEK: Parizek, Board.

I did point out to Joe Whalen Figure 27. These are perfect pendulums with the bulbous tops, and if you were going to try and get some sense of ground motions that would take to topple those, they're in the lithophysal cavities, and what a fantastic place to look. So, I guess no one has tried to topple one of those?

AKE: Well, at this point, we haven't actually finalized what we're going to do to carry on with this in the next stage here. We think this is real important to try and come up with with more physically realistic low probability motions. So, we've sort of danced around with the appropriate way to go forward with this. The clearly lab testing, or something like that, of these types of samples would be something that would be quite useful to undertake.

PARIZEK: Thank you.

NELSON: Nelson, Board.

Can you just summarize for me, I note that you've got some typical shear wave velocity--excuse me--shear modulus and damping flux on 34 in the appendix. Can you talk to me just a little bit about what you're doing regarding the strain rate?

AKE: Yes, that's a very good question. Essentially, this data here is for very high strain rate. Okay? The dynamic cyclic tests like this are done at very high strain rates. This data was worked up by Ken Stokoe at UT Austin. Most of the data you see plotted here is actually for tuff samples that underlie the proposed surface facilities area. There's only six, I believe, six data points in here that are from the tuff units that are in the proposed repository elevation.

Ken's apparatus here is limited to only go to a tenth of a percent strain. I should point out two of the six samples of the tuffs from the repository horizon were not from lithophysal units because they are small samples, actually failed prior to a tenth of a percent strain, which is consistent with the estimate we're coming up with.

The other large sample block samples that we looked at were basically very low strain rate. So, we really had these two end numbers in terms of strain rate right now, but they tend to, based on a very limited sample here of the tuffs in the repository horizon, they tend to predict kind of consistent results. We feel comfortable that the low strain rate results are usable because of the fact that the wave lengths of these incoming incident waves, at least in terms of peak ground velocity, are long and that the strain rate there is probably somewhere between those two extremes. It's a problem, and it's a problem endemic to testing in the earthquake engineering field, because nobody has the apparatus to test at the strain rates you really want.

NELSON: Nelson, Board.

And, I appreciate all of your efforts here, because I think it's important that the science and engineering weigh in on this, and, so, I encourage you to get this all into print as soon as possible.

But, the issue of the strain rate, and also, I think you've tried to capture some of the influence of existing porosity, not matrix porosity, and not just lithophysaes, but, you've got cooling cracks, you've got other porosities in the rock mass, which can actually affect damping in a way that's not captured here.

AKE: Right.

NELSON: So, I think that you might be able to bound some of those effects rationally, and that would be interesting, and I think the overall profession needs that input of your thinking through this. It will really help us overall. So, thanks.

AKE: Okay, thank you.

BULLEN: Bullen, Board.

Just a quick question on Slide 22. I'm interested in how this is going to be incorporated into TSPA. And, as I look at this slide, it's the same slide that you showed us in 5.

AKE: Yes.

BULLEN: Would you expect there to be a peak ground velocity cut-off, or something to that effect? And, how would you see that as incorporated into the TSPA?

AKE: Well, that's a good question, and that's a hard question. As it's being incorporated right now in the current runs of TSPA, this is the existing hazard curve for mean peak ground velocity. Each realization, samples from that and compares it to a value that simultaneously samples from bounded peak ground velocity distribution, which is a uniform between one and a half and 5 meters a second, and compares it to and uses the minimum of the two. So, it doesn't affect anything up here in high probability space, but it does begin to affect you in this range down through here.

And, effectively, what this is doing, more or less, is putting a fuzzy boundary on this and causing this to, instead of becoming asymptotic like this, begin to have more of an asymptote to the Y axis, which is precisely what you would expect.

Ultimately, the final implementation of this may be somewhat different. We have discussed with Allin Cornell the possibility of maybe doing this as a Bayesian update problem, where you regard this as your prior, and the likelihood function you apply to that is in fact on the ground motion value, which then, your posterior then will be a modified hazard curve. So, we're still working through that, the proper implementation of that ultimately.

BULLEN: Thank you.

REITER: Leon Reiter, Staff. Just a few questions on this last thing.

What I'm interpreting is that those strain limits that you've talked about correspond to one and a half to 5 meters.

AKE: Yes, per second, yes.

REITER: The question I think we've talked about before is that you addressed the PSHA, but there's still another problem which can cause a very large ground motion, is the way you do the time histories and get some very large things, and you haven't decided how to deal with that.

AKE: Well, obviously, at some level down the road here, we will have to address that by essentially recalculating all the hazard curves, not just PGV, and developing new time histories that are consistent with the observed strengths of the materials.

REITER: But, for LA, you have to do that?

AKE: No, I may wish to defer to Bob about that, but we have time histories that were developed for 10^{-7}, 10^{-6}, 10^{-5}, and I think one for 10^{-4} that were used in the TSPA--excuse me--were used in the development of the seismic consequences. And we'll probably use those. Really, what's happening is you're also de facto altering the probability of those by changing this.

REITER: Is there you said a ground breaking, these studies are really on the cutting edge, and to more recognize that in this letter, and the Board recommended, that this would be a good thing to subject to external peer review. Do you have any plans to do that?

AKE: Yes. Right now, as I stand here today, we're still not 100 per cent sure what direction we're going to go over the next 18 to 24 months, but it's likely we will have a review board involved in this. Our thinking right now is more of a participatory peer review. We'll have a small board that will help us through this. We'll have our own experts that we will utilize, as well as project staff, and likely have an oversight board that we'll meet with frequently, rather than get all the way to the end, present the results, and hope that they think it's okay. We would like to have them participate in the process.

REITER: One final question. Because you're limiting yourself to what's--you're putting the limits on the ground motion based on what's been observed in the mountain for the past 10 million years, how do you account for the argument then that what's 10 million years, for example, is not a good enough time period to put limits on the 10^{-7} current, which is one in 10 million years?

AKE: It does not permit you to put an absolute bound on that.

REITER: But, you are.

AKE: In a sense, the way we're doing this right now, it is, but if you go through, it's informative to go through the, operationally, to go through the Bayesian update, because what you do then is you apply that as an observation that you have in 10^{-7} years, that you have no observations greater than this in 10^{-7} years. It does not imply that a value greater than that is absolutely impossible. It only says that I have 10^{-7} years of observation, and have not seen anything greater than that. And, when you apply that as a constraint, you find that the hazard curves drop like a rock.

REITER: I guess I'm going to see those curves with an explanation.

AKE: Yes. Well, and that's what we hope to bring forward.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

LATANISION: We will be moving from discussing broad issues associated with the project to a very specific issue, namely corrosion during a thermal pulse. This is a very important topic, which as Dave Duquette mentioned earlier this morning, will essentially occupy not only the rest of this afternoon, but all day tomorrow.

I'm Ron Latanision. I chair the Board's Panel on the Engineered System, and I will lead off this afternoon's conversation.

Last October, the Board issued a very focused letter about corrosion during the thermal pulse. It was an unusual letter in a couple of ways. First, the Board was unusually direct. The letter stated in no uncertain terms that the Board had serious concerns about corrosion of the waste package during the thermal pulse, and that the concern was based on data in hand. That last comment is very important.

We didn't say that there was uncertainty about whether there would be corrosion. We said that the data in hand, and this data is mostly from the project, but also from the Center in San Antonio, and the literature, indicated that corrosion is likely.

The other unusual aspect of the letter is that it was signed by all the Board members, not just the Chairman, which is our normal practice. There was a reason for this, although all Board letters and reports have always been consensus documents, we wanted to be sure that there would be no misunderstanding about all of the Board members' positions, any of the Board members' positions, with regard to the statements in the letter.

A month after the letter was issued, we issued a detailed report, giving our technical basis for the letter, and touching on some of the related issue. Also in that report, we acknowledged that the Department of Energy did not believe there would be a significant corrosion problem during the thermal pulse, and we stated why we felt the DOE's technical basis for believing that corrosion during the thermal pulse would not be an issue was not adequate.

Again, every Board member signed the letter transmitting the report individually for the same reason as in the case of the October letter.

Once again, an unusual aspect of the report was that one member appended additional technical comments to the report. No one can remember that ever being done before.

Now, that brings us to the purpose of today's meeting. Since we wrote the letter and the report to the

Department, we are particularly interested in the project's view, the DOE's views on statements in the letter and the report, as evidenced by new data and analyses. By new, I mean not previously presented to the Board.

We know that the State of Nevada, the NRC, and the Electric Power Research Institute are also very interested in the topic of corrosion during the thermal pulse. The purpose of this meeting is to provide an opportunity for the project and others to present relevant data, and analyses, and to engage in an open and thorough discussion of the issues.

My goal as Chairman of this Panel is to give the Department and the project the opportunity to have a full and objective hearing on the issues that concerned us. And, in that context, we're going to change the format of our discussions a little bit, in the sense that after each of the Panels present their discussions, and we will hear first from the NRC, during that period of question, that's identified as question and answers, we will invite questions not only from the Board and the Staff, but from the audience. This is a departure from our normal practice. If we have an overwhelming response from the audience, obviously, we're going to have to limit the number of questions that may be entertained, but we do want to open this up. We want as open and full a discussion as is possible.

So, the program for the rest of the meeting today and tomorrow is simple. The Department has all day tomorrow. They asked for that much time and they also asked that their presentation come last. Frankly, this is not the way I would have preferred it, particularly since we're really addressing expressions of concern on the part of the Board to the project, but nevertheless, that's the way we will conduct the discussions.

The Board, the NRC, the State and EPRI will all have opportunities to speak. The latter three will speak today.

We're going to start by presenting what we said in our October letter, and our November report. We don't know what the NRC, the State, or EPRI, or the project will say today or tomorrow. The only ground rules that we've established were to try to keep the presentations related to the topic of the meeting, that is, corrosion during the thermal pulse, and to emphasize new information, and to discuss relevant experimental and analytical work done in the past year, or planned for the future. I know that all of those organizations have been working very deliberately at this, so I'm confident that we'll have meaningful conversations.

I just want to make an observation in terms of the presentations by the State of Nevada today. I'm sure you all know that last week, the State held a press conference here in Washington, in which they presented corrosion demonstration. It is, therefore, a reasonable question to ask whether the State has been showing, and also doing, at Catholic University, whether that work is the same as the Board's main issue, which is the deliquescence induced localized corrosion. And, the short answer is that they are not the same issues.

We are, as mentioned, concerned about deliquescence induced localized corrosion. From what we've heard and seen, the State's corrosion issue is really quite different, and has to do with the, essentially, the pore water evaporation, or concentration of pore water, and the production of acids by various means, which are known, or shown to be corrosive to Alloy 22. So, they really are two different issues, and we want to be clear that they are not the same issue.

My sense of the distinctions I'm sure will become clarified as the presenters from those organizations have the floor from that organization.

I have asked two of my colleagues, Thure Cerling, to speak about his views on the environment that might be generated during the thermal pulse, and Dave Duquette to talk about his views, or the Board's views, as manifest in our documents, our reports and letter, following my short introduction.

I'm going to try to cut some time because I know this is getting long. A month ago, after the letter was issued, we drafted a report. Let's show the next slide. I'm going to skip some material here. Sorry. You really have heard this, so I'm not going to spend much time telling you what the Board does. I do want to tell you what the Board does not do.

The Board does not make or enforce regulations. We don't advise the NRC or EPA or Department of Transportation or anyone else, except the DOE and Congress. We don't make policy. The Board does not do experiments or design work. What we attempt to do is to objectively evaluate the Department of Energy's work by analyzing their data and work products and other relevant studies. And, that's exactly what we did last fall when we wrote the letter and report that was delivered to Margaret and to the Department of Energy.

Let's look at the next slide. Over the past 14 years, the Board has spoken and written frequently about issues and problems associated with uncertainties during the thermal pulse. The letter we wrote last fall, and the report, presented data that the Board had seen from presentations given before us at earlier meetings by the Department and by the Center in San Antonio. We wrote these reports because it appeared to us, based on the data presented, and I would lead you specifically to the January and May 2003 meetings, that all of the conditions necessary to initiate crevice corrosion on Alloy 22 would be present at the same time for significant periods during the thermal pulse. These conditions are identified on this slide. They include corrosive brines containing chlorides, high temperatures, and project data showing that crevice corrosion initiation under such conditions would be likely.

It is well known that certain oxyanions, such as nitrates in particular, inhibit initiation of localized corrosion. However project data presented to us indicated that the effect is diminished, or may not exist at the highest end of the temperature spectrum where corrosive brines might be expected to exist.

Compounding this situation, were data from the project and from CNWRA showing, not unsurprisingly, that greater susceptibility to localized corrosion occurred in the case of welded or aged Alloy 22 structures.

I want to close my comments by addressing a particular sentence that appeared in our October letter. And, that sentence read, "The Board believes that Total System Performance Assessment, TSPA, should not be used to dismiss these corrosion concerns." I think the sentence is clear enough, but it has been something of a mystery to some people, because we haven't explained why we said it.

What I'd like to do and what's shown on this figure are at least some of the reasons why we chose to make that comment. I would like to just go through these very quickly. First of all, it is more difficult to achieve fundamental understanding of the repository system at high temperatures. Using TSPA to dismiss concerns about crevice corrosion is primarily an approach that focuses on regulatory compliance. Of course, compliance is absolutely necessary.

The Board has stated, however, that there is a growing international concern that fundamental understanding of the repository system is as important as showing compliance. And, above boiling repository that, among others, introduces concern about crevice corrosion is much more difficult to understand that a below boiling repository.

DOE's TSPA Peer Review Panel put it very well back in 1997 when they stated, and I quote, "For a repository to be licensable, it must be analyzable." The Panel specifically raised issues about the analyzability of the response of the systems to the thermal pulse. We feel, the Board feels, that a below boiling repository is much more analyzable than an above boiling one where thermally coupled processes are more of a concern.

Second issue. Don't compromise an important barrier. NRC's regulation for Yucca Mountain, which is 10 CFR 63, may be mostly based on performance assessment, but not exclusively so. It is based also on principles of defense in depth that permeate most, if not all, NRC regulations, and it has a requirement for multiple barriers, in particular, that there be both natural and engineered barriers.

The Alloy 22 of the waste package is very important, if not the most important component of the engineered barrier. It seems to us sensible that one would not want many defects or penetrations in such an important component, particularly if there appears to be an easy way by which they could be avoided. The latter being, we believe, a low temperature design.

Thirdly, it makes better engineering sense from our perspective to avoid the problem through a design decision than to attempt to accurately quantify it. When dealing with uncertainty inherent in natural systems, for example, such as volcanic eruptions, or transport through the unsaturated to saturated zones, the only recourse is to collect data, generate the best models available, and attempt to reflect both the parameter and model uncertainty in the calculations.

In the case of localized corrosion, the Department is faced with a problem largely caused, largely having its origin, in a design decision, to have an above boiling repository. Localized corrosion processes are particularly insidious form of corrosion because of the details of the initiation and the difficulty in predicting the propagation rates, which can be extremely rapid.

It seems to us to make better engineering sense to avoid localized corrosion altogether by design decision rather than to rely upon one's ability to accurately model and quantify what will happen or limit the consequences.

Fourthly, uncertainty in estimating the consequences of crevice corrosion is an important issue. If the data which has been presented to the Board indicates that crevice corrosion is likely to occur during the thermal pulse, then there is still much uncertainty with respect to determining its consequences.

Making bounding arguments with a reasonable degree of certainty would obviously be very difficult, and, so, we're concerned about studies which use different assumptions, or using the TSPA now under consideration for development of a licensing application which may show different results.

Dose rate is also an important assumption, other than waste package integrity, and, so, we're obviously very concerned about how all those parameters play out.

Finally, the safety case based on multiple lines of evidence is something that we find very important. TSPA is a very powerful tool, but it is only as good as the abstractions, the assumptions, and the data upon which it is based. These limitations are often obscured by the inherent complexity found in large performance assessments, such as those conducted at the site. Frankly, our sense is that TSPA is so complex that it ought not to be relied on exclusively. Multiple lines of evidence derived independently of TSPA should be considered as well, and this is the fundamental idea behind providing a robust safety system.

In summary, regardless of whether TSPA shows that compliance can be achieved, the potential for corrosion during the thermal pulse is a serious issue, because it reduces defense-in-depth, compromises a major barrier, and reduces the safety margin, thereby undermining confidence. That's essentially the expression of concern that we have presented in our letter and our report.

Now, there are going to be two other Board members following me in making presentations. What we'd like to do is defer questions or comments until all three of us have spoken. I have spoken, and so I will next turn to my colleague, Thure Cerling, who will talk about environments that form on waste package surfaces during the thermal pulse.

Thure?

CERLING: I'm Thure Cerling, a member of the Board, and when I'm not doing Board work, I'm at the University of Utah, where I'm a Professor of Geology and Geophysics, and also Biology. And, my interest and expertise is in the field of terrestrial geochemistry.

What I'm going to do just in the next ten or fifteen minutes, in part to get everybody at the same place, is just to describe some of the things that the Board said about the environments on the waste package surfaces, in particular, those during the thermal pulse.

Next slide? Do I have that? I'm going to mention a few things in my talk today. One will be talking about the temperature on the waste package surface, and I would like to point out at this point that all of the data that I'll be showing in any other material is basically from previous presentations by DOE, so there's really actually nothing new that's in this presentation. We're simply restating what we understand to be the model that is being used by DOE.

So, first of all, the temperature that we're talking about when we're talking about temperature in our report is the temperature on the waste package surface. Okay? There are other temperatures in the repository, but in particular, in the slides that we'll be looking at, this is the temperature on the waste package surface, not the highest temperature in the repository, it's not the lowest temperature in the repository, but sort of a generic surface temperature.

The relative humidity that's shown on these slides is the relative humidity for that generic sort of temperature on the waste package surface. And the temperature and humidity are closely linked.

The next very important issue is the dust that settles on waste package surfaces. There certainly will be some sort of dust. We know there's dust in the tunnels. And, then, the important aspect that follows on from that is the property of deliquescence.

Along with that, there's some uncertainties in the in-drift environment that DOE still needs to consider, but I'll try to go over all of these things today, and we'll just kind of wrap up with not so much of an environmental research recommendation, but research issues that clearly DOE has been following on in preparation for this.

Next slide? Okay, last year, DOE presented in sort of a poster format, an illustration describing the evolution of environment at Yucca Mountain, and what's important is sort of this purple band here that is sort of the time on X axis temperature history for the waste package surfaces. And, the details of this purple band are shown in the next slide, which is a similar sort of slide, but just shows more detail, and where some of the what might be perceived as fuzziness came from. These represent different modeling runs for different specific waste packages, and so on, and this one cuts off after about 20,000 years.

The concern that we have today is really mostly with this short period where we go from relatively low temperatures, below boiling, to above boiling, and then we decline, so this higher area is what we refer to as the thermal pulse.

There's just a few things that I do want to comment about the thermal pulse issues. And, some of those have to do with uncertainties in the thermal pulse calculations, and these have to do with several different things. One of these aspects is thermal conductivity, and one of the things that we think the DOE should consider is that the thermal conductivity that's used in their calculations, it's possibly that it may be too high, and specifically that the thermal conductivity in the lower lithophysal zone where most of the repository would be located would be high.

Some of their tests, field testing, lab testing, and statistical tests point to a lower value than is used, and if the thermal conductivity is too high, then the temperature estimates will, in fact, be too low. So, this is just an area that we feel should be considered.

Another very important aspect with the thermal calculations have to do with the drift degradation. If there's drift degradation during the thermal pulse, it will come perhaps in response to seismic events, to thermal stresses and other things, and Mark Peters showed us that they were doing some studies on thermal stress, and we don't know how widespread this effect would be, but the drift degradation could necessitate recalculation of some of these thermal history curves.

Another important aspect is the problem of both natural circulation and natural ventilation. Natural circulation is the phenomenon by which air circulates through the mountain, but doesn't exchange outside the mountain, and ventilation is where there's actually outside exchange of air with the mountain. These two properties will tend to have a cooling effect, and we're not really sure, we're not completely confident in all the calculations that these things which actually may be an under-estimate of temperature, and perhaps not a good enough consideration of this, may result in an over-estimate in temperature. We just feel that there's some uncertainty in the temperature and, therefore, relative humidity predictions that they have made. But, significantly, these two work in one direction, and this works in the opposite direction.

Next slide? Okay, this is a relative humidity diagram that, as you can see, is closely related to the inverse of the temperature diagram, and I'll actually be using the next slide, which is a similar diagram, which shows that the humidity goes from relatively low levels to very, very low levels during the thermal pulse. And, then, when the thermal pulse ends, it begins to go to higher and higher levels. And, these are the most up to date curves that we've been able to consider, and significantly, the lowest humidities that are encountered, between 15 and 25 per cent, are 70 to 80 years after the closure of the repository.

Okay, next slide? This is an important slide that's sort of the crux of what I think will be the discussions of the next few days, and that has to do with problems of deliquescence and then what follows on from that is corrosion issues. The important part of this figure are really these two curves over on the right side, so the X axis is temperature, the Y axis is relative humidity. And, what is plotted are the boiling points on the boiling water curve for these saturated solutions with these different salts, calcium chloride, calcium nitrate and on up the line to the univalent salts.

So, what we see in this slide is that there are some salts, in particular calcium nitrate and calcium chloride salts, that can, we believe, can deliquesce at very, very low humidity, and there are some that deliquesce at much, much higher humidities.

One of the significant things about this curve is that it doesn't show any binary or ternary eutectic points, because we know that the mixture between two different salts, just taking as an example sodium nitrate and calcium nitrate, or sodium chloride and sodium nitrate, or any two on their, the eutectic deliquescence point for most salts, actually, is lower than for either of the end members. So, one of the things that's been lacking in the discussion so far has been a discussion of what their deliquescence point may be of these mixtures.

So, we know that deliquescence is possible from this data of these various different salts, and as we'll get to in a bit, what we don't know is really what the deliquescence is of the salts that are likely to be actually in Yucca Mountain.

Next slide? So, this is some data from Lawrence Livermore's Lab, thermogravimetric data, and this is data from a one-half inch by two inch by a sixteenth inch coupon of two different alloys. The important one is Alloy C-22, the redline, the other one is some other alloy, which were coated with salt, in this case, calcium chloride, and this is done on a sensitive balance, and the humidity is broad, up to the point where deliquescence occurs, which, in this case, is about 22 1/2 per cent. And, the crux of the matter is that we begin our experiment at time zero, and what we see is change in weight immediately, and there's an increase in weight, and this increase in weight is due to the absorption of water. So, this is showing that deliquescence does occur.

And, then, what we see is that, again at these high temperatures, 150 degrees C temperature, we see that there's actually a weight loss, and the weight loss is thought to be due to the formation of hydrogen chloride gas, hydrochloric acid, and the formation of probably a calcium oxygen chloride compound.

So, this is just an example of one of the possible salts, and I think we'll hear a discussion on what other salts may be present in Yucca Mountain. But, this shows that we do get deliquescence, and then there's some other chemical reactions going on at these high temperatures and these low humidities. And, in this particular example, we note that there's no evidence of corrosion of Alloy 22 in this deliquescent experiment. And, on the other hand, this other material, which was also studied at the same time, in fact did show some corrosive behavior.

Next slide? This is I think one very important next part of the puzzle, and that is going to be the composition of the dust. And, the way that we can possibly get deliquescence forming is if we have dust deposited on the waste package surface before closure, or even after closure. And, we note that there's at least a 50 year period that desert air will be circulating through to the system through heat by the packages in place in the repository. One significant thing might be to consider whether or not this air is filtered or unfiltered air.

So, what is the source of the dust? There's at least two sources of dust. One is dust that will result from decrepitation from the drift walls, and it will be circulated either by the ventilation air or even after the system is closed, by air currents just produced by differences--due to temperature differences in pK heat.

So, one of the sources is going to certainly be a local source within the mountain, and that's one of the things that we have only a little bit of data on so far. The other source of dust could possibly be brought in from the outside. And, so, one of the things that has been shown is that the dust that is present in the mountain certainly has all of these components present, chloride, which is of great concern in corrosion, nitrate, which is also important, especially as in certain temperatures, it has a mitigating effect, and magnesium and calcium chloride, which are the salts, which have the lowest humidity deliquescence point.

Okay, the other important thing in this comment is there's a lot of silicate material and poorly soluble material as well, and this makes up in a very important and perhaps a very reactive component of the dust.

And, just one other comment that has been brought into this many, many times, and so I'll make sure that it's presented here, is that a few years ago, the Livermore Researchers in the paper by Rosenberg, et al, used a synthesized pore water, which was evaporated down, and in that synthesized pore water, they observed tachyhydrite, which is a highly deliquescent calcium magnesium chloride.

An important thing to notice about that experiment was when the evaporation experiment was done in the presence of volcanic ash, this particular mineral was not observed. So, it was only in the basically a silicate-free environment where that was observed.

So, one of the points that we think is important here, which hasn't been completely addressed at this point is that the sources of calcium and magnesium and other chlorides spilling from the desert environment have to be evaluated. We know that some of those are present in places like Bristol Lake in the Mojave Desert, which is not far from Yucca Mountain, and there are other playa deposits as well.

Okay, I guess just following on that, we also note that there's been a lot of work recently by Meredith Reheis and John Isbecky (phonetic) on collecting dust in the southwestern U.S., and that will have a very, or could have an important contribution to this study.

Most of the dusts actually have only between about 1 and 10 per cent soluble minerals. Most of them are these insoluble materials, and virtually all of them contain chloride.

Okay, next slide? Okay, one of the things that we felt is that where we were last year, is that at that time, there was insufficient technical basis for DOE's claim that there would be no corrosion. And, our reasons for that were based on published and presented materials, and this list gives some of our reasons for having the hesitancy we had in embracing those results.

One is that the brines tested so far may not be representing or bounding brines that would exist in the repository, and this can go in both ways. The brines tested tend to be almost pure calcium chloride, not binary mixtures for better or for worse that would be actually found to exist. The experiments to date were run only over a fairly narrow part of the temperature and relative humidity range, over which deliquescence can occur. And, I think Dave Duquette will discuss some of that. The experimental systems were done essentially as open systems, and one of the questions that we have is completely open system behavior really the appropriate way to model this, or is a more closed system behavior sometimes more appropriate to model some of these aspects of short-term repository behavior.

Another serious concern that we had, again, which will be the focus of some of the talk that David will be giving, is that some of the samples that were used in these experiments didn't have crevices, and to test the conditions for crevice corrosion, it's useful to have crevices. And, then, it appeared that there was some contradictory results between the corrosion experiments, in particular for the electrochemical methods experiments didn't seem to give the same results as some of the thermogravimetric data.

Okay, next slide? There's still the problem with nitrate that is still an unresolved issue. DOE has not established that nitrate would actually inhibit localized corrosion over the entire range of temperatures over which the brines could exist, and the concern is that as you go to higher and higher temperatures, perhaps this inhibiting aspect of nitrate may disappear.

Another important issue is are there natural processes that could separate nitrate in chloride during the behavior of the repository. And, another thing that we are concerned and just would like to have addressed is that the effect of microbes on the nitrates has not really been completely demonstrates. Will microbes actually have an effect over time?

Okay, next slide? I'm just going to wrap up a couple of things here, and just mention that there are just several things that still seem to be left to be not completely resolved, in our view. One is the issue of a capillary barrier. We realize that a capillary barrier in certain environments certainly can occur, but are concerned that some of the aspects of drift degradation and so on, and rock bolts, may actually cause a disruption of the capillary barrier.

Another issue is is there a potential for refluxing of fluids, and in the refluxing, change the chemistry in a way that is deleterious to the waste package. Drift collapse is an issue that we consider still to be a problem. And, then, the other problem, of course, is vaporization barrier, and the vaporization barrier is, of course, only as good as there is in fact a vapor, and this just has to do with the temperature and the chemistry of the final salt solution that's in equilibrium with the environment.

Next slide? I was just briefly going to mention, for the sake of completeness, some technical comments. These were made by Mike Corradini, who was on the Board when we submitted the letter, but has since resigned, and he just, there were three issues that he brought up in his comments. One was that perhaps that DOE actually over-estimated the relative humidity during the thermal pulse by not completely taking into account circulation and mass transport.

Secondly, he also believed that the deliquescence issue actually by DOE may have been over-estimated, because that the waste package surfaces will be hotter than the surrounding air. And, he suggested that deliquescence experiments should actually be undertaken using a heated surface. And, lastly, he made some discussions about some diffusion transport, which is really outside the scope of the Board's report.

Next slide? So, lastly, I think what we'll be hearing in the next day and a half is some interesting results that may not change the temperature estimates, but certainly the temperature estimates have a direct bearing on the relative humidity, and significantly, we hope that we'll hear something about dust composition and how that dust composition will play into the role of deliquescence, which then plays into the role of corrosion, which is where I will hand the baton over to David Duquette.

DUQUETTE: I'm afraid the Board is guilty of violating its own time slots in this particular case. That must be Mark's problem.

What I'd like to do is just summarize a few of the concerns the Board has had. This is just to wrap this up. As I indicated this morning, much of what I'm going to present--well, all of what I'm going to present is already on the Board's website relative to the letter we had presented to the Department of Energy with respect to the localized corrosion problem.

The Board feels that based on the data that has been presented by the Department of Energy so far, that all of the conditions that are required for localized corrosion can occur. And, if we take a look at the next slide, I'm going to talk a little bit about that issue, localized corrosion, an issue we don't know very much about at this point, that is, generalized corrosion, some of the implications of what our letter indicated, and some things we might like to see addressed in the very near future, although it's not our position to tell DOE what to do or what not to do, but simply indicate what some of our concerns are.

I we take a look at the next slide, there are several different kinds of localized corrosion that can occur. The one we're mostly concerned with is crevice corrosion, and the repository gives us an interesting set of conditions. Normally people worry about crevice corrosion because of mechanical crevices if you can think of a washer on a surface. One of the things I've mentioned to several people is when you fly home, take a look at the rivets on the airplane, and there is a very nice crevice, the crevice between the head of the rivet and the area on the wing itself, and that's corroding we speak, and there have been some serious problems with aluminum alloys because of that. So, most of them are mechanical in nature.

In this particular case, the dust itself not only sets up the crevice, that is, a place where you have an occluded cell, if you will, with some limitation of environment to the area under the dust, but it also gives you the chemical environment. Normally, the chemical environment comes from an external environment. Again, for those of you flying home anywhere near an ocean, that's basically salt water that you're concerned with. In this particular case, the environment sets up its own environment.

So, it's rather insidious because, again, with the rivets, sometimes you'll see a little black ring around them, and you'll know that the plane actually has crevice corrosion problems. Sometimes you won't because it's very difficult to see. So, we consider it to be insidious because it's very difficult to determine.

When you put a piece of meter in a corrosive environment, it arrives at a steady state potential, that is, that's based on the oxidizing capability of the environment that it's in. That's the corrosion potential that you're interested in. For most metals, there is also a critical potential, or a potential at which crevice corrosion, once initiated, will propagate, or if it hasn't initiated, can initiate. We're calling that right at the moment a critical potential. If the critical potential is an oxidizing potential that's quite far removed from the corrosion potential, crevice corrosion becomes not a problem, because you don't reach that critical potential.

Two things happen as you increase the temperature. One of those is that typically, the corrosion potential moves in a noble or up direction, and the critical potential moves down in the active direction. If they meet or cross over, then you have the possibility for crevice corrosion, and that's what our concern is, based on some of the data that's been presented to us. So, what we're really looking at is the difference between this open circuit or corrosion potential, and the critical potential to either initiate or propagate a crevice due to corrosion processes.

And, the next slide shows the data that was presented to us I think last January, based on the difference between that potential difference, and again, this is DOE data, this was generated in calcium chloride brines, this particular data has some nitrate added, I think it's about 10 per cent, but it doesn't really make much difference. I'll show you that in just a minute. This bounding region that you see here is the surface temperature of the canisters, or the containers. And, what you notice is this curve comes down and goes through zero right in this region that's bounded in red, and that bounding was done again by the Department of Energy.

So, now, we're looking at a situation, we have a surface temperature at which the difference between the open circuit potential and the critical potential for crevice corrosion falls into this zero region. And, I also would like to point out that there's a lot of scatter in this particular area right here. So, in this particular solution, you would expect crevice corrosion to occur and to propagate once it initiated.

If you take the nitrate out, which is shown in the next slide, that moves that curve somewhat to the left. You'll notice that the intersection before occurred here about 150. This curve moves over by about 10 degrees, and that simply indicates if I take the nitrate out of the solution, the propensity for crevice corrosion and crevice corrosion propagation increase. It's somewhat unknown, as far as I can tell, exactly what the nitrate, the chloride concentrations are in the repository. And, there's also the possibility that was brought up at our meeting in Las Vegas recently that nitrate might be consumed by microbes or other species in the environment. So, there is some concern as to whether nitrate will be important or not.

There are still other considerations that can change that crevice corrosion tendency in these particular materials, and the next data, which was presented by San Antonio Group, simply points out what happens if I have metallurgical effects that happen. These were done on alloys where either the alloy was aged, that would mean something that would occur adjacent to a weld, for example, where it sees a high temperature for some period of time, or if the alloy was welded.

There's a lot of data in this particular curve, but what I'd like you to take a look at are these solid blocks right here. These are the temperatures in which the tests were performed in chloride environments. So, these tests were performed at 60 degrees celsius for an aged sample, and these are these green dots right here. What you notice is as the chloride concentration increases as it becomes saturated, if you will, that the repassivation potential which for all practical purposes is the critical potential for crevice corrosion growth, drops down quite dramatically over several hundred millivolts as you increase the chloride concentration at 60 degrees.

If you increase the temperature to 80 degrees, you'll notice that that curve drops still more. And, so, the crevice corrosion potential increases, the potential doesn't increase, but the potential for crevice corrosion increases. If you increase the temperature to 95 degrees for that same sample, you'll notice that this curve moves still further down, approaching quite low numbers for repassivation potentials.

If you look at welded samples, this is a welded sample at 60 degrees, and this is a welded sample at 95 degrees, what you see is that also moves this in this direction. So, almost anything you do to the alloy increases the possibility for crevice corrosion in chloride environments, even at temperatures as low as 60 or 95 degrees, although we don't think this is a problem at the present time, based on the data that has been presented so far.

The other problem with crevice corrosion, and something we know very little about at the present time, is the data show a tendency for the initiation of crevice corrosion. So far, as far as I know in the environments that are expected to be seen in the repository, and certainly underneath dust particles, no one has done any quantitative measurements of crevice corrosion propagation, how rapidly it will propagate.

I might point out some numbers to you. In DOE's TSPA Peer Review Panel, there was a comment in their second interim report in December 1997 that, "When crevice corrosion is active, the metal penetration rates are high and rapid, penetration can be observed 1 to 10 millimeters per year." I might note, by the way, that I think two members of that panel are here in the audience, Dr. Budnitz and I think that Joe Payer was also involved in that particular meeting. So, they should be quite aware of that quote, although it may be taken out of context and they may want to quote on it later on.

DOE itself uses some crevice corrosion propagation data in their results. In the September 2003 Corrosion AMR, they've weaved their reviewing, as well as the NRC. They give a distribution for crevice corrosion rates somewhere between 12.7 microns per year to 1270 microns per year.

That's in one of their own data points. And, I might point out that the thermal pulse is supposed to last about 1000 years. At 12.7 microns per year, you'd lose about 13 millimeters of material. That's at the lower bound. Obviously, it's 100 times larger than that at the upper bound. So, those would be pretty severe corrosion rates in that particular case.

And, so, there is data out there not only that the initiation of crevice corrosion could be a problem, but we know very little about the propagation of the crevice corrosion process.

Going to the next slide, I'm not going to say too much about general corrosion, because we don't know very much about it. At the present time, I think it's assumed that the passive current density that will be observed on short-term polarization first represents the general corrosion rate, that is, the current density associated with that. We know almost nothing about the temperature dependence, although there was some data produced at Livermore on short-term electrochemical data that seemed to imply that the temperature dependence obeyed a typical Arrhenius relationship going up exponentially with temperature. We don't think that data has been fully utilized at this point, although, again, it's not our position to tell the DOE how to utilize data, but just that it's simply out there.

Let's take a look at the next slide, and I'm going to make this fairly quick. What are the implications? Of course, we have significantly reduced the safety margin. And we've weakened the multiple barrier concept. We've reduced confidence. Recently, I had to testify before a Congressional subcommittee, and one of the questions that was asked of me about this corrosion problem was that if you breached the containers by corrosion, do you automatically jeopardize the environment, that is, will it not meet the regulatory condition. And, my answer to that was the TSPA that's used is very complex. This is a problem that I think that we believe as a Board can be avoided by simply lowering the temperature into a situation where you can't get crevice corrosion.

So, the answer is I think the calculations would indicate that TSPA says that if I breached the containers, you will meet the regulatory requirements, but just barely. That makes an assumption that your models, which are fairly complex, are accurate. That's a potential problem. So, I think there is some reduced confidence in that particular case.

I don't think the Board wants to go on record for saying that corrosion of the containers, or breach of the containers by corrosion, will necessarily jeopardize the environment. We're simply saying that it doesn't make sense as far as we're concerned to simply throw away a potential barrier and rely entirely on mathematical formulas to decide whether or not radionuclide release is going to occur.

The next slide, we've labelled this research that's really not what we're interested in. I think these are the things that concern us about the unknowns at the present time, that is, what are the expected repository environments? I think none of us believe it's going to be necessarily just saturated with calcium chloride at 150 degrees celsius. But, we don't know what that is, and can only react to the data that's been presented to us by the project at this point.

We know almost nothing about crevice corrosion propagation. I don't even think that anyone has done a good job yet on modeling or determining what the environment would be in a crevice set up by dust sitting on the surface of a container. We don't think that thermogravimetric tests that have been done are complete, and there's a lot to be done, and of course this issue of nitrate, which does inhibit some degree of crevice corrosion, although not very much, as you saw, there was only about a 10 degree bonus that you picked up from it, at 150 to 140 degrees in that area, and we believe that there's also a lot of data out there in the literature that still hasn't been accessed completely, and can be used to make some of these determinations.

And, so, I think our parting comment is that we believe that crevice corrosion is a possibility. We think it can be completed avoided by simply lowering the temperature, assuming that the environments we're looking at are the environments that we can see in the repository.

And, the last slide--that was the last slide. So, the purpose for the letter was simply to say that based on the data that has been presented to us by the Department, there is evidence that given the environment that the tests were performed in, that crevice corrosion will occur. And, if it will occur, it probably will proceed at a fairly rapid rate. And, I think that concludes my remarks for the present time.

LATANISION: Thanks to Dave and Thure. I'd now like to honor the commitment I made at the outset, and that is to open the discussion up to the audience. By my reckoning, we have about ten minutes of time allocated for the presentations and for Q and A. So, the floor is open. I would just ask you to identify yourself when you come up to the microphone. And, if I see no questions, I'll start asking some. Roger?

STAEHLE: I don't know if this is a question or not. Roger Staehle consultant for Nevada.

You know, one of the things that nitrate does, aside from inhibiting some things, is a very potent oxidizer, and it's not so clear to me in this system that it's functioning so much as an inhibitor, but maybe more importantly as an oxidizer.

The second problem, I think, has to do with this question of what's on the surface. The surface is really a hot surface, and hot surfaces tend to concentrate solutions. I think what hasn't been dealt with, unfortunately, is the detail of the hot solutions and their corrosive behavior. And, I'm not so sure it's a crevice problem as it is one of simply a concentrated solution that's sequestered. Now, that's a little bit different, because you can still get access of air. I mean, it's not like a differential cell. But, maybe what the problem is is we have a not quite unboundable, but almost unboundable problem that has a lot of discussion yet to come, and I'm concerned pretty fundamentally about whether or not we have even approached the question or approached the problem of how do we model it and can we bound it.

LATANISION: Any response or comment on that issue? Go ahead.

DUQUETTE: Duquette, Board.

I can't disagree with you. I think that the very thing that we're concerned with is the concentration of these salts on the surface at the present time. Whether you want to consider it a crevice or not, I do think the remainder of the canister, if you will, is a very good place for reduction of oxygen. And, so, there's going to be some differential action between what's happening underneath a dust particle and some other concentrated species on the surface, and that's going to help drive the situation.

STAEHLE: Yeah, that clearly will be a driving process. It's just that you were speaking about nitrate, and I was thinking, well, the nitrates do several different things. But, the lower pHs, the primary role of the nitrate is read to be an oxidizer.

LATANISION: I saw Joe Payer's hand. Joe, why don't you approach the microphone.

PAYER: Joe Payer, Case Western Reserve, and a DOE consultant.

A couple points. This issue of will dust act like a crevice, it's pretty clear it's not a traditional crevice that we form in the laboratory using teflon and forming very tight crevices. The experience is that with Alloy 22 metal to metal type crevices are difficult to get started. There's not a lot of information on ceramic Alloy 22, and I would agree, Dave, there's not much on dust. But, it's not the traditional crevice corrosion that you see in the corrosion textbooks, and things of that sort. You can have occluded cells, you can affect the environment. And, that's an active area of research.

I think you will see a lot tomorrow, and the rest of this afternoon, about what is understood about the chemistry and what happens. There's work at several different places that are addressing that, what happens under the dust, and so forth.

But, the other part is that, a comment to make, and we'll reiterate this tomorrow, that using the criteria for crevice corrosion of the critical potential and the corrosion potential, and the difference between those as a criteria of can crevice corrosion occur, is certainly widely accepted. I don't think anybody is contesting that.

But, what we will show tomorrow, or just remind folks, is that when you meet that criteria, it doesn't necessarily mean that crevice corrosion starts and continues and propagates. There's this issue of propagation rates. Also, it's an issue of will that environment, if it's formed, will it persist, and is there a crevice there that in fact it will sustain it. So, just to meet that first criteria is the first step in the decision for you. Thank you.

LATANISION: Joe, while you're on the floor, let me pursue the comment that Dave quoted from the TSPA Peer Panel in 1997. 1 to 10 millimeters per year, hypothetical or what's the perspective?

PAYER: I don't remember the quote. I probably made it. But, I think what that's based on is when you measure the initial corrosion rates under crevice corrosion of a susceptible alloy, you know, the standard ones that we always look at are the austenitic stainless steels, 304, for example, and if you look at the initial corrosion rates of those, they can be very, very high. So, then, the issue is will that rate be sustainable, and again, we'll talk a little bit about it tomorrow, but we believe that when you're not fully immersed in a beaker of environment, or in a laboratory cell or in a marine environment, can the cathodic reduction activity outside the crevice support those rates for very long? And, we don't believe they can.

LATANISION: We'll look forward to tomorrow's presentation.

PAYER: There you go.

LATANISION: David Shoesmith?

SHOESMITH: David Shoesmith, a consultant to Bechtel. Actually, Joe addressed most of the points, but I just wanted to address one issue, which is the corrosion potential and the critical potential are on a collision course at all times, and that oxidizing conditions are forever driving the corrosion potential positive, and bad environmental conditions are forever pulling down the critical potential. That is not actually true. As bad environmental conditions develop, they actually pull down the corrosion potential as well, and it's not necessarily as easy to naturally, without the electrochemical, the advantage is the electrochemical driving forces to get that criterion to be established. It seems to be particularly difficult on Alloy 22.

LATANISION: David, just a comment on that point. If you examine the data that we have been presented from project work, and some of it shows up in the backup slides on Dave Duquette's presentation, it is very clear that the corrosion potential is in fact approaching, is moving in the oxidizing direction.

SHOESMITH: But, if you look at that data, you will notice that as you lower the nitrate concentration, the corrosion potential actually drops as a function of the nitrate concentration.

So, my point is as you are going more aggressive in the environment, not only are you pulling down the critical potential, which is the one you're concerned about, but you're also simultaneously pulling down the corrosion potential.

LATANISION: Let's end on this point. But, could you show me the first of Dave Duquette's backup slides? That one. We're looking here at temperature dependence of corrosion potential, and the critical, repassivation potential in this case. And, you can see the change in the repassivation potential, which is becoming more reducing, change in the corrosion potential is becoming more oxidizing. But, even more importantly, after years exposure, the corrosion potential of the base metal has increased into this band, and the corrosion potential of a welded structure is even in a more oxidizing band.

Now, we could discuss this data, and perhaps find some common ground, but I'm simply making a point that based on data that has emerged from project work, it would tend to support the comment that Dave made.

SHOESMITH: That wasn't the data.

LATANISION: Okay, I'm sure it wasn't. That's fine. We'll take one more question. Comment from Roger Staehle, and then we will go on.

STAEHLE: One of my concerns about these data and this discussion is that the nitrate is not inherently an inhibitor. Nitrate happens to inhibit some reactions, not necessarily because of being at some kind of an absorption process, but in fact maybe because it raises the potential and takes you out of the zone that cracks, or does something. But, in acid solutions, nitrate really does raise the potential. It is not an inhibitor. I think to make the assumption that nitrate, just because it's nitrate, is an inhibitor is wrong. And, I think to put that up there as a nitrate inhibitor and leave the impression that nitrate is always an inhibitor is very, very misleading.

LATANISION: Fair enough. We're going to now end this conversation, and I'm going to ask Dan Bullen to take the chair, and we will continue with some presentations by our friends from the NRC.

BULLEN: Thank you, Ron.

Contrary to my predecessors today, I'm going to be very strict in adherence to time. I have the magic time device right here, which for each Panel, Panel's are allotted about 75 minutes, I'm going to set it to 60 minutes. After 60 minutes, the timer goes off, at which point, I'd like to begin questioning. So, we're going to wrap it up at that 60 minutes. Unless you wanted an earlier notice, I'm just going to do it to that extent.

I also want to apologize to each of the Panels, because we normally do do very detailed introductions, noting the very significant credentials of the people that are presenting.

The next three sessions that we have, we'll have two before the break, and then we'll have one after the break, the first session is by the Nuclear Regulatory Commission and its contractor, the Center for Nuclear Waste Regulatory Analysis. Presentation from the NRC will be made by Tim McCartin, Roberto Pabalan, Darrell Dunn, and Tae Ahn, and Tim McCartin will begin, and I will set the magic time device for 60 minutes.

MCCARTIN: Thank you. I will have some very brief remarks to introduce my colleagues to provide some context for the presentations you'll hear in far more detail about the corrosion processes.

First, I'd like to go to my first slide, in terms of giving some context for the NRC approach to regulatory review and getting ready for the regulatory review of the DOE license application, first, it's a risk informed approach where we would be focusing on those things most important to safety. Second, we support exploratory and investigative studies at the Center for Nuclear Waste Regulatory Analyses in key areas where the data is limited.

Thirdly, and I will spend a little time on this one. We use performance and safety assessments to assist our understanding. I possibly should have capitalized and use the bold font for the word assist. I did underline it. It does not do our thinking for us, and I know ever since we published Part 63 as one of the authors of that, people have in part interpreted that we would run a performance assessment code, look at the final result, and compare it to a limit. It's either above or below. Our three year regulatory mandated review would take three minutes, and I guess we'd spend the rest of the three years acting like we're busy. But, no, that's not the case. And, let me explain what I mean when we say we're going to use this performance assessment to assist our understanding.

I've been running performance assessment codes for over 20 years at NRC for high-level waste disposal. I still don't believe any number coming out of a performance assessment code. What I use is the performance assessment code to challenge my thinking, and now my job is you run the code, you see the results, now it's a question of why should I believe those results. And, that really, to me, is the performance assessment process, going in and understanding all the attributes of the repository system, how uncharacterizing, how it's being represented in the performance assessment, why do I believe that's a correct representation of the performance. And, that really is the way performance assessment is used. It challenges us.

I remember two or three years ago at a Board meeting, Dan Bullen looked at DOE's performance assessment calculation where they showed the results of a hot and cold repository were somewhat the same. He said he didn't believe it. I believe it was Dan who said he didn't believe the results. A fair statement. The question then is is looking at it, well, why don't you believe it? What's wrong with this? And, all that thinking process, that that is what's going to take the years for the NRC review. Maybe there's something wrong with my understanding of how things behave. Maybe there's something missing in the performance assessment code that needed to be in there. Maybe something is represented incorrectly.

But, that process of going through and pouring through the results, why should I believe it, I think it gets back to the first bullet, risk informed. What are the important attributes of the system? Have I captured it, and is it appropriately represented. And, compliance, in terms of comparison of the dose limit, ultimately, clearly we want to see what relates to that dose limit, or to the dose estimate. But, just comparison is the easy part of the job. We would expect, as all NRC applicants when they come in, they are showing that numerically, they are below our limits. The question is have they demonstrated why they are below the limits, and that's really the essence, in my mind, of the performance assessment review.

Additionally, we would consider all publicly available information in doing our review.

Next slide? In terms of the three talks you'll see after mine, and I promised I will keep to my five minutes, first certainly we heard about the near-field environment. Bobby Pabalan will talk about that. Darrell Dunn will then talk about factors influencing uniform and localized corrosion in Alloy 22, and Tae Ahn will follow with sensitivity analyses we've done with the waste package. All of these are in the context of understanding the corrosion processes, and how they relate to representing a potential repository at Yucca Mountain.

Next slide? However, I do want to point out importantly, the regulatory review is based upon DOE's design and technical basis as they describe in their license application. As the applicant, DOE has the responsibility to support and defend its performance assessment and its results.

Next slide? And, the reason I say that first is you will see certainly the NRC, as any technical person when you start a review, you will bring your experience, your understanding to inform the review. Ultimately, you will see my colleagues present some understanding. It's what the DOE presents. It's not our analyses. It's DOE's analyses.

We continue to prepare for the license application, and certainly once again, today you'll see us have some results with respect to performance assessment, some statements made about chemical environments, corrosion rates, et cetera. Conclusions regarding the performance of a Yucca Mountain repository will come based upon our licensing review. We are not there yet. This is not our licensing review. We don't even have the license application.

So, I'll conclude with that. Those are some context remarks, and I'll turn the stage over to Bobby Pabalan.

PABALAN: Thanks, Tim.

There are three types of potential in-drift water sources. One, seepage water. Two, deliquescent brines. And, three, condensed water. The evaluation of the chemistry of in-drift waters, and it depends on the fact on the degradation of drip shields and waste packages is complicated by the effects of coupled thermal hydrological chemical processes.

Next? In addition to the temporal evolution of the temperature and relative humidity within the repository, a complicating factor is the spatial variation of temperature and relative humidity, as indicated in this schematic of the temperature and relative humidity within the repository footprint, where the center of the repository will be hotter, and with a lower relative humidity relative to the intermediate portions of the repository, and certainly relative to the edges of the repository footprint.

Next? To simplify the identification and evaluation of the potential scenarios for aqueous corrosion of drip shield and waste packages, we define four thermal hydrological environments in a potential Yucca Mountain repository.

First, we define a dry environment at relatively high temperatures that is characterized by the absence or near absence of seepage water or condensed water at this high temperatures. The water above the drifts is unable to penetrate, avoiding isotherm, or at least the probability of seepage water entering a drift is very low.

The second environment is still above the boiling isotope, but the likelihood of localized penetration of water into the drift is much higher, so you have seepage water that can undergo some evaporation processes.

The third environment in our thermal hydrological model is below--the temperature of the drift wall is below the boiling point of water, such that there's no more seepage coming into the drift environment, and evaporation processes occur, as well as condensation of water inside the drift. This is a much wetter environment than the first two.

And, the fourth one is when you now have considerably reduced temperatures relative to the first three. Evaporation rates are certainly much reduced compared to environments three and two, but condensation of--there's circulation of hot moist air within the drift environment, and condensation of these moist air occurs in the colder parts of the repository. This mixing of condensed water can potentially alter the chemistry of any seepage water inside the drift.

Next? Of most concern for us under the dry period is the deliquescence of salts on the waste packages that can form brines and could result in the initiation of localized corrosion of Alloy 22.

Next? For environment two, where you have seepage plus evaporation, the evaporation of seepage water could result in brines with high concentrations of corrosive species, such as chloride and fluoride on the drip shield, and also on the waste package surface after drip shield failure. In this environment, you can also form brines by salt deliquescence.

Next? Under environment three, you have the same potential corrosion environment as in environment two, but condensation here is more important than in environment two, and could modify the quantity and chemistry of in-drift waters.

And, lastly, for environment four, the water will be relatively dilute, and the potential for localized corrosion is likely reduced.

Next? As I mentioned, the process of most concern to us for the dry environment is the deliquescence of salt mixtures. The deliquescence relative humidity of salts or salt mixtures that are present on the drip shield and waste package surfaces determines the time and the temperature of rewetting of those surfaces. For example, for this figure where you have deliquescence relative humidity of 50 per cent, a value used by the DOE in its TSPA for viability assessment, one could have an initiation of corrosion at approximately 700 years, just for illustrative purposes, and a temperature of about 115 degrees centigrade.

Next? On the other hand, if the deliquescence relative humidity goes down to 30 per cent, then you can have an initiation of corrosion at much earlier times and also at much higher temperatures.

Next? There's some uncertainty with respect to the deliquescence relative humidity of salts and salt mixtures. In particular, there's really very little data for the DRH of aqueous mixtures. We have been conducting experiments to determine the deliquescence relative humidity of aqueous mixtures of cations, of the cations calcium, magnesium, sodium, potassium, and the anion chloride, carbonate, bicarbonate, nitrate, sulfate. We are also interested in the potential effects of corrosion products, so some of the experiments involve using analogues for corrosion products, chromium, chloride, salts, and also ferric chlorides. Measurements were done by two methods. One, with a hygrometer, and another using conductivity cells.

Next? Some of our results have shown here what is clear from these experiments is that when you have salts involving calcium and magnesium, whether in the form of chloride or nitrate salts, those salts or salt mixtures tend to have very low deliquescence relative humidity. Another interesting point is that once in the presence of corrosion product analogues, such as chromium chloride and ferric chloride, these salts contribute to the lowering of the deliquescence relative humidity of the salts or salt mixtures.

We observed that if these two salts are present, it is possible to sustain the low deliquescence relative humidity for the system of interest. Also of interest is the deliquescence relative humidity for the mixture of sodium, potassium, chloride and nitrate. This mixture is the predicted predominant composition for Yucca Mountain seepage water based on the DOE analysis. What is interesting is the relatively strong temperature dependence of the deliquescence relative humidity for these mixtures. We don't have experimental data right now above a temperature of 85 degrees. We are still in the process of setting up our equipment that hopefully will take us up to about 150 degrees centigrade.

But, if you extrapolate the temperature trend for this particular mixture, it is possible to speculate that even for these kinds of waters, that you can have relatively low deliquescence points of elevated temperatures.

Next? The important thing with respect to deliquescence of salts is that even if deliquescence occurs at relatively low values at high temperatures, what is important is the composition. There are a few samples taken by the USGS inside the ESF that suggest the salt dust inside the ESF have a lot of chloride and also nitrate. But, there is additional information for dust compositions in the Yucca Mountain and vicinity that indicate the presence of significant concentrations of nitrate and sulfate. These oxyanions potentially can mitigate the localized corrosion of Alloy 22. These figures show tens of ppm of concentration for sulfate, nitrate, as well as of chloride, but of particular interest is the ratio of nitrate, sulfate to chloride.

Next? A potential process of concern for environments two and three is the evaporation of seepage water. As previous studies by the DOE have demonstrated, the chemistry of brines formed by evaporation is dependent on the initial composition of the seepage water. There's still some uncertainty with respect to the composition of water that may enter the drift. Our evaluation of this composition is still ongoing.

To provide us with some information about the potential range of chemical compositions that may arise by the evaporation of seepage water, we have conducted some thermodynamic simulations using a thermodynamic code to see what ranges in concentration of the chloride and also the oxyanions result by evaporation of a range of initial water compositions. Shown in this ternary diagram in pink are the USGS data for unsaturated pore water chemistry. We have selected about 30 of those compositions as inputs into our thermodynamic simulations of seepage water evaporation.

Also shown here for comparison are the eleven bins that DOE uses in its seepage model from the technical basis document Number 5. What we are interested in particularly are the concentrations of the corrosive species, chloride, fluoride, and also the concentrations of the inhibitors, particularly nitrate, sulfate and carbonate.

Next? There is also shown here, these are plotted in terms for the three brine types that are classified for the chemical divide theory, we have calcium, chloride, neutron or sulfate brines, and alkaline or carbonate type of waters. What the results show is that some brines can have high concentrations of chloride and certainly fluoride concentrations that can cause enhanced general corrosion of the titanium drip shield. But, what is interesting in perspective is that most of the waters also have a high ratio of inhibitors. For example, inhibitors, nitrate, sulfate, bicarbonate and carbonate, the ratio of these inhibitors for the corrosive species chloride.

Next? This is important because the window of susceptibility for localized corrosion of Alloy 22, as the next presentation will show, will be chloride to inhibitor concentrations approximately about 10 or higher. Most of the brines that evolve by evaporation of those waters with chemistry similar to Yucca Mountain saturated zone porewaters are relatively benign to Alloy 22.

Now, even for the calcium chloride brines that seem to have high chloride inhibitor ratios, certainly within the window of susceptibility of corrosion of Alloy 22, these high chloride inhibitor ratios result from the formation of the calcium, nitrate and sodium nitrate aqueous complexes. And, at this time, we acknowledge that these aqueous species have uncertain thermodynamic data, which we are still evaluating.

Next? Now, Catholic University has conducted a laboratory study showing acidic condensates where HCL and nitric acids are formed by evaporation of calcium chloride type of porewaters. Some of the results are shown here, which the pH is a function of volume fraction evaporated, showing the tendency to form very low pH, some less than 1. These experiments have used an experimental system shown here, where an upright condenser was used to minimize or reduce the loss of fluid from the system. In essence, it's a relatively closed system.

Next? We've done our own thermodynamic analysis to see if we can duplicate the results of these experiments. What our simulations show is that if you evaporate these waters, yes, you can form very acidic conditions, but look at the fraction evaporated. These are very extreme evaporations. The temperatures are for these last fractions of condensates and residuals are at very elevated temperatures.

Next? Certainly, to form this acid condensate, you're going to be above the seepage threshold, or what is also called the vaporization barrier, so that the likelihood of forming such acid condensates are very low in a repository setting.

Next? So, we acknowledge that such mechanism of acid gas generation is possible for some seepage water compositions, but is likely not to be significant to performance. Like I said, it requires an extreme degree of evaporation to reach the pH of 1 that I showed in the previous diagram, requires a concentration factor of about 20,000 times. To put that into perspective, you'll need to evaporate 100 liters into a few teaspoons. It also requires the high temperature, which is above the vaporization barrier, or seepage threshold.

In addition, there are mechanisms that can mitigate the formation of acid gases and its effect on corrosion. The acid gas likely will mix with other in-drift gases, mainly through natural convection. There are also interactions of those acid gases that an occur with the wall-rock, with the in-drift materials, and also with seepage and condensate waters.

Next? For example, these are calculations that show if you mix acid condensates with an initially low pH of

5.6 with some porewater that certainly would be present inside a drift, you can get pH pretty much close to neutral by this mixing process.

Next? This figure shows a simulation of a reaction between a condensate with pH initially of about 6 or so, with an analog for Topopah Spring tuff. The simulations show that within a matter of days, you already achieve a pH close to neutral, and within a period of 200 days or so, you can achieve steady state conditions.

Next? In summary, in support of the NRC regulatory activities, we have been conducting experiments and thermodynamic modeling to define the range in chemistry of waters that potentially can contact the drip shields and the waste packages.

Next? Of the four thermal hydrologic environments considered, we believe environment two has the greatest potential for accelerated corrosion of the drip shields and of the waste packages after drip shield failure. But, the concentration of corrosion inhibitors may be high enough to mitigate the potential for localized corrosion of Alloy 22.

Environment four, which has the longest duration of the four environments that we considered, has a limited potential for enhanced corrosion of the drip shields and waste packages.

Thank you.

BULLEN: Bullen, Board. My compliments. You're three minutes in, and halfway done, that's great. Dr. Dunn, you're on.

DUNN: No doctor. Okay, well, first let me start by acknowledging my contributors at the CNWRA, and also the Nuclear Regulatory Commission for funding this work.

Next slide, please? I'm going to just talk mostly about localized corrosion of Alloy 22, and I'm going to go over the effects of temperature, aggressive and inhibiting species, and metallurgical conditions, such as what happens if you fabricate, weld or thermally age this material. I do have one slide where I'm going to talk about passive dissolution and the effect of temperature and metallurgical condition, and also loss of passivity that can occur if you were in a high temperature acidic chloride solution.

This slide shows some uniform corrosion rates that were measured using electrochemical impedance spectroscopy with Alloy 22, and both of these are done as a function of temperature. The slide here on the left shows the mill-annealed alloys, the black symbols. And, as you can see, the corrosion rate does increase if you go to elevated temperatures.

I'd like to point out that this is data that was obtained for a short-term exposure, and we're pretty confident that the corrosion rate actually decreases with time. So, I wouldn't take this activation energy just yet.

If we look at the effect of fabrication processes, these are shown as the blue diamonds and the inverted triangles, the inverted triangles being the as-welded material, and the blue diamonds being thermally aged material, both of these materials have topologically close pack bases which consume molybdenum and their primary effect really is to increase localized corrosion susceptibility, but there also is a slight effect on the uniform corrosion rate.

The slide here on the right shows the same data for the mill-annealed Alloy 22, and I also have some data here for, again, mill-annealed Alloy 22 in a very concentrated magnesium chloride solution. In this particular solution, you can see that there's much higher corrosion rates, because in this particular condition, which is 7 molar chloride, there are less than pH 3, one may have a difficult time maintaining an acid film on the alloy. And, so, you can get higher corrosion rates under those conditions. But, this type of condition with this pure, very concentrated chloride solution is not something that we would expect in the emplacement drifts.

The rest of the presentation, I'm going to just talk about localized corrosion tests. The slide here shows an example, or the figure here is an example of some of our localized corrosion tests where these are electrochemical tests where we control the potential of the specimen. This is plotted as this black line here, so we start at some low value and we ramp the potential up and sit at some high value for a while, and try to initiate localized corrosion of these specimens. After that occurs, we slowly decrease the potential and measure repassivation of crevice corrosion.

This crevice corrosion repassivation potential is what we use in the total performance assessment code for assessing the localized corrosion susceptibility of Alloy 22. We use these tests to evaluate the effects of inhibiting species, such as nitrate, bicarbonate, sulfate. Also, we looked at different fabrication processes, welding, post-weld heat treatments. And, these tests are backed up with some long-term potentiostatic tests that are done under a long period of time, a number of months, and also some open circuit potential tests where we look at the initiation of localized corrosion under open circuit conditions.

This particular figure here shows results for a pure chloride solution in the red, where we observe that the current density is quite high when we initiate localized corrosion. If we take a similar solution with 4 molar sodium chloride and sodium nitrate, a little bit of sodium nitrate in the solution, no localized corrosion is initiated, and you can see there is quite a different current response for this material.

Well, this, I guess, very similar slide shows up in Dr. Duquette's backup slide, so I think the Board has seen this particular data set before. The blue symbols here are for the mill-annealed Alloy 22. The thing I want to point out is that there is a strong effect of alloy composition of course as we increase alloy and all that composition, with particularly molybdenum, we push the region of susceptibility of these alloys to higher potentials and higher chloride concentrations. This figure also shows the triangles here, the red triangles, or the black triangles, either thermal aged material or as-welded material, and you can see that if we take Alloy 22 and we do some fabrication process, we shift the susceptibility of this material back towards lower chloride concentrations and lower potentials.

So, clearly, the material in the as-welded condition, or thermal aged condition, is more susceptible to localized corrosion compared to the mill-annealed alloy.

Next slide? Again, this is a very similar slide that shows up in Dr. Duquette's backup slides. This was data that was also, of course, previously presented to the board. The only thing I've added here is the different environments from Dr. Pabalan's presentation. So, this is environment one, which we expect to be essentially dry, no seepage, and then environments two and three are a combination of evaporation and seepage, evaporation, seepage and condensation, and in environment four, at much lower temperatures is the seepage and condensation.

So, at high temperatures where you would expect to see enhanced susceptibility to localized corrosion, and certainly that's indicated by the low values of repassivation potential, the modeling here would indicate that the environment here is actually dry, and there's no seepage water coming into the drift.

The figure here on the right is also the same as what was presented in Dr. Duquette's presentation. This was the thermally aged alloy at 60, 80 and 95 C. I've thrown in some additional data here. This is a welded Alloy 22 that's been solution annealed. It behaves a little bit differently than the thermally aged alloy, but what we were actually doing here is using the performance of the thermally aged alloy to represent, give the as-welded, or welded in solution annealed Alloy 22.

Next slide? This slide shows some corrosion potential measurements of Alloy 22 in a variety of different solutions. What's shown here is the corrosion potential is clearly a function of pH. It's not really a function of chloride concentration. The red open circles here are 4 molar chloride at around pH 3, and look at a similar set of data in a much more dilute chloride solution, there's very similar corrosion potentials. If we go to more alkaline pH, you can see that the corrosion potential drops quite a bit.

And, the figure here on the right is the corrosion potential data, superimposed is bands that are independent of chloride concentration, with the repassivation potential data measured for the thermally aged alloy, which we were saying represents both some thermally aged or as-welded or welded and solution annealed, and this blue line here is the repassivation potential data for the mill-annealed Alloy 22. In order to have localized corrosion occurring, you need to have a corrosion potential that's greater than the repassivation potential, and for mill-annealed alloy, that's possible if we're in concentrated chloride solutions, particularly if we had an acidic pH.

For the thermally aged alloy, because the repassivation potential has shifted towards lower potentials and lower chloride concentrations, we would expect this alloy to be much more susceptible to localized corrosion than perhaps a broader range of solutions.

I want to point out that this particular data does not include the inhibiting effects of the different anions that would likely be in solution.

So, this is the criteria here for localized corrosion initiation of Alloy 22 as shown here in the red. We say that the corrosion potential has to be above, not just above initially, it has to be above and be maintained above the critical potential for localized corrosion, which is the repassivation potential. Chloride concentration has to be above some critical value for localized corrosion to occur. And, we also have to have an inhibitor concentration that is low with respect to the chloride concentration solution, and some of the subsequent slides that I have will show this data, and also, the temperature has to be above a critical temperature for the localized corrosion to occur.

If these conditions are satisfied, the PPA code calculates the repassivation potential using this common regression equation, and I've put values for these different parameters here in the table. We have values for the mill-annealed alloy, and a different set of values for the thermally aged alloy. I've provided some temperature ranges over which these parameters are valid.

The critical chloride concentration for the mill-annealed alloys have molar, and for the thermally aged alloy, at high temperatures, it can be quite low, it can be .01, but down at 60 C, it increases quite a bit. And, some of the subsequent slides will show the inhibit chloride effects. For the mill-annealed alloy, a very small concentration of inhibitors will completely inhibit localized corrosion of Alloy 22. You need a little bit more for the thermally aged alloy.

Next slide? This is more recent data that we haven't presented to the Board before, looking at both mill-annealed Alloy 22, and also thermally aged Alloy 22. This was done in very concentrated 4 molar magnesium chloride, temperatures up to 110 degrees C. And, what's shown here is the repassivation potential as a function of the nitrate to chloride concentration ratio. And, what you can see is that if we just look at the high temperature data, one can see as we increase the nitrate to chloride ratio, we see an increase in repassivation potential. We still get localized corrosion. A little bit higher, localized corrosion is still observed, but repassivation potential is getting very high, and we don't want that. We don't want localized corrosion at all.

The same thing for the thermally aged alloy, the same type of response, it just takes a higher value of nitrate to chloride to completely inhibit localized corrosion. The bars here at the top indicate the likely range of nitrate to chloride in evaporated brines. And, so, for most of the evaporated brines, the nitrate to chloride ratio is sufficient to inhibit localized corrosion of the mill-annealed alloy, and a substantial fraction of the brines, evaporated brines, would have enough nitrate to chloride to inhibit localized corrosion of the thermally aged or welded Alloy 22.

Next slide? This slide shows data for sulfate and fluoride. Again, this is thermally aged Alloy 22. We used a lower chloride concentration here because sulfate and some of the other oxyanions have more limited solubility, which I'll show in a subsequent slide. So, we wanted to use a lower chloride concentration to expand the range of anion to chloride ratio that we could explore. And, what we see here is that if we add a sufficient amount of sulfate to solution, again, a sulfate to chloride ratio of about .1, we pretty much inhibit localized corrosion. We do have one case where we're getting localized corrosion, but the repassivation potential is quite high, certainly above what we would expect for any value of open circuit potential. We don't see that fluoride inhibits localized corrosion of Alloy 22. It really appears to act more as a diluent, which means that it neither inhibits localized corrosion, or does it enhance the effect of chloride. So, it doesn't act as a synergistic ion with chloride.

The likely range of sulfate to chloride in evaporated brines, however, is fairly low, and, so, this is the upper end right here, about .02. So, it wouldn't appear as though many of the evaporated brines would have enough sulfate by itself to inhibit localized corrosion of Alloy 22.

This is a similar data set with, again, thermally aged Alloy 22, and half molar sodium chloride. And, here, we're looking at carbonate and bicarbonate as inhibitors for localized corrosion. And, so, what we see is if we add a little bit of carbonate to solution, repassivation potential jumps quite a bit. Add a little bit more, and we don't observe localized corrosion at all.

A similar effect with bicarbonate, it doesn't appear to be quite as good, but it's pretty clear that both carbonate and bicarbonate can be inhibitors of localized corrosion. And, again, the bar at the top indicates the likely range of both carbonate and bicarbonate to chloride in evaporated brines. And, so, for some of these evaporated brines, there could be enough carbonate and bicarbonate alone to inhibit localized corrosion of Alloy 22.

This figure shows the maximum concentrations of carbonate, sulfate, bicarbonate as a function of chloride concentration. It doesn't indicate what we expect to be there, just the maximum value that you could put in solution and still be soluble. So, you know, our tests were done in half molar sodium chloride solution, and these particular speciation calculations, of course, show that as you get to really concentrated chloride solutions, the amount of these oxyanions that you could put in solution diminishes quite a bit.

That's not true for nitrate. It's highly soluble, as I showed in some of the previous slides, and can act as an inhibitor, even in concentrated chloride solutions. So, again, our likely range of nitrate to chloride in evaporated brines ranges from maybe just below the threshold of critical value for the mill-annealed material, up to values well above the critical nitrate to chloride ratio to inhibit localized corrosion for either mill-annealed or thermally aged Alloy 22. And, this assumes that none of the nitrate complexes, calcium nitrate or sodium nitrate complexes, that Dr. Pabalan mentioned would occur.

If we look at all the inhibitors, that means nitrate, sulfate, carbonate and bicarbonate, it's slightly higher, mainly because of the contributions of carbonate and bicarbonate. And, so, this value is slightly elevated for most of the brines, most of the evaporated brines. And, again, our premise here is that localized corrosion is inhibited if we get an inhibitor to chloride ratio that's greater than about .1 for the mill-annealed material, about .02 for the thermally aged or welded Alloy 22.

This table shows a summary of environmental and metallurgical factors for localized corrosion. In just kind of a decoder wheel here, the plus symbol indicates an increase in corrosion potential, or repassivation potential. The minus, of course, is a decrease. And, zero is no change. And, topping the list, we think it's really obviously the most significant, if we have the nitrate or other inhibitors in solution, don't expect to see too much of a change in corrosion potential, but we do see a substantial increase in repassivation potential, indicating that the material is not likely to be susceptible to localized corrosion.

If we see an increase in pH, this tends to decrease the corrosion potential. It doesn't have any affect on repassivation potential.

The chloride concentration I've listed here is decreasing the corrosion potential, although you will note maybe in one of my previous slides, we didn't really see that very well. If we went to really concentrated chloride solutions, perhaps neutral pH chloride solutions, we would see a solving out, a decrease in the dissolved oxygen concentration, and that might actually decrease corrosion potential, but we didn't actually observe that in our tests.

We do, of course, observe that it decreases the repassivation potential. And, of course, temperature, we really think that if you increase temperature, you decrease corrosion potential, at least at temperatures below boiling anyway. And, certainly we do see a decrease in the crevice corrosion repassivation potential.

Some of the other things I didn't cover here, the effect of reduced sulfur species and other species that can increase the corrosion potential, like radiolytic species, hydrogen peroxide, ferric irons, for example. We do see an increase in corrosion potential if we age the passive film, although this is pretty limited. It doesn't affect repassivation potential. And, the fabrication process is where we have formation of intermetallic phases at grain boundaries, or segregation of alloying elements in welds. These tend to have a negative impact on repassivation potential, but don't affect corrosion potential too much.

So, our summary, we have looked at passive corrosion rates. They are dependent on temperature and metallurgical condition, but the passive corrosion rates are low under steady state conditions. We have observed an accelerated uniform corrosion of Alloy 22 in acidic concentrated chloride solutions at high temperatures, but we note that these solutions are not expected within the emplacement drifts.

The localized corrosion susceptibility of Alloy 22 depends on a number of factors, include chloride concentration, concentration of inhibitors, temperature, and, of course, metallurgical condition. The fabrication processes can have a negative impact on localized corrosion resistance.

A number of the anions studied have been shown to be effective inhibitors, nitrate, carbonate, bicarbonate, and sulfate, when they are present in sufficient concentrations relative to chloride. And the nitrate to chloride concentration ratio necessary to inhibit localized corrosion is in the range of .1 to .2, slightly dependent on chloride concentration, temperature, and metallurgical condition.

So, if we went to even higher temperatures, we may have to have an increase, a slight increase in the amount of nitrates you would need. But, as long as you have nitrate present in sufficient concentrations, I would expect it would inhibit localized corrosion.

Thank you.

BULLEN: Bullen, Board.

Thank you very much, Darrell. I'll point out to Tae Ahn that my little timer says ten minutes left. So, you might want to cut their funding next year so they don't talk so well. See, Darrell, you can't win. I'm sorry.

Tae, you're on.

AHN: Good afternoon. Bobby Pabalan addressed the importance of the evolution of the high temperature deliquescence salt, including especially two salts. One is the calcium magnesium chloride. The other one is a mixture of sodium potassium chloride and nitrate, which will elevate the aqueous condition near 250 degrees C.

Then our later data done, conducted the corrosion experiment, considering the inhibitors, as well as the high temperature in determining the uniform corrosion rate, as well as the--to localized corrosion. As Tim mentioned, in the regulatory perspective, we needed to know the consequence of those factors in the Total System Performance Assessment to assist with the understanding of the process.

What I would like to present here is to consider those high temperature deliquescent salt effects, also the inhibitor effect in the NRC's report on Total System Performance Assessment.

What I would like to present here is the previous analysis of NRC's Total System Performance Assessment. Then, our current analysis of Total System Performance Assessment, and a basis will be presented. And assuming we have a long-term passivity, I would like to go over issues involved in projecting the laboratory testing, which are all over the geological period. Then, I will conclude.

Next slide, please? This is the previous NRC analysis of Total System Performance Assessment Code. All corrosion parameters were from electrochemical tests in pure sodium chloride solutions. And, the deliquescent salt mixture or inhibitors were not considered. And, the drip shield life time was sampled from a lognormal distribution of 3700 to 27,300 years, and no corrosion failure of waste packages was detected in 10,000 years. This previous TPA exercise resulted in about 0.03 millirem per year at 10,000 years.

Next slide, please? In this current analysis, we considered the effect of the deliquescence salt reaching high temperature aqueous corrosion, and also the effect of inhibitors, and the effect of evaporation, assuming low crevice corrosion would occur.

Next slide, please. This slide has been shown already three times, including myself. This is crevice repassivation potential versus temperature. I would like to emphasize that this particular set of data is in pure sodium chloride solution, and the concentration varied from .5 molar to 4 point molar. 4 point molar means near saturation at this particular temperature. This is an important point. And, as you see here, it indicates scenario one and two and three, and in this temperature regime, the Alloy 22 will be susceptible to localized corrosion in pure sodium chloride solution.

And, the next slide shows when the inhibitors, in this case, nitrate, are added in sufficient amounts, this crevice repassivation potential will stay constant. As the nitrate concentration increased, the ratio increased from 2 to 4 in this case. A couple of points, this is the weighted Alloy 22. It's not real Alloy 22. The detailed windows of the susceptibility were given by both Pabalan and Darrell Dunn. I will not go over this one in detail.

What I am emphasizing here, with a sufficient amount of nitrate, the repassivation potential stays very high here, as the next slide shows.

This is the TPA output. The left slide is the analysis using current information of repassivation potential, up to 150 degrees C., considering such a high temperature deliquescent salt, such as calcium magnesium chloride, or a mixture of sodium potassium calcium nitrate. In this case, it does not have inhibitors, therefore, we expected a larger number of waste package failure. Indeed, about 87 per cent of waste package failed within 10,000 years. At 10,000 years, those went up to almost 3.7 millirem per year.

And, the right figure is from the exercise using the inhibitor curve, assuming abundant nitrate present. There are basically no corrosion failures of the waste packages was observed, and those were very low, 0.027 millirem, mainly from--failure of waste package. Again, in this case, pure sodium chloride solution.

Another note here is in this particular exercise, there was no drip shield. However, we believe availability of fluoride can limit the drip shield corrosion.

The next slide shows--before I go over there, I would like to mention that from the data and Bobby's presentation, the effect of temperature and inhibitors there is significant, and the high temperature deliquescence could occur in calcium magnesium brine, and in the brine of sodium potassium chloride nitrate mixture. The fracture of the deleterious chemistry such as a calcium chloride brine could be small. That's the first note here.

However, as the uncertainties associated with having beneficial or deleterious chemistry, we have developed, with time, we needed to consider the probability of having a deleterious chemistry from the high temperature deliquescence. So, this is the one example exercise of a probabilistic approach of the evaluation of high temperature deliquescence and inhibitors.

In this particular example, we sampled critical relative humidity to upset the aqueous corrosion from a normal distribution, from 0.35 to 0.60, and considering the high temperature deliquescence, as well as inhibitors in a random manner.

In this particular exercise, about 17 per cent of waste packages were failed from the distributions, giving those at 10,000 years about 0.95 per cent. And, this 17 per cent is important, representing the distribution of deleterious aqueous chemistry and inhibitor distributions, both in time and space.

And, the detailed distributions of the chemistry are deleterious or are beneficial chemistry, as well as the window of the susceptibility, such as anion inhibitor to chloride ratio, as presented by Darrell, are currently under implementation.

The next slide shows assuming localized corrosion could occur in certain areas, we needed to consider whether that the partial exposure of surface areas could affect the release of radionuclide. In this particular exercise, we modified inputs to estimate the effects of exposed surface area from size and the frequency of perforations.

There was some--this question about the stifling of the pits in the crevice this afternoon. This exercise is based on the observation that, first, pits could be stifled under open circuit corrosion conditions. If pits are kept in line, all criteria are critical repassivation potential, and so on, came from the extra chemical conditions, giving the forced electrochemical conditions. That's one basis, we considered the stifling and pitting the exposed surface area constant.

The second area is a crevice area likely to be restricted. You have limited distribution of particles, also limited rock bolts and contact area. These two facts led us to exercise the limited exposed surface area. This is those curves from the TPA exercise. This red curve is from the previous slide showing no effect of the restricted area. In other words, there was no exposed area from the pit. It's completely the waste package was removed.

The below one is a sample of the exposed area from

10--one to one, from the literature data, side and the pit density. As you can see, at 10,000 years, those dropped from 3.7 millirems to about 0.2 or 0.3 millirems per year.

The next slide shows--now, our data also showed some concern about the high temperature uniform corrosion rate. Because these two conditions of high temperature may lead to high temperature, we considered the effect of high temperature on uniform corrosion rate.

The first case is sodium potassium chloride nitrate combination, the effect. This case, corrosion rate is not very high at high temperatures. However, as Darrell mentioned, the corrosion rates I expect it to decrease with time. For example, weight loss measurements up to five years shows much lower value than the chemical test results.

In the case of calcium magnesium chloride high temperature deliquescence, pH may go down, leading to enhanced uniform corrosion, as shown by Pabalan, however, the fracture of these salts is low, as I mentioned earlier, and this salt is likely to decompose, and the resulting acids will evaporate.

BULLEN: Bullen, Board.

Tae, you've got about five more minutes.

AHN: Okay. That's all I need. And, the next slide shows time and extent of waste package corrosion is important. Given no localized corrosion condition with passivity from laboratory testing, we need to assess the stability of passive film over a geological time period. We use inference from modeling and analogue study, emphasizing potential long-term latent effects.

The next slide shows we considered in the modeling all the formation, anodic sulfur segregation at metal-oxide interface, anion selective sorption in crevice, and development of large cathodic surface area of corrosion products, all to see the stability of passive film.

In the analogue study, we investigated the responsible mechanisms for the long-term survivability of analogue, such as passivity, and models and analogues gives a better technical bases.

The next slide shows, we summarize, we need to consider both deleterious and beneficial conditions. We need to consider magnesium based and mixture, high temperature deliquescent salt. Waste packages could be passivated by the effects of inhibitors. The release can be limited by the limited amount of deleterious high temperature salt, and surface area exposed.

And, the performance assessment provides tools to evaluate the impact of these high temperature effects. Understanding of the stability of passive film over a geological time period is being conducted, assisted by analogues and modeling.

Thank you.

BULLEN: Thank you, Tae. And, thank you, Team NRC for giving such a nice presentation in a concise time.

Now, I'm going to go to the front of the room, because I guess I have to take questions from everybody.

Board members will be first, and I'll--no, I don't have a question for you. We'll start with the Board members. David, and then Ron?

DUQUETTE: Duquette, Board.

A couple of questions, and I'll try to keep them short. One of them is I don't know if you want to call it a policy question or not, but there's some testimony before Congress, Acting Chairman Diaz indicated that the NRC's data disagree with the Board analysis. Would you comment on that, please?

MCCARTIN: Well, I was not there for that testimony. What we've presented today are the results of the information we have and our current understanding of the state for corrosion of Alloy 22. I'd have to get back to you in terms of--I'm not going to try to guess, you know, exactly what the chairman was stating. I was not there.

DUQUETTE: Okay. A second question is, Duquette, Board, virtually almost all of your data are at temperatures at 95 degrees Celsius and below, with a few data points at 110, and some more recent stuff at higher temperatures. Is there some reason why NRC chose to stay at 95 degrees Celsius?

DUNN: Darrell Dunn, CNWRA.

The boiling point of water at Yucca Mountain is 96 C. We intentionally chose to go higher and lower, and I think that the calculations that were shown here for the seepage threshold would suggest that we've explored temperatures above and below the seepage threshold of water in the emplacement drifts.

We've explored a range that spans above and below that. And, certainly the data that's used in the TPA Code to model the localized corrosion of Alloy 22 goes above 95 C. In fact, the lowest temperature there for the material is 80. So, it goes from 80 to 125 C.

DUQUETTE: You also indicated that your inhibitor concentrations in general have to be greater than about 10 per cent of the chloride concentrations. Is that based on, obviously, your data says that. Do you agree that the salts that will be present in the repository will be at that ratio of, for example, nitrate to chloride?

DUNN: That's what, you know, the bars that I showed on the graphs where we indicate the likely range of concentrations. Essentially, that was 75 per cent or more of the evaporated brines would have those high concentrations of nitrate to chloride.

DUQUETTE: And, finally, Duquette, Board. You indicated

I think in your presentation that your observations were that as temperature went up, your open circuit potential went down, whereas I think the data that was shown on my backup, which is DOE data, shows the open circuit potential going up as temperature goes up. Any comment on why the difference?

DUNN: Well, we didn't actually present our data. In fact, we're not acquiring it yet. But, we've started at high temperatures and decreased that when we see that the corrosion potential goes up as we decrease temperature. That's the basis for my statement.

I think that the reason why you see the DOE data showing higher corrosion potentials at higher temperatures may be in part because much of that data is limited to very low pH simulated acidified water conditions. That particular solution has actually the greatest range of corrosion potential data over temperatures I think from about 30 or 25 to 90. Some of the other solutions that were near neutral, there was a more limited range of temperatures explored.

DUQUETTE: Duquette, Board.

No, I meant that the really high temperatures over 90. If you remember, that curve went up pretty dramatically between about 90 and 150, for example. The corrosion potential went up with temperature.

DUNN: No, I don't have an explanation at this time for that. I'm not prepared to comment on that.

DUQUETTE: Thank you.

BULLEN: Bullen, Board. I'm going to actually take a chairman's prerogative here real quick and ask if you could put up Bobby's Slide 4. And, I know it's going to take a little bit because I'll give you a little introduction to what I'm going to say.

I actually saw Bobby Pabalan's Slide 4 previously, and I was very intrigued by the fact that you divided it into four regimes, dry, seepage, all the way down to seepage plus condensation, and identified dry as greater than 105 degrees C. And, I guess the question that I have for you is are you familiar with the results of a large block experiment that was completed in about 1997? And, the reason I say that, you don't have to answer, I'll tell you what my story is.

The large block experiment was a very large block of volcanic tuff that was carved about two meters on a side with four or five heaters that were put in the base, and unfortunately, or perhaps fortunately, they forgot that it rains in the desert, and, so, at one point when the temperatures were greater than the boiling point of water, on the order of 100 to 135 degrees C. We had a very large rainfall event, and lo and behold, all of the thermocouples in the region near the heaters that were greater than 135 degrees C., and one data acquisition time step, homogenized to 96 degrees C., which tells me that there are events that there overcome the seepage threshold.

And, so, I understand that these are calculations and that the seepage threshold is probably based on what would be considered a steady state event, but would there be a possibility for transient events, based on the data that I just showed you, to basically drop that threshold and actually overcome the possibility if it's going to be dry at greater than 105 degrees C.?

PABALAN: Roberto Pabalan, CNWRA. Yes, actually, the value of 105 is not meant to indicate the absence of seepage water. As you can imagine, as you increase the temperature, it requires much more flow to pierce this voiding isotherm. So, this is really only--one can say that there is a spectrum of temperature at which seepage can occur either by focused processes or preferred flow paths. The higher the temperature, the lower the probability that you will have seepage water. So, 105 degrees is not meant to indicate an absolute value.

BULLEN: Bullen, Board. Thank you.

And, actually, when we heard this morning about seismic events with low probability, high consequence events, I think that we have not necessarily a low probability, but a sporadic probability that you're going to have a high influx that could indeed overpower any boiling isotherm that you've identified. And, then, you've got probably the worst of all conditions. You've got hot and wet, and that's not the conditions that you want.

I'd also like to go to just Figure 13 on the same slide. Tim, do you want to comment?

MCCARTIN: Yes, just follow up a little bit on that. Certainly in our performance assessment, we've looked at, you know, there's going to be variations in infiltration rates, and the one thing we do consider is there should be some correlation, that if you get a lot of dripping, a lot of water everywhere in the repository, it's going to be of a small volume. As you get to limited number of dripping locations, you could have larger amounts of water. But, I don't know if you're suggesting a lot of water to a lot of places in the repository. It would be more limited as you increase if you get a focused flow, for example, it would be limited areas.

BULLEN: Bullen, Board. I would tend to agree. But, the problem is it's focused at an area where you're going to have a very aggressive environment and may lead to package failure or drip shield failure. Can I see Slide 13 just for a second?

The only other comment I'd like to make--keep going all the way down, I guess. It's his summary. Actually, right here, that last one, environment four. If environment four has limited potential for enhanced corrosion in drip shields and waste packages, why wouldn't we always want to be in environment four?

PABALAN: I will defer--

BULLEN: You don't have to answer that one. That's a Dan Bullen question and I defer.

Ron Latanision, David Diodato, and then we're going to break. Okay, I'm sorry, I've got to cut you off.

LATANISION: You didn't give him a chance to answer, Dan.

BULLEN: I know.

LATANISION: Latanision, Board.

I'd like to turn to Slide 6 of Darrell Dunn's talk. I think this slide on the right is a particularly interesting and instructive one, if I understand it correctly, and I want to make sure by some questions here that I do understand it.

What I read that data to say is that in concentrated brines, at temperatures as low as 60 degrees Centigrade, there is evidence of crevice corrosion.

DUNN: That's correct.

LATANISION: And, moreover, if you have thermally aged or welded structures, you see an even greater susceptibility over the same range of compositions and temperature.

DUNN: Let me go back to the first question. The first question was focused only on mill-annealed material, or welded material? That's either thermally aged or welded in solution annealed for the 60 C. The mill-annealed, the lowest temperature shown there is 80 C.

LATANISION: Right. Okay. And, what about the 95 degrees thermally--welded and solution annealed, and then you have thermally aged, okay. I see.

But, is it your comment, though, that you feel that these data--let's focus on the first point. You're seeing evidence of localized crevice corrosion at temperatures as low as 60 degrees Centigrade. Your comment in response to Dave, and in your text, is that your sense is that the natural, the inhibitors that are naturally present, the nitrates, for example, that are naturally present in the repository would be sufficient to inhibit these problems. So, I'm wondering about the practical implications. From your perspective, are you prepared to make a judgment on viability of the waste package in the repository environments based on the data you have available to you? Do you feel comfortable making judgments about the stability of the waste packages?

MCCARTIN: Well, as I pointed out, we are not making any judgments here. We will make a finding based on our licensing review. It will be based on the information the DOE presents in their license application.

What we're showing and talking about today is in getting prepared for review. We are developing our understanding. We certainly bring, as any analyst brings to a problem, their understanding of the problem, and we will bring our understanding to it. But, our review will focus on what is DOE telling us, and are they supporting what they're saying. And, that judgment will come during our licensing review.

LATANISION: I appreciate that. The point I want to follow up on is the importance of the issue of taking the position that the natural ambient provides a sufficient inhibitor population, as I understand the data that we're looking at, to actually provide remediation or protection from the point of view of crevice corrosion. That's a pretty important statement, and I think I'd like to hear perhaps from some of the other folks in the audience on that as well. But, I just want to make sure I have the correct perception of what you folks are saying.

MCCARTIN: Certainly. And, what we do in getting ready is looking at things not only that are beneficial, but deleterious to repository performance to get a sense of if DOE is going to claim certain things as beneficial, have we looked at certain processes, and you're right, some of the evidence points to that some of the inhibitors will be beneficial.

Likewise, you know, we look at retardation factors, absorption of radionuclides in the geosphere. There's a lot of processes. Some are good, some are deleterious.

LATANISION: Thank you.

BULLEN: Okay, I know Thure has a question and David has a question, and I haven't asked anybody from the audience. So, let me ask a couple questions. Thure, do you have a burning question that you can't live without, or do you really want to know?

CERLING: Just a short--

BULLEN: A short question from Thure, and then I'm going to accept one from the audience if it's a really important one.

CERLING: So, Roberto Pabalan's Slide 9. Okay, in this slide, you show an area where you have these calcium chloride brines and they seem to attract a lot of attention. Do you have a sense of what fraction of pore fluids in the mountain might be represented in that field?

PABALAN: Roberto Pabalan, CNWRA.

No, not at this time. Our analyses of the potential chemistry of seepage waters is still ongoing. So, we don't have any information yet with respect to the fraction or the probability of the different types of water types that can enter the drift.

BULLEN: At the risk of asking this question, anyone in the audience who would like to make one--Don Shettel, who's going to be up next, so you'd better watch what you say. Don, one quick question, and then we're going to take a break.

SHETTEL: Using this slide here, DOE makes an assumption that they can lump all the vadose zone waters together and thereby statistically, not chemically though, dilute the importance of the waters that are above the repository level, which I presume would be the calcium chloride waters. So, why hasn't the NRC concentrated on the most deleterious solutions, which would be the calcium magnesium chloride solutions?

DUNN: We have looked at calcium and magnesium chloride.

SHETTEL: Yes, but you've also looked at all the other waters that are below the repository, and are really not important.

DUNN: Well, are you speaking of corrosion tests? Because I showed some data--

SHETTEL: Deliquescence, corrosion, everything.

DUNN: Right. I showed some data with concentrated magnesium chloride, both uniform corrosion rates, and localized corrosion susceptibility. Some fairly high temperatures, I guess--

SHETTEL: They weren't really very concentrated, though.

DUNN: That's 8 molar chloride. That's pretty concentrated.

SHETTEL: I think you'll see more concentrated solutions later today.

BULLEN: Okay, we've seen a preview of coming attractions. Now, I'm going to take another chairman's prerogative. We're going to have a ten minute break. Count them, ten. Okay? The trumpets are going to sound at about 20 minutes to 4:00, and I'd like to ask the Team Nevada to come up and get set up at their station, so that we're ready to go, if they would.

(Whereupon, a brief recess was taken.)

BULLEN: Our next set of presentations--aren't you up here next?

STAEHLE: Do we sit up there?

BULLEN: If you would, please.

I need a few Board members. That's correct. Could I ask a couple Board members to at least come and take their seats, please? All I can do is ask. I have one. Okay. I have two. Okay.

Well, I'd like to thank the audience for their indulgence, and also to say that we're going to continue until we're done. So, we're going to allow another 60 minutes of presentation time for the team from the State of Nevada. The first presentation will be made by Don Shettel, and the second presentation by Roger Staehle.

Don, it's all yours.

SHETTEL: Thank you, Dan. I'm going to talk about the evolution of near-field environments, and I'm going to present some alternative models.

Next slide, please. The State of Nevada has an excellent inter-disciplinary team that works very well together. This includes chemists at Catholic University, engineers from Dominion Engineering, Roger Staehle, who's going to talk next. GMI has a staff. Maury Morgenstein is the project manager, and our fearless funder, Susan Lynch, supporting us from the State of Nevada.

Next slide, please. I'm going to start off with showing some very qualitative experiments on some rocks. We collected some samples from the tunnel, ESF, last summer, and we are in the process of coring these for some other work, but noticed some interesting things. These were cored under water for about an hour, and as soon as the excess water on the surface ran off and the surface dried, we noticed that the fractures are wet here. In this sample, you look at the core, you see some wetting of the fracture, whereas, the matrix is dry.

Most people think of water flow at Yucca Mountain, they think the matrix is going to embibe or suck up all the water, and I think these show something different. The fractures, in fact, if there's water available, the fractures will take the water.

Next slide, please. We also did some additional experiments, a thin slice of these cores to some PVC, and then putting some water, tens of cc's of water, put a little head on this, and tried to determine when the water comes through these samples. It turns out the water will emerge from the fractures in about an hour, or so. The matrix takes much longer, days, weeks. Some of the samples, the matrix never even got wet. And, this suggests to us at least that the time steps the DOE is using in their modeling may be way too long, and especially when you have important processes like flow in fractures.

Now, the reason we're interested in fracture flow, aside from the obvious, is in the next slide. One way the water is going to reach the engineered barrier system is through thermal seepage, and this is going to be primarily flow in fractures. DOE believes that there is a vaporization barrier here that keeps the rock dry for a very long period of time. They also assume that this occurs at 96 degrees, which is the boiling point for pure water. They don't consider that the waters can be concentrated in the rock above the drifts and, therefore, you get an elevated boiling point.

When you elevate the boiling point, the vaporization barrier doesn't mean so much, and you get a higher probability of more concentrated solutions reaching the drift. And, this is illustrated in a diagram of Hele-Shaw Cell. Liquid water above, the hot drips down below, you have gravitational instability here, and you can have fingering of water through the fractures, even if you consider this is one fracture, even along the fracture. So, channelization in the fractures.

DOE takes a non-conservative approach, and they have many papers where they look at fingering and flow in fractures, but it's always with essentially distilled or dilute water. They don't consider any concentration, significant concentration of water that might flow at a temperature above the boiling point of pure water.

So, they fail to consider boiling point elevation, and the wall rocks are going to be above boiling for, depending on location, for a fairly long period of time.

In the next slide, we'll see that I--we believe it is possible to concentrate solutions to some extent above the emplacement drifts, looking at a cross-section of a drift here with a canister, when the rocks get hot and you get some initial boiling, and you can have a refluxing zone. You have boiling water, steam rises, condenses and comes back down. You have some input from percolation above. But, it's also possible to lose some condensate off to the sides, both sides here, and, therefore, you have the potential to concentrate water above the drifts.

Looking long-ways along the drifts, there are at least ten designs for waste packages, some are big, some are small. The heat output of these are going to vary depending on burn-up rate, storage, ventilation, and all those kinds of factors. DOE's isothermal boiling line, they would have you believe that the average for the entire drift is a constant distance above the drift. But, in fact, some waste packages may be hotter than others, and this so-called boiling isotherm may vary its distance, and again, you could have concentration from along the drift coming into a thermal load here with the possibility you've decreased the distance for thermal seepage here on some of these things.

Next slide, please. One of the major points that many people may not think about when they think about DOE's description of the chemical divide and everything, is that in their binning techniques, they classify all the vadose zone pore waters, they're above the water table, is that they believe the magnesium is removed, and that's why so much attention has been paid to calcium chloride brines and calcium chloride nitrate brines. We believe magnesium is removed as Sepiolite, which is a changed silicate, and this assumption began essentially with Garrels and McKenzie (phonetic) in 1967, evaporation of lakes and streams in the Sierra Nevada. Hardy and Oyster (phonetic) continue that assumption, evaporation of lakes.

But, if you look at the experimental data, and waters that are relevant to the repository, which means UZ porewaters that involve the repository level, in other words, the calcium chloride sulfate brines, Catholic has not found any magnesium silicates experimentally, and we have a long list of ones that they've looked for.

Rosenberg, 2001, a much discussed paper, found only smectite in an amount they didn't specify, and with some powdered tuff added. There is also a large temperature difference between these two sets of experiments necessarily, and Catholic has also added some tuff to their experiments. But, the point is no Sepiolite or essential other magnesium minerals has really been found in any quantity. We can only conclude that this really is an artifact of geochemical modeling and it may not occur in real life. On the other hand, is what you actually get is calcium, removal by precipitation of calcite, Gypsum, and Anhydride.

Next slide, please? There's been a lot of talk about deliquescence. I'm not going to spend a lot of time on that here, but to say that DOE has taken a non-conservative approach to start with, considering simple binary salt pairs. The Center has shown that mixed salts have a lower deliquescence, and what they have really failed to consider are these ternary systems, and even a quaternary system, calcium, magnesium, chloride, nitrate. These mixed salts have lower mutual deliquescence relative humidity. And, this is a conservative approach that they should have taken, versus this non-conservative best case scenario, one might say, that has been taken by DOE.

And, I have a little diagram here at 130 degrees, for calcium chloride magnesium chloride and water, with tachyhydrite actually is in the center here.

Next slide, please. Now, to consider the temperate of all the waste packages taken from Technical Basis Document Number 5, we believe that salts can develop as the temperature is increasing towards the thermal peak due to evaporative and thermal concentration, or thermal seepage, as DOE likes to call it. But above 160 degrees, the magnesium chloride hydrates can be composed to yield hydrochloric acid gas, and the removal of this is the driving force interaction.

As you come back down, you can get more thermal concentration. You certainly have boiling point elevation from these concentrated solutions. If they get concentrated enough, they are essentially molten hydrated salts. And, you also have deliquescence. Intermittent seepage on here is a very important factor as far as corrosion goes. Wet/dry cycling enhances the corrosive effect of the brines.

Next slide, please? And, the model, therefore, that we have for possible near-field environment that we believe is certainly a possibility, we have the bowing and refluxing zone out here. This is kind of a graphic temperature scale from hot to cold here. We have fractures, lithophysae. We have refluxing here. We can have concentration of mixtures of porewater and infiltrate and a percolating water. Lithophysal cavities can represent spaces for the boiling and mixing of water. You may get initial precipitation of carbonates and sulfates out in the refluxing zone, thereby giving you a more concentrated solution that is capable of dripping on the canister. Once it hits the canister, and if it hits it in the right place, or not even, it can migrate and evolve by essentially open system, or a full type of geochemical modeling where you leave precipitates behind as the solution moves.

On the hot metal canister, precipitates separate from the solution, and you can end up with a final assemblage of hydrous magnesium nitrates, hydrous magnesium chlorides, and some minerals like tachyhydrite, which are not present in any DOE geochemical modeling program.

Next slide, please. The previous diagram, although it showed some fractures in the lithophysae, was a diagram, and if we look at a real picture of the lower lithophysal zone, these are 12 inch boreholes, this one in the ESF, this one in the ECRB, you can see the lithophysae are fairly abundant. These are connected by tubular structures which form early on when the ash was laid down and essentially connect to gas pockets, which are the lithophysae. So, there's a lot of possibility to collect and mix some boiled water in the lower lithophysal unit, which is where most of the repository is going to be.

Next slide, please. We could give a whole day's lecture on the chemistry of all this, but I'll try and summarize this in one slide here. I haven't talked about J-13 water, because that's below the repository and, therefore, not important. But, basically, when you evaporate it, the pH increases basically by driving off CO_2, and at higher temperatures, you may--and other phases, and also drive off CO_2, which increases the pH.

We're looking at unsaturated zone porewater above the repository level. Essentially, you're heating it with excess calcium, and you precipitate calcite. But, we have been criticized in the past perhaps for using one specific unsaturated zone water composition, but really the important thing is that calcium is greater than bicarbonate in this ratio, and, thereby, you lose all the bicarbonate, and you lose a lot of the calcium. Magnesium becomes an important cation, and these other ones that are a lot more soluble than carbonate or sulfate increase.

And, actually, I left a step out here. The acidic solutions that occur below 160 degrees are the magnesium calcium, magnesium nitrate hydrates. Above 160 degrees, you can get this thermal decomposition of magnesium calcium and magnesium nitrates. And, this is, actually, you see this as very low water composition, but when it decomposes, it gives off essentially an acidic gas.

Now, we're not saying that the environment that's possible on the EBS corresponds to boiling nitric acid. But from the manufacturer's manual on C-22, boiling 10 per cent HCL, they give a corrosion rate of 10 millimeters per year. This particular sample here is below the surface level for the boiling acid. It has shown some thinning, uniform corrosion at a rate of about 2 millimeters per year corrosion.

The sample with slightly less acid, so the part of the foil strip is exposed above the liquid. You see the acid vapors very rapidly decompose that, and we get about a 4 1/2 millimeter per year corrosion.

Now, I must repeat, we're not saying that we get this environment on the canisters from concentrated brines. However, we have gotten this type of corrosion rate from concentrated evaporated unsaturated zone porewater that comes from at and above the repository level. The points of this is that we can get rapid corrosion in the absence of nitrate.

Next slide, please. We've talked about thermal concentration of brines and boiling point elevation. We can get fingering of concentrated solutions in fractures, thereby increasing the probability and percentage of thermal seepage waters that might reach the drift on the EBS. We have mixed salt deliquescence, not so much from the dust that's on the canisters, but from the increased amount of thermal seepage water that we believe can reach the EBS. And, if these evaporated or concentrated solutions can reach the EBS before the thermal peak, then they can become, even after the thermal peak, get hydrated salts with thermal decomposition, with the evolution of acidic solutions and vapors. And, one of the most important aspects of this model is the wet-dry cycling or intermittent seepage. If you get some seepage on the canisters, and it evaporates to some extent, dries out, the addition of water to that can generate acid.

And, my final slide? We believe that the high temperature design for the repository is fatally flawed for the number of reasons that I've discussed, and that emplacement in the saturated zone would be much better, because that's essentially where DOE has tested their metals at. And, the saturated zone is also the much less complicated in terms of processes and modeling.

I think that's all I want to say right now. Thank you.

BULLEN: Thank you, Don. We're going to defer questions until after both presentations. So, Roger, Roger Staehle, you're up next.

STAEHLE: I'd like to start off someplace with the purpose. The central question that we're all considering here is really the integrity of the container. So, whatever we're thinking about has to be directed toward the integrity of the container, because that's the primary or virtually the only barrier to release of radioactivity.

Now, when we're thinking about the integrity of the container, the most important consideration and design is to define the environment on the surface of the container. Because without the definition of the environment on the surface of the container, you cannot run corrosion tests on any metal that are relevant. So, you can take a large amount of the corrosion work that's been done nominally in support of this program, and get rid of it, and you'd never miss it. And, the reason is because it's not founded on careful, thoughtful work having to do with the real chemistry on the surface.

Now, the real chemistry on the surface of the container is dominated by the fact that the surface is hot, and it's that hot surface that is the primary consideration, not for reaction rate, but for concentration of species.

Now, the source of the environment is going to be primarily from the unsaturated zone, as Don mentioned. So, the environment on the surface of the container which is to contain the waste then is dominated by two important ideas. One is a hot surface, and the second is the chemistry of the unsaturated zone.

Now, this means also that the chemistry that has to be dealt with on the surface is a broad range of chemistry. There is no single chemistry here. Even if we take the water from the unsaturated zone, or the chemistry, we can concentrate that in many different ways and many different evolutions, and they will all produce different rates and morphologies of corrosion.

So, the first issue in thinking about the integrity of the container, which is our main concern, is to think about what the environment is on the surface of the container. Now, that's essentially been the objective of the Nevada program, and I'm going to show you some results from measuring corrosion in environments which are nominally representative of what's on the surface, but to say there's many more possible environments that need to be considered.

It's for this reason, the multiplicity and complexity, that having an adequate or permanent or defined definition of both the corrosion and the chemistry is a very difficult, if not impossible, job. It may, in fact, be unboundable.

I'd like to show you some of the work that we've been doing, and I'm going to run through some of it, because

I think some of it's well known. This compares the 1X saturated zone water from J-13 with the unsaturated water. You'll see there's some significant differences, mainly with respect to the ones that Don mentioned. You can read that for yourselves.

Next slide? And, we've approached this primarily by using this corrosion cell, which is a cell that has a cup here that has pure solution in it, with the bottom having a concentrated solution that results from evaporating. This is a fairly simple device, but it's directly geared to trying to understand what happens on the surface of a--on the hot surface of a container.

Next slide? And, these are the experiments that have been conducted to demonstrate that Step 1 is evaporating the solution, and that vaporization goes on until a certain pH is reached, on the order of 1.5, and then the solution, the deposit that's built up as a result of this evaporation then is transferred to this configuration to conduct the corrosion test.

Now, this procedure has all been worked out by Dr. Pulvirenti and Professor Barkatt at Catholic University. They have done some really fine work there. It's really impressive. So, the specimens I'm going to be talking about and the corrosion rates and morphologies come from this kind of an experiment where the solution has first been concentrated, and then the corrosion experiment is conducted in an environment that has these deposits, and also is in a dynamic equilibrium with the solution in this non-deposit case.

Next slide? We're also going to talk about a little bit of work that's been done in a condensed Erlenmeyer System, where we put various chemicals in the flask and measure their corrosion behavior.

Next slide? Now, this corrosion cell that has been developed I think applies pretty directly to the reality of what's happening on the container. You've got heat on the inside, heat here. We have on the top, we've got deposit, we have porewater, or maybe other sources of water that come from the UZ chemistry. And, so, we're looking at the hot surface either as a paste like deposit, or as a liquid that would be in some kind of deposit on the surface. There are also crevices at these support locations, which are of some interest, but I think this is the primary concern that we're addressing. So, this is the relationship between the corrosion cell and the container.

Next slide? The specimens we've been using, and when I say we, I just want to emphasize this is not my work, but is Dr. Pulvirenti's, we used a foil, which gives us a high surface area, a U-bend, which gives us stress, a disk, which provides a thicker material, and also a coupon of the same thickness in the soxhlet.

Next slide. Now, in examining these specimens that have been exposed to a variety of environments, so far, we've identified three main modes of corrosion. The first mode is a terrace-ledge-kink dissolution, happens mainly in hydrochloric acids, and it tells us there's virtually no passive film on the surface. And, we'll talk about that in detail. The second is a continuous localized corrosion with re-nucleation. You develop some corrosion, maybe like a baseball, re-nucleates, re-nucleates, and re-nucleates, and this gives you a way of drilling a hole through the material.

The third type is a, or the third morphology is the same thing, but initiated at grain boundaries, and you get the same kind of penetration, but dominated by the grain boundaries. We've actually observed one case of stress corrosion cracking, but only one, and I'm not so sure that's a dominant pattern in these specimens. But, those are the four morphologies that have been observed on a set of specimens we've examined so far.

Next slide. For those of you unfamiliar with this idea, metals with no passive films can dissolve in two ways, either in an astructural way and the metal just dissolves so rapidly and the over-potential so high, that it just dissolves without attention to the structure. If the dissolution is a little bit more orderly, you essentially lose atoms by dissolving from kink sites migrating onto the terraces, and desorption is an ion after it loses electrons. This is the terrace-ledge-kink model. It shows you a lot about whether a film is present or not.

Next slide. This is the continuous growth by re-nucleation. It can be non-structural. It does not depend on boundaries or just dissolves the material. And, it is an initial event, it re-nucleates at the bottom, then continues its growth by re-nucleation, and seems not to stifle itself.

Next slide. The third variation of that theme is for this re-nucleation process to be dominated by grain boundaries.

Next slide. Now, the environments that we're talking about in these corrosion cell, there's a paste at the bottom. It's very difficult to analyze because it's hydroscopic. It's very heterogeneous. It is continuously wetted by the dump of water or dump of solution from the soxhlet. X-ray signals show this dominated by sodium chloride and calcium sulfate. But these appear not to be dominating of the corrosion process. It appears that what's dominating the corrosion process is essentially an interstitial fluid of nitric acid and hydrochloric acid.

The wet paste with the calcium sulfate and sodium chloride, together with the two acids, gives a pH of about 2.3. Without the liquid, the pH is about 8. There is also a liquid at the bottom of some of the flasks. This boils at about 145 to 150 degrees Centigrade, and, therefore, it's obvious that it's a mixture at least of concentrated acids. The pH of this fluid is on the order of pH 0, possibly less. In the soxhlet, the specimens are totally emerged, and the temperature is near about 75 degrees Centigrade. There is some cycling.

Next slide. Now, to give you a sense of first morphology, the re-nucleation, this is an experiment from the corrosion cell with the foil, 150 Centigrade, and that's just the boiling temperature of the solution. Less than a five day test. The corrosion rate was greater than 3.7 millimeters per year. That's not microns, that's millimeters per year relative to a 20 millimeter wall thickness. It comes out to about a six year lifetime. And, you can see that it's astructural. It just simply goes right across the grains and twin boundaries.

Next slide. And these are various features here showing variations on the same theme. You can see that this re-nucleation doesn't seem to be gravitational. It moves in various directions.

Next slide. And, here's a picture of a broader specimen showing the penetration and the nature of the growth of these re-nucleated sites.

Next slide. And, still the same thing. Just, again, more of this re-nucleation. You can see that it looks like it's doing this internally, homogeneously, if you will, but obviously, it comes from some other sources.

Next slide. This is one stress corrosion event we saw. I'm not so sure that that's the general case, but I'm just reporting it as an observation. These foils are stress foils, that is, they're whole work.

Next slide. This is now the disk in the bottom of the corrosion cell. This was run for six months, but the corrosion rate was about the same, that is, the corrosion rate in six months, or over six months, was about the same as the corrosion rate for the foils for five days. So, it gives you some sense over this relatively short time admittedly that the corrosion rate doesn't slow down very much.

Next slide. This is just more of this same thing. This is a disk, the same disk, showing the local events as they move the frontier back. Same kind of process of re-nucleating events, pushing the corrosion forward.

Next slide. Same thing here, except this is now importantly no longer at the bottom, but it's in the soxhlet is fully emerged. There is no water line, and the corrosion rate here is 5 1/2 mls per year. That's 75 Centigrade, think about that, 75 Centigrade, 5 1/2 mls per year, no crevice.

Next slide. This is more of the same thing, just showing you that the mode here, the morphology, is this re-nucleation mode.

Next slide. Same thing, except on the U-bend, no stress corrosion cracking, six months, 145 Centigrade, 2.1 millimeters per year of this specimen, and I've corrected for the fact that the corrosion only comes from one side. Both sides corrode, and those of you who think about these things, know that, well, wait a minute, Roger, you forgot, you didn't divide by two, but I did, just so you know I was sort of on my toes.

Next slide. Now, let's see, this shows you general pictures of how these things propagate locally. This is all this re-nucleation morphology. This is a six month test, the same as the previous one.

Next slide. Now, turning to a different environment. We've so far just been discussing the paste environment, the soxhlet and equilibrium with the paste, and some different thicknesses of specimens. Now, one of the things that's become obvious to us is that we're not talking about a single environment. We're talking about many environments. So, we're broadening the chemistries that we're examining, because it's pretty clear that there are a variety of chemistries in these deposits. So, we're exploring, for example, ferric chloride. We're exploring HCL, and we will explore more different kinds of environments, because it's clear that there is a broad set of environments which are aggressive in this canister type heated surface.

So, this is ferric chloride. It has the same pattern, the re-nucleation process. This was for an experiment that was six days, corrosion rate greater than 1.6 millimeters per year.

Next slide. Now, this is the same experiment now looking at it in some detail, and I'm not so sure whether this was general or localized, but it's generally localized. That may work for some of you. But, the point is this is very aggressive, and re-nucleates and re-nucleates and re-nucleates. This is a very aggressive, non-stifling corrosion process.

Next slide. And, this is the same environment, the same conditions, is accompanied by this mode of grain boundary penetration. These are preferentially nucleating and propagating corrosion processes of grain boundaries. So, it appears that there is both a structural response and an astructural response to how the corrosion propagates. It's not clear to me what the relative importance of the two is. It's clear that the whole corrosion in the ferric chloride is quite aggressive.

You all know this I'm sure, it's oxidizing, the ferrous, ferric couples about .7 volts at room temperature, whereas the nitrate, nitrite equilibrium is about 1.1 volts, or so. So, these are somewhat similar in their oxidizing capabilities of the nitrates.

Next slide. Ferric chloride again. This is another grain boundary thing. I've already shown you that.

Next slide. This is a different kind of geometry, where the process is essentially taken off and drilled holes in the foil, and you'll have to admit that this is almost like a perfect circle, not quite. I don't know what we call this kind of corrosion, but there's no question it's aggressive. And, there's no question that it has some reason of persimetry in this, which may be just simply a variation of these holes getting bigger, but it's not clear quite how that works. But, it's very clear that it's certainly aggressive, and non-stifling.

Next slide. This simply shows the same thing. These holes having eaten out various parts of the foil. This is all greater than 1.6 millimeters per year, experiment ran for six days. That's six days after the water hits it. It doesn't take long.

Next slide. Now, turning to a different environment, this is the third environment, this is hydrochloric acid. Again, this is just one of the components of the environments. This is a foil in the bottom of the Erlenmeyer, and it was bent foil, so it would stand straight up. And, I want to point out something here. This is the top of the foil, and this is the bottom of the foil. This is below the solution interface. This is above the solution interface. Here is the interface between the saturated vapor and the liquid. There's no waterline effect here, contrary to the idea of the crevice effect.

But, what does happen is that the accelerated corrosion is not occurring in the fluid, it's occurring in the region above the fluid. That's tells us something else about what's possible. Now, again, this needs exploring, but this region here is not in the solution, but is above the solution.

Next slide. Now, let me show you how this dissolves. The previous dissolution I spoke about was this structural/astructural nucleation and re-nucleation. This is the terrace-ledge-kink process. You can see very clearly here that this is the upper surface now, you can see very clearly this is a terrace, these are ledges, and the dissolution is occurring by a clearly terrace-ledge-kink process.

Next slide. This is the bottom, and the rate is about half the rate on the top, still significant, but maybe only 2 millimeters a year. But, again, the same dissolution behavior, a very clearly terrace-ledge-kink. These are almost classic. This is textbook stuff. But, this is how it dissolves. It also tells you that this alloy is not passivating. This is virtually an uncovered, unpassivated material dissolving like this.

Next slide. This is just aesthetics. After you look at something like this, you can't--but you have to give yourself a while to look at it before you go onto something else.

Next slide. And, this is a fully emersed specimen, where the fluid was covering the foil, and the get the same result, but it corrodes at the rate of the foil beneath the waterline that I showed previously. Again, a terrace-ledge-kink dissolution.

Next slide. Now, about the morphology then of corrosion, the corrosion observed in these SEM examines, these are different morphologies, even within a single morphology, i.e. like the ferric chloride versus the concentrated UZ tap water. The different morphologies seem to result from various effects of absorbed ions on the velocity of recession. The mix of anions in solutions should be expected to exert different influences on the shapes. I'll give you an example of this from the work of Bill Cullen, who is now at the NRC. And, so, for a given overall corrosion, you may get quite different morphologies and quite different local penetration rates.

Let me show you the next slide. Now, these are data from Alloy 600 and 690, at a somewhat higher temperature. This work, I think, was done at 315 Centigrade. These are general corrosion rates. In the nuclear business, they call general corrosion wastage. I never understood that, but they have some peculiar views. But, anyway, this is general corrosion versus pH for a solution that's a 1 molar solution. This is all sulfate, and this is all chloride. Now, what's the inhibitive ion here? The inhibitive ion is chloride. It's not sulfate. And, that tells you this concept of which ion is slowing things down is not a general concept, but is a local specific concept having to do with other factors than an inherent property of the ion.

So, what this tells you is that the chloride solution, 100 per cent chloride, reduces the corrosion rate, the general corrosion rate, about a factor of 100 over the range of pH 1 to 7. Let's look at this now again in work from Was, University of Michigan.

Next slide, please. Was has studied the acuity of the aspect ratio depth to width of pits, versus the chloride/ sulfate ratio, with the idea being that the chloride will give you an inhibited lateral dissolution, and what that does is as you increase the amount of the inhibiting ion, this is obviously probably affecting the terrace velocity, you most from a relatively wider pit to a narrow pit, and possibly to cracking. This again was a higher temperature, but the concept is the same. Depending on the mix of ions, you get different morphologies. And, that's what we're seeing in the previous slide. You go from a totally terrace-ledge-kink to a re-nucleating set of baseballs.

Next slide. Now, in summary then, the modes and rates of corrosion for the foil at 150 centigrade, greater than 3.7 millimeter per year; for the foil disk at 145 centigrade, about 1.9 millimeters per year; for the soxhlet, which has no paste, above the solution, and no crevice, the corrosion rate was 5.5 millimeters. Just imagine there's two m's there. This is a high corrosion rate, but it's not meters. Okay. Forgive the mistake there.

And, this is the U-bend again, 2.1 millimeters per year; the stress corrosion crack, which I say is not the general case, but I think you never know today's single observation may become a dominating thing later. The hydrochloric acid was clearly a terrace-ledge-kink process at 2 to 6 millimeters per year. The saturated ferric chloride gave us several different geometries, these circles and the very local attack, and the grain boundary attack. So, this is kind of where we are at the moment on morphologies.

Next slide, please. Now, there's some warnings here. One of the things I'm concerned about, I've lived through the nuclear power from 1957, and I know something about warnings, and I watched every experiment that was ever done on corrosion come true, even though the old gray heads in the beginning through, well, that will never happen. It did happen, and I could cite you chapter and verse if I could have until midnight.

But, the point is virtually every major corrosion finding and the alloys used in nuclear power, mainly with respect to steam generators, came true, and despite the fact that people said well, this isn't going to happen, this isn't going to happen. So, I'm saying this because I think there are warnings, they're already here, that we're not paying attention to.

Now what are they? So, there are warnings clearly that the corrosion of C-22 is inevitable and it's rapid. This idea that C-22 is a corrosion resistant material is just wrong. It may be corrosion resistant in a given environment. It's not corrosion resistant on the surface of a container with a concentrating environment. From unsaturated zone materials, it is not corrosion resistant.

A good paradigm can be found with Alloy 600. Alloy 600 has broadly failed, and this could easily have been prevented. Every mode of failure that was observed, there was a warning out there from reputable people doing good work.

Now, there are abundant warnings about the C-22, and some of these warnings are founded on data which is 15 years old. There's also abundant evidence that the Yucca Mountain site itself is not adequate. And, this comes from my geological colleagues.

The analogies of warning from the present nuclear industry are abundant and apply directly to whether or not the present design at Yucca Mountain is adequate. And, the answer is it is not.

Now, some of the warnings from experience of the water cooled reactor industry apply directly to the design and development Yucca Mountain. These should be carefully assessed, especially as they apply to heated surfaces.

Now, finally, the incapacity to inspect the Yucca Mountain containers requires assurances of reliable performance that are at a higher level than was ever used in nuclear power which inspects regularly every about two years.

Next slide. So, let me show you an example of a warning. These are data from 1960, actually '59, through 1985, looking at a form of localized corrosion of high nickel alloys in pure water, so-called low potential cracking. The industry calls this primary water stress corrosion cracking, but that's another dumb idea. So, the laboratory experience is Andre Coriou in France at CEA, identified in 1959, the occurrence of cracking of high nickel alloys in pure water.

The first failure in a plant occurred in 1965 at Agesta, in 1972 at Obrigheim, and then starting about 1978, a whole series of failures occurred. That's got to tell you something; that this experiment on this material in that environment should have told everybody that something was going to happen that did. Coriou was vindicated numerous times, and there were ultimately many laboratory experiments that vindicated Coriou. So, this is an example of a warning.

Now, let me show you an example of a result.

Next slide, please. Some of you, I don't know how many of you in this room know about the so-called Davis-Besse problem. This is not a song and dance team. This is a name of a reactor in Northern Ohio, where the top of the vessel corroded completely through between inspections, and probably before that. Why did that happen? Well, first of all, there was a weld here at the control rod drive housing, and this weld created local stresses, which produced sufficient stresses to cause stress corrosion cracking here. And, the velocity of the stress corrosion cracking was about to penetrate four-tenths of an inch in about 20 years. That was based on existing data.

Then, when this perforated, the water came through, and in the water of a primary system, there is boric acid, and the boric acid in the nuclear plant is 1000 ppm, 2000 ppm, but when it evaporates, it's concentrated. And, when it's concentrated, it is very corrosive. And, so, the rate of corrosion here from this borated water was about three inches per year.

Now, why did this happen? There was a lot of discussion here, and I'm not going to debate all this, but the point I wanted to make from a purely technical point of view is the rate of corrosion in carbon steel at that pH was already well known in 1946 from work by Pourbaix, who showed that the corrosion rate of steel at room temperature at that pH would go at that rate. That's a warning. And, this is what happened. This could easily have blown up. Fortunately, the stainless steel clad held, and it didn't blow up, and the Davis-Besse people found this, and of course have fixed it.

But, the point I wanted to make here is that you see the data from the stress corrosion cracking of the high nickel alloy was known in 1959, and here was a result that occurred in 2002, which could easily have had a disastrous implication, even with inspection, incidentally, and somehow, nobody got the point.

And, my concern is we are in the same situation today. We ought to learn something from these kinds of experiences about warnings and inevitabilities.

Next slide. So, the "knowns" about corrosion of C-22, the deposits which are reasonably expected can produce corrosive environments. Relatively simple experiments can model reasonably expected conditions. However, the inherent complexities prevent precise modeling. You've got to bound these things if you can.

A range of chemistries from concentrating the pore water can be expected, including nitric acid, hydrochloric, hydrofluoric, and others. The corrosion produced by these environments can proceed at rates of 1 to 6 millimeters per years compared to a 20 millimeter thick C-22 wall. That looks to me like about three years of lifetime at worst case. And, then, of course, you've got to go through a backup, but that's not a big challenge.

The temperatures over which these high corrosion rates can occur, as we just saw, are in the range of 70 to 150 degrees centigrade. That's a pretty broad range. It's low temperature. And, you know, the activation energy for most of these kinds of reactions is in the range of 5 to 10 kilocalories. What that tells you operationally is there's not a big difference in rates inherently from 70 to 150 centigrade, there's a difference. The big difference of the temperature is with respect to concentration and not with respect to reaction rate.

There is no evidence from the work we've done so far that the corrosion is self-stifling. The corrosion that we observed proceeds without stress. This is not a stress corrosion cracking problem. This is a pure dissolution corrosion problem. And, accelerated corrosion is observed in the paste, in the liquid layer, in the saturated vapor, and in the liquid formed from refluxing, a whole range of environments.

Next slide, please. So, what are the facts that are relevant to this corrosion-related integrity of the container. First of all, there is water in the unsaturated zone on the order of 80 liters per cubic meter. The rock is extensively fractured, which is a preferred pathway. The surface temperatures, depending on the deposit and how much of the circumference is covered, will be in the range of 90 to 250 centigrade.

The porewater is concentrated with acidic solutions on hot surfaces. There will be increasingly thick and increasingly circumferential deposits. The UZ porewater produces acidic species when concentrated. We've demonstrated that at Catholic University. So, we can obtain this array of non-stifled corrosion of multiple modes without stress, with rates 1 to 6 millimeters per year compared to the--and non-stifling rates, these rates compared with the C-22 rate.

The porous rock is a minimal barrier to release of radioactivity, no matter how you cut it. And, the saturated zone, which has been studied extensively, produces alkaline species when concentrated by heat, but this work is all irrelevant to the integrity of the vessel.

Next slide. There are certain "inevitabilities" about this corrosion. C-22 sustains rapid corrosion in environments that can be reasonably expected to develop on heated surfaces. A significant amount of water is present in the unsaturated zone. The porewater contains chemical that produce acidic environments. Don mentioned that.

The extensively fractured rock above the containers provides easy access of porewater. The continued formation of deposits on containers will increase surface temperatures and accelerate concentration, as well as sequestering corrosive chemicals. Stress is not necessary for rapid penetration. Other alloys beneath the C-22, like stainless and zircaloy, are unlikely to provide significant barriers. Penetrating the C-22 will be the slow step. And, the lack of capacity to inspect containers over time exacerbates the seriousness of the present state of inevitability.

Next slide. Now, my primary conclusions then are the following. There are now ample and compelling evidence that the container of the present design in the present location and the present materials will not work. Further, the "band-aids" that have been used cannot reliably provide a significant assurance of satisfactory performance.

Second, penetration of the corrosive chemicals that can reasonably be expected to accumulate on the surface could perforate to the fuel as early as ten years, and is especially accelerated during the thermal pulse. We're not talking about 10,000 years. We're only talking about tens of years, or less.

There are no reliable barriers that have been identified to prevent the release of radioactivity to the atmosphere through the porous saturated zone.

While the possibility of such a failure is clear, the detailed avenues and rates for such failures cannot be readily bounded. Thinking about bounding this, I'm reminded of the fact that some of my best friends have worked 40 years to figure out what the predicted corrosion rate in steam generators, with a well defined water environment, in a well defined geometry, and well defined metals, and nobody can still make a prediction. And, if we think we can bound and predict simply the conditions on the surface of these containers, which is virtually an unbounded chemical situation, I think we need to have some revision of our thinking process. I said that politely.

The principal factors that are critical to lack of integrity have been known for long times: The importance of hot surfaces was first identified in the late 1980s. This was for these vessels. The porosity of the saturated zone was known at the same time. And, the fact that C-22 could not sustain concentrated acids has been known for at least ten years.

Clear warnings that failures of the containers are inevitable are already available. However, quantifying these warnings is difficult in view of the complexity. This is a very complex problem to model and predict, except to bound it, and I'm not so sure about the bounding.

Now, I have two items of summary here in the next slide. My version of what this design looks like is a patch on patch, that ventilating, dry mountain, drip shield, lower residual stresses, corrosion resistant alloy, nine barriers, rock bolts. You know, this is all patches. There's nothing fundamentally high integrity about the present design.

Next slide. And, here we are sitting in the middle of all these possibilities, and I guess the question is what? Me worry?

Okay, Dan, I'm done.

BULLEN: I'd like to thank both Don and Roger for actually getting us closer to being on schedule, although I think my little clock is going to go off any second now. I would like to take questions from the Board first, and then the audience, and David will get one this time, I promise.

Dr. Cerling?

CERLING: I'll just start with the first question I asked the last speaker, which was--I'm Thure Cerling. First of all, how representative are the fluids that you chose as unsaturated fluid. And, then, following on to make sure that

I can ask the question I really wanted to ask, how representative is this to evaporate, this water, in the absence of silicates, when we know that acid metasomatism often neutralizes solution?

SHETTEL: Don Shettel for the State of Nevada.

Some of these experiments were conducted in the presence of silicates, powdered tuff. They did not show, as I recall, from the Catholic University people, and Abe can correct me if I'm wrong, but we did not see any significant effects of the silicates. And, that may be because in these concentrated solutions, there's just not enough water available, and the solubility of the silicates in such concentrated solutions may be really small. So, apparently there was no effect.

STAEHLE: There's another possible thought about your question, which is I think the idea of having a quote "representative solution" is probably not a useful idea. I think what you need to think about is at least a uniform set of solutions, where that set is probably someplace between 10 and 30, that we have a much more complex chemical situation here than I think we're prepared to admit, and certainly we need someplace to start, which we should have started ten years ago or fifteen years ago.

CERLING: One of my points is that as I go through the literature and look at all of the now hundreds of unsaturated zone chemistries that have been produced, this particular one is actually pretty uncommon, and many of them are much more like the J-13 water.

SHETTEL: Well, I believe if you consider the location of those samples, the ones that are like J-13 are below the repository level. The ones that we are playing with and evaporating are essentially all at or above the repository level. So, in terms of spatial location, we're dealing with the right solutions and, therefore, by analogy, that means DOE is not dealing with the right solutions in their tests, sub-boiling, submersed tests, which are done in essentially J-13, which is groundwater.

BULLEN: Other questions from the Board? Dr. Latanisioon?

LATANISION: Latanision, Board.

I think the operative issue, and I'm addressing this to Don Shettel, is the evolution of environments that are reasonably expected. And, that's what Thure was addressing. But, you show that only in one slide the basis for the chemistry that these tests were performed in. I'd like a little elaboration on that. Can you walk me a little more slowly through the evolution of these very, very aggressive environments?

SHETTEL: Okay. Well, first of all, I don't think we're dealing with just one chemistry here. There's a range of chemistries that you could conjure up, and the main characteristic of this is that calcium to bicarbonate ratio is greater than that ratio I showed 1 to 2, just a molar ratio. That essentially drops out the bicarbonate and allows magnesium to concentrate relative to calcium.

LATANISION: I don't know what number it would be. 12. Okay, Don, I'm sorry.

SHETTEL: That's a very summarized slide. The unsaturated zone waters I'm talking about are at and above the repository level. The ones below are essentially like J-13. But, above, you get calcium greater than bicarbonate. Therefore, you're dropping out calcite. Sulfate is additionally removed as precipitation of gypsum or anhydride, and that allows the magnesium, chlorides and nitrates to concentrate.

LATANISION: Well, let me ask this differently. Is there an exposition on this question on the evolution of the chemistry you can provide me?

SHETTEL: Yes, the talk essentially I gave a year ago January, where I first showed that you have this division in the water chemistry between porewaters that are above the repository and those that are below the repository.

LATANISION: I just don't remember the detail. What you comment on is that the detail is in that talk?

SHETTEL: Yes, I provide a lot of the data on that diagram, and show that you basically, I'm not saying that the fields don't overlap, but perched water and groundwater are essentially the same. And, as you get closer to the water table, you become more J-13 like. Above the repository, you get more of the calcium, chloride, sulfate type of water.

LATANISION: I'll buy that. But, I'm looking for the concentration process on the chemistry, that leads to the concentration into the acids that you are testing.

SHETTEL: Well, the concentration process involves an open system type, where you essentially remove the precipitates, a flow through type system for those geochemical modelers. But, you're essentially removing the precipitates as you evaporate, and, so, you don't have early minerals available that might neutralize.

BULLEN: We're back. Thank you. Don, I'm sorry to interrupt. Go ahead.

SHETTEL: Okay. So, you have open system evaporation, where the mineral precipitates are removed essentially as they form. You get to these acidic concentrates. If you did all this in a beaker where you could keep all the minerals that precipitated in an equilibrium with the solution all the way along the process, you wouldn't get this. But, since we have a very hot repository and hot metal surfaces, we're going to have hot rocks above that, and in addition, a thermal gradient, I think there's a large possibility that you can achieve this kind of essentially fractional crystallization type process as you go along. As the water percolates down, you lose the less soluble minerals, until you get down to the most concentrated waters, which precipitate out the lease soluble phases, which are the magnesium chloride hydrates, and the magnesium nitrates, nitrate hydrates.

LATANISION: Latanision, Board.

My point is simply the photographs that Roger showed are obviously pretty dramatic photographs, and the operative is can you demonstrate that this is, I think the language that was used is reasonably expected environment, and I guess I'm going to reserve judgment on that until I look at, once again, at the text of your presentation for January.

SHETTEL: One of the keys to this is pre-concentrating the water in the rocks above the drift. DOE doesn't admit to this. They don't think it's going to happen. They like their vaporization better to stay at 96 degrees. I don't think that's the most conservative assumption you can make. In fact, that may be the most optimistic, non-conservative assumption that you could make. You're going to have boiling above the repository for, depending on location in the repository, tens to hundreds of years, and I think there's ample opportunity there to pre-concentrate these solutions before they penetrate through and drip on the EBS. Once they reach the hot metal surfaces, they can further concentrate to develop the type of acidic solutions that Roger showed the corrosive results for.

BULLEN: Thure Cerling, then David Diodato, and then I'll take a question, and I'll ask if the audience has any.

CERLING: I think it's a very important point, your model for this evaporative concentration, and that's one of the things where I'm concerned, is that this water that you're using is evaporated in the absence of alumino silicates, such as tuff, and if it's going to be evaporating up in the zone above the repository, then presumably, the opportunity for water/rock interaction, which could neutralize the acidic.

SHETTEL: Except that you're dealing with a lot of this can occur in the fractures, which may or may not be coded, so that the surface area for interaction with alumino silicates is much reduced, compared to if you're just doing this in a very porous matrix rock, which it isn't.

CERLING: Right, but above my point would be that there would still be far more silicate available than what's done in a beaker where you're not allowing--

SHETTEL: These experiments have been done with and without silicate, and the silica precipitates out fairly early, actually. So, you reach saturation with silica fairly early in the evaporative process.

BULLEN: Maury, do you want to say a brief comment, please?

MORGENSTEIN: Just for clarification.

BULLEN: Identify yourself, please, Maury.

MORGENSTEIN: Maury Morgenstein, GMI.

If you're above the drift in the rock fractures and you're pre-concentrating, what you do is you drop out sodium chloride and you drop out gypsum, calcium sulfate, and any reactions that might take place in your aqueous phase, even if you neutralize that down to zero, 7 pH, or even 8, wouldn't make much difference, because as you drip that liquid back into the repository, you will start to concentrate at chloride nitrate phases. And, it's the pH of that liquid as it evaporates on a metal surface that actually counts.

So, in the presence of tuff dust on that surface, what we see happening is just residual sulfates and carbonates and chlorides that are left usually cover up dust and remove it from reaction. If you didn't remove it from reaction, your observations are probably correct.

SHETTEL: Well, another point then, this is Don Shettel, another point to make is that if you look at some of the evaporation curves from Catholic University, the pH does not get very acidic, and, so, you're down to about the last 5 per cent of the solution. So, if that evaporation occurs on the canister, that's where you're going to get the very acidic conditions, not up in the rock. We're just looking at the rock to pre-concentrate the porewater.

BULLEN: David Diodato has been very patient. One quick question.

DIODATO: Diodato, Staff.

My questions all relate to water/rock interaction.

BULLEN: Okay, I'll ask if there are any questions from the audience before we proceed. Okay, Bo was first, and then David.

BODVARSSON: Bo Bodvarsson, Lawrence Berkeley Lab.

Just a quick comment regarding this concentration of the water above the drifts, and we're going to be talking a lot about this tomorrow, so I'll make it very brief.

The concentrations will actually be diluted and not concentrated, for the following reason. When you boil off the water due to heat, it condenses above the drifts, a lot of it sheds off, and there is rock/water interaction, so you have more and more of condensate, with very little new chemicals in it, because the water doesn't have time to pick up a lot of minerals from the rock, because the permeability of the fractures is so high that most of it will shed and not concentrate. So, I think that's one problem in this, and I think Carl Steefel will explain this a little bit more tomorrow.

SHETTEL: Don Shettel. I'd like to respond to that.

BULLEN: Go ahead.

SHETTEL: That's been DOE's standard argument for saying that water does not concentrate above the drift. But, in fact, if some of the condensate is lost over the side over time, I believe you could concentrate it, and I don't see that you can say for sure, since you've said last year with the billions of fractures, that you don't know which ones carry water, and condensate is water, therefore, you can't predict I think with any degree of certainty how much of the condensate is going to escape over the side of the drifts, and whether or not that amount is more or less than the amount of percolation that's coming down.

BULLEN: David Shoesmith?

SHOESMITH: David Shoesmith, Bechtel, consultant to Bechtel, rather.

Roger, I wanted to ask you what you thought the significance of the second re-nucleation process was. Let's use the sense of intermittence in the process, in that it starts, it doesn't want to go, it starts again.

STAEHLE: Well, I don't know that the answer I'd give you was any better than anything else where you all imagine, but what's obvious is that it slows down laterally and stops, but it continue to nucleate at the bottom.

SHOESMITH: So, this is a material that would stop.

STAEHLE: Pardon?

SHOESMITH: This is a material that can stop.

STAEHLE: Well, it obviously from the experiments, it just continues to propagate lateral--I'm sorry.

SHOESMITH: That is my point.

STAEHLE: Yes.

SHOESMITH: My point is that it has to keep--this is like the inverse of crystal nucleation and growth. If you could nucleate many times, but you won't grow if it will not grow, and, therefore, you keep on nucleating. We've seen this morphology a few other times. Dick McDonald has done this at plus one volt to try and drive the pit, and he sees those little scallop pits, which are all dying, and when you analyze them in that situation, one volt is very, very aggressive electrochemically. They will not grow.

And, I think if my memory serves me correctly, you see the same morphology inside the electrochemically driven crevices in some of the Alloy 22 specimens at Lawrence Livermore, and that you often see that, geometry suggesting that this is an alloy that unless you overload it electrochemically, or with acidity, would in fact stop propagating.

STAEHLE: Well, I think we know that C-22 is corrosion resistant in many applications. I saw an argument here.

SHOESMITH: This is an active condition. This is an active situation where it's trying its best to repassivate. It's either being overloaded electrochemically, or it's being overloaded by acidity.

STAEHLE: Well, it's--I don't know whether it's being overloaded. It's responding to the environment that's there.

SHOESMITH: But, it does have the capacity to stop propagating.

STAEHLE: And, it could be worse. I mean, we're at some kind of a boundary in here where it's clear that it doesn't stop and it continues to re-nucleate.

SHOESMITH: Well, my issue here is not whether or not this is the correct environment to test it in. It is that we have an alloy which is showing all the features that you would expect for material that you can force it to start, but it really does not want to propagate, except under extremely severe conditions. That's my only point.

STAEHLE: Well, I guess this is sort of a qualitative argument then.

SHOESMITH: That's still a point, though.

STAEHLE: It sounds like a good point.

BULLEN: Mick Apted, do you want to take the last questions from the audience, please?

APTED: Mick Apted, consultant to EPRI.

These two presentations side by side I think form an important link between the chemistry on this group and the corrosion results you present. And, I think I really like this idea that Don has put up. It's very hot, he says 100 years were above boiling into the rock, we get a dry-out zone, and this broad band condensation zone.

My problem is when I come over to this set of apparatus, which is claiming to sort of simulate I think this situation, we certainly see the boiling here, the surface of the canister, and then I guess some sort of refluxing condition of solution, which is also maybe some later cooler part of the canister history.

But, this condenser here, it would seem to me if this condenser were actually tuff, you might have had some experiments in which the subsequent corrosion results might have been meaningful. But, with simply just condensing the fluid phase here, you've really dropping out this very important potential set of reactions, and I think if we go back to Bobby Pabalan's presentation, we see that certainly in their modeling and their understanding of the system, that instead of a very broad range of chemistry you keep insisting on, Roger, that the type of chemistry that develops here and comes back in is actually rather restricted, and we don't get this sort of unbounded type of water. We actually find a very strongly buffered type of environment.

STAEHLE: I think that debating that at the moment is not worth it. It's an interesting comment, but--

SHETTEL: Don Shettel. I have a few comments on that.

First of all, this model here is DOE's model. I just took it as it is. I don't believe that the so-called vaporization barrier is fixed at 96 degrees, and as far as this condenser, that could be the titanium drip shield. So, you're not going to get a lot of buffering, as you think it might be condensing on the rock surfaces, or something. If it's condensing on the drip shield, you're not necessarily going to get any buffering.

BULLEN: Bullen, Board. I reserved the last question for myself.

Could we go to Don's Slide 9? I've got to have an equal opportunity question for every presenter here. So, we see this really aggressive environment above 96 degrees C. Is the environment any less aggressive below 96 degrees C. if you never go there?

SHETTEL: Well, thank you for asking that question. Don Shettel.

BULLEN: Well, I had to ask the question for each group, so it's the same question.

SHETTEL: It made me take one conclusion off of my last slide there, which said essentially that the low temperature operating mode isn't much better. The rates, I'll stick my neck out here and say we see the same type of things below boiling, the rates are somewhat slower, but the processes in general are still there. So, taking out my middle conclusion, that still leaves the saturated zone the best environment, not necessarily Yucca Mountain, because you have other problems with earthquakes, volcanicity and a discharge to the surface of the earth.

BULLEN: Okay. Thank you, and I thank the group from the State of Nevada. And, I guess this is a forewarning of the question I'm probably going to ask the group from EPRI.

If I can ask them to come up and we'll continue, we're going to go for 60 minutes with their presentation, with 15 minutes for questioning. I apologize to the audience for the late time of day, but we're going to finish this out.

And, the presentation will actually begin with Dr. John Kessler, followed by Don Langmuir, Fraser King and Mick Apted.

Dr. Kessler, the podium is yours.

KESSLER: Thanks, Dan.

Well, thank you for being such a patient group. Let's hope we don't tax your patience too much, but we'll do our best to at least not go overtime, any more overtime.

I'd like to begin by acknowledging the presenters and additional authors. I'll talk about the additional presenters on one or two viewgraphs in. Randy Arthur, who's with us today, did some of the geochemical modeling. Matt

Kozak did a bit of the TSPA modeling, and I put Dave Shoesmith up here for work that he did a while ago when he was under contract to EPRI on pit crew.

Next, please. What we're going to talk to you about is that really, we commissioned this work at EPRI because we were concerned that the November letter overstated both the likelihood and the importance of localized corrosion during the thermal period. That's really what was the impetus for our work here. So, we commissioned an independent analysis of the TRB scenario.

We also evaluated the related work sponsored by the State of Nevada. So, you're going to see some of both. We figured that anything that was sort of under a hot temperature environment that might cause rapid degradation of the containers was sort of the same issue, even if the mechanisms are somewhat different. So, we looked at both.

So, the experts you're going to hear about, some of them today I've got listed here.

Next, please. What we'll talk about first is an approach we took, which is a decision-tree approach to evaluating the TRB scenario. I'll cover that, and I'll hand off to Don Langmuir, who will talk about the geochemical analysis that both he and Randy Arthur did. Then corrosion analysis will be presented by Fraser King, with input from Dave Shoesmith, followed by TSPA and regulatory compliance analysis, which Mick Apted will present, as well as the conclusions.

Next, please. So, I'm going to go through here, the decision-tree approach that we came up with for the scenario evaluation. I hope this isn't too much of an eye test, but I'm kind of worried it is, so I'll just read them here. We split up the TRB scenario into a bunch of questions that we felt all of the questions had to be answered yes for the TRB's deliquescence scenario to be of concern.

So, here's the questions we asked ourselves. First, can the proposed pure divalent-chloride deliquescence bring form? If the brine forms, is it thermodynamically stable, and will it exist? If the brine is stable and persists, will it retain a corrosive composition? And, if the brine remains corrosive, can localized corrosion be initiated? Don Langmuir will talk about those issues, as a bit by Fraser King at the end.

Fraser will then continue with the decision tree and ask the question again, if brine remains corrosive, can localized corrosion be initiated? As well as asking if localized attack initiates, will it continue to propagate? Assuming all of those answers are yes, then Mick will talk about if there is early localized corrosion, will the repository fail to meet the standard, the regulatory standard? Only if all of those are yes, then in our opinion, TRB's scenario is of concern.

Next, please. So, we sort of had to ask ourselves what is it that we care about? What is it we think might be the issues related to a localized corrosion of Alloy 22? So, these are very approximate. We've seen literature that suggests that somewhat at temperatures maybe down to that range you might under very aggressive other conditions, get potentially localized corrosion.

You've already heard about nitrate/chloride ratios that have to be less than a certain value, roughly .2, and then there are mechanistic requirements. For example, you need to have local oxygen depletion, followed by, and they're almost the same, separation of anodic and cathodic processes. And, then, local acidification inside the occluded region. All of these are required for there to be localized corrosion.

So, you will see us address issues about temperature some. We'll hear about nitrate/chloride ratio discussion from us. Fraser will talk about these two, and you'll hear a lot from Don about whether we believe that you can get high acid environments or not.

Next, please. Okay, this is my last viewgraph. To give you the conclusion up front, multiple lines of evidence indicate there is no technical foundation nor safety-assessment basis to support concern about the TRB scenario.

Our analysis that we're going to present here suggests the answer is likely to be no at all the decision points on the decision tree. And, the remainder of the presentation provides the bases for the conclusion. And, I'd like to give to the Board a more detailed report that goes along with this talk that goes into the issues in a little bit more detail.

Don?

LANGMUIR: The first slide, our goal is to assess the likelihood that acid gases from a breakdown of deliquescent salts might cause the localized corrosion that results in failure of waste packages.

Well, we can't really address this question intelligently without considering the behavior of all reactive components in the repository system towards these salts and acid gases, not just one piece of this, but all of these components, because they all include reactants that can affect the conclusions we're going to try and draw.

Today, we'll talk about the ones that are underlined. Gas phase in the drift, waste and waste packages, dust on waste packages. And, note, I put here and minor amounts of soluble salts. They are minor. And, geologic materials and the porewaters in the drift walls. All of these are important components, and they all get involved in answering this question.

This is our repository system schematically. Lots of engineering components here, drift wall, ribs, so on, waste packages. All of these things are of concern to us in answering this question.

Next is the decision-tree again. The first question: can the proposed pure divalent chloride deliquescent brines form?

Next slide. We're going to focus in our talks today on the thermal period of the repository when temperatures are above 100 degrees C. This is what the Board was concerned about. This will be our focus.

The formation of these brines presumably depends on salt bearing dusts that occur on waste packages in the repository. We're going to look at these dusts as the source of the salts. The information available to us is the USGS's work on ESF dust collected on the waste packages and in the tunnel, rather, by Peterman and others. We've worked with the USGS, I shouldn't say worked with, collected from them publicly available information on wind-blown dust. We've added to this with mineralogy work that we've done on materials they provided for us that's available to everybody. This is the likely dust to be in the repository after closure, this wind-blown dust.

Key data for both dusts which we've collected is the abundance of soluble salts that might promote corrosion, i.e. chloride, and ions that may inhibit it, nitrate and sulfate, and abundances of minerals that will affect, neutralize and acidities associated with the deliquescent brine.

This table is a summary of the ion concentrations in the different kinds of salts in the dust, this is the wind-blown dust chemistry, based upon the USGS work, and then here's the USGS work on the ESF dust salts. And, notice I put up here along with the salts information, precipitation chemistry map information on the ions, the cations and ions, these are molar values, from maps, and these are two local sites for sampling of precipitation, which shows similar kinds of chemistries.

I've used this information from these intermediate three precipitation sources, averaged it to produce the cation values up here for wind-blown dust, which has not been yet measured, and it strikes me that since the anions are almost identical, it's a pretty good assumption that the cations are likely to be, too. So, precipitation chemistry is probably pretty much the same as dust chemistry, which makes good sense.

Notice in this figure that in the slide, that the nitrate is the dominant anion, for all of these precipitation and wind-blown dust examples, and along with sulfate, it dominates over chloride as well in the ESF dust. Chloride is about 10 per cent in the wind-blown dust and precipitation results.

Next, please. This summarizes the salt contents. Even the wind-blown dust, notice nitrate dominates, sulfate also, chloride 10 per cent. The nitrate, sulfate, chloride ratio 9 to 1, a lot more than--less than .2 that's an issue for corrosion.

The next one, please. Same calculation for the ESF dust salts. 3 to 1 the ratio here, chloride 25 per cent, less than either sulfate or nitrate, and the ratio again that's of concern is .2 or less.

Next. And, this summarizes the anion compositions on a trilinear diagram. This is the chloride corner, and you can see that this yellow part of that corner is where corrosion is an issue, if your compositions are up there, they're not. They're way down in the bottom of the figure where it's non-corrosive, close to the nitrate corner, or somewhere in the middle.

Next, please. One of our questions is what happens up temperature to this system? It's pretty hard to picture that a calcium chloride brine is going to hang around as such very long at high temperatures. We'll talk later about it breaking down thermally. But, if it persists, it's surrounded by dust particles at the 99 per cent level, and it's likely to dissolve any nitrate and sulfate that occurs along with it. It's going to be tough to separate itself from that much other material, and these are likely to dissolve in it and make it less corrosive.

Next, please. So, answering the first question. Will a pure divalent-cation chloride deliquescent bring form? Highly unlikely. And, the reasons for this, the only source of chloride salts in this period above 100 is going to be ESF dust or wind-blown dust.

Predominant solids are alumino silicates, silicates and carbonates. Wind-blown dust is less than 10 per cent soluble salts, and only .4 per cent chloride. And, if you calculate the calcium chloride content of wind-blown dust, it's less than 1 per cent, if you convert the chloride to calcium chloride.

Calcium chloride brines are likely to re-dissolve nitrate and sulfate salts and contact with them and become somewhat less corrosive. And, this point, reaction to magnesium in brines with silicates in the dust is likely to remove the magnesium from the brines.

Next. The second question. If the brine forms, is it thermodynamically stable and will it persist?

Next, please. To answer this question, is to consider the system an open system, and if it's an open system, which is presumably is, by and large, you're going to lose volatile acid gases that will de-stabilize any brine fairly quickly at elevated temperatures. And, these are the kinds of reactions likely to occur.

Calcium hydroxide product from this breakdown of a chloride brine using this as our example, HCL gas released to the atmosphere in the drift, perhaps a calcium hydroxide chloride salt, again, acid gas release, and if any moisture is around, this will all be converted to calcium carbonate, because that's the stable phase of the CO2 question on the drift. And, again, HCL is gone.

The HCL is transported away from the package surface, which drives all these reactions to the right. Brines are decomposed, leaving you a non-deliquescent, solid, and alkaline solid.

The next, please. A schematic of these reactions. These are values for 25 degrees, but they will remain alkaline to near neutral at higher temperatures as well. We've done some modeling of this one. The pH of this at 146 is 6.2, neutral is below that. So, this is a slightly alkaline solution. This is what you might have--I'm sorry. The breakdown here, pH 12 1/2, is the pH of calcium hydroxide. If CO2 is added from the drift atmosphere, giving you the carbonate, it's stable at pH 8.3. Again, these are low temperature values I've computed. But, they're also going to be alkaline values up temperature.

Next. We've discussed this a little bit before.

As you heat the package, it's the hottest thing in the system relative to the drifts, which are cooler. You've got a concentration gradient from the source of the HCL gas on the waste package. You've got a thermal gradient as well. Both of these tend to drive the HCL away from the package towards the drift wall.

Now, the tendency for the gases to react with the drift wall is related to the relative areas of the packages and the drift wall. And, you can calculate those in a qualitative way. If you assume a geometric surface to the waste package, and I picked the largest waste package likely to be used for this calculation, and you consider the roughness of the drift wall in terms of the geologic material, and this is a typical roughness figure, you will find the waste package comprises less than 5 per cent of the area of the drift wall.

If you look at the drift wall differently here, as a bunch of little tuff particles, which has been done by the DOE in a number of studies, you can calculate that based upon that with a 1 millimeter skin of drift wall, the waste package is less than 1 per cent of the area of the drift wall.

So, where is the acid gas going? It's going to a cooler drift wall, which has most of the area, and it's a lower temperature.

Next. This is a calculation of what the chemistry looks like in a calcium chloride brine that's trapped on the surface of the waste package. This is an 8 to 6 calculation from Randy. And, it will have an HCL pressure that's $10^{-3.5}$, and under those conditions, if it's trapped and it can't breathe to the atmosphere in the tunnel, you'll have calcium chloride brine stable.

DOE has done a calculation of the chemistry of condensate up in the drift wall. I've got the reference in our notes, and it's in our handout materials. And, in this calculation of condensate chemistry, they find they have a very, very low HCL pressure. Notice that the H2O pressure is 4.3 bars. This means this will dry up, since the equilibrium pressure is one bar.

So, on the drift wall, you're not going to have any water under these conditions. This is 146 celsius--I'm sorry, it's 96 at the drift wall. It's going to dry up, and calcium carbonate is a stable phase.

Now, you can back calculate from the information for a closed brine and the drift wall, and calculate what would be an equilibrium with the drift atmosphere on the waste package surface in the presence of atmospheric pressures, and this is what you get. And, again, calcium carbonate is the stable phase. This high water pressure tells you it's going to dry out, and you've got a low HCL pressure.

Next, please. Well, what happens to this HCL if it gets to the drift wall? It's going to react. You've got an alkali world out there in the drift wall. Essentially everything is alkaline. The HCL gas will react with albite feldspar, which is 24 per cent of the drift geology, as an example, pH drops to 5.6, you make a clay. It reacts further as you add more HCL, and you end up with a mixture of kaolinite clay and albite, which buffers the pH and it will never go below 5.7 at 146--I'm sorry--96 degrees in the drift wall. So, you're buffering the pH, and that's as low as it will go.

Next, please. So, summarizing this question. If the brine forms, will it be stable and persist? And, the answer is no. You'll keep losing HCL from a calcium chloride brine. The brine will decompose, forming a non-deliquescent solid, which will dry up, which will be calcium hydroxide initially, and perhaps ultimately, calcium carbonate on the waste package surface. And, the concentration gradient of volatile HCL will drive it from the hot waste package surface across the drift, into the drift wall, where it will tend to dissolve in pore waters up in the drift wall, and be neutralized with reaction with tuff minerals.

And, this reaction is driven by temperature gradients, chemical potential gradients, concentration gradients, and the fact that the area of the drift wall is 20 to 100 times greater as a reactant in this system than is the waste package.

Next. The third question. If the brine is stable and persists, will it retain a corrosive composition? Our approach here was to model the chemical processes that might create conditions that would initiate local corrosion. And, the TRB has talked about it happening under a crust or in an occluded location on the package of a surface under closed system conditions.

Now, we've just talked about the composition of such a brine. So, under such a condition, if we could make it, if we could create this brine, and this is hypothetical, it could not lose its HCL, and the reactions then would be limited to reactions with dust in that fracture, which are dominant materials, and the Alloy 22.

Next, please. What's the dust made out of? And, this is some work we've been doing at EPRI. It's a combination of what I'm calling basic minerals, which are minerals that consume acidity and will continue to do so, which represent 60 per cent of the dust, whether you're talking about ESF dust or wind-blown dust. It's about 60 per cent reactive minerals that will consume acidity.

Yes, it has some inert minerals in it, but these are the important ones from the point of view of the possibility of acidity persisting. And, notice the soluble salts. In ESF dust, .3 per cent. In the wind-blown dust, less than 10 per cent.

Next, please. These are low temperature calculations of what happens when these minerals contact water in the drift, and what they show is that if these minerals react with water in the drift, this is called weathering at low temperatures, but the same things happen when you get high temperature, too. It's a weathering process when acids hit these things. The pHs are near neutral to alkaline. And, they will also be near neutral to alkaline at high temperatures.

Next, please. The point of this is to show you the stoichiometries of these weathering reactions. So, here's K-spar, and it consumes 4 protons when it's broken down as it's attacked by any kind of an acid gas, 8 protons for the Anorthite, these are 3 feldspars, for the clay, 7.32, for calcite 1 proton.

With this information, and with an analysis of the rock, and the amounts, the molar amounts of the minerals in the rock, we could calculate the ability of the rock to consume acidity, which we've done.

Next, please. This, by the way, is material that's in the back, in the back of the handouts. The soluble salts and ESF dust are .3 per cent. That's only .02 per cent chloride, by the way. Basic minerals dominate here, and if you were to convert all of the HCL, all the chloride, rather, and all the nitrate that's in that salt, and the hydrochloric acid and nitric acid, you could consume it and you'd be left with 99.7 per cent of the basic minerals left.

So, it isn't going anywhere. If you make acid on the waste package in this stuff during the thermal period, it's going to be neutralized right in place, and there's plenty of dust left over to do the job.

Next, please. For the wind-blown dust, the soluble salts 9.6 per cent, convert all the chloride and nitrate in that salt to an acid, and you still have 92.7 per cent of the basic minerals left, because they're intimately mixed with the salts, and they're going to react with them. They can't avoid it.

Next, please. Let's talk about the brine itself. This is the saturated brine in some sort of an isolated atmosphere. This is the hydrogen iron concentration, call it, if you like, the pH descriptively here, 6.15, neutral at these temperatures is 5.82. The chloride brine itself is slightly alkaline.

Next, please. What happens if we have dust down in an occluded place in this waste package and it's isolated from the atmosphere, what's it going to do? It's going to come in contact with the minerals in the dust, which dominate the percent of material in what you're looking at. And, what happens to it? Initial brine, 6.15. Add a little calcite, pH goes to 10.6. If, instead, you add a little albite feldspar, pH goes to 8.35. You make a clay, and then the pH goes up to 8.8. That's going to happen in your crack before you get any chance to cause corrosion. Those are the conditions of the brine in that crack.

Next. Question. If the brine persists, will chemical conditions within the brine necessary for initiation of localized corrosion be maintained? And, after all I just talked about, I'm going to say no.

Naturally occurring minerals in the dust have a strong and rapid buffering capacity and will neutralize the acidity. The abundance of basic mineral phases greatly exceeds that of soluble salts. Corrosion-inhibiting soluble salts, nitrates and sulfates, greatly exceed the concentration of chloride salts. And, finally, the ratio of nitrate to sulfate, plus sulfate to chloride is 3 to 1 in the ESF dust, and 9 to 1 in wind-blown dust. So, you're way outside the range of ratios that are concerned with corrosion.

I think we're ready for Fraser King.

KING: Okay, thanks, Don.

So, just to recap, two of our six decision points concerned corrosion, and those are the two issues that I shall be talking about in the next few minutes.

So, we have two questions to answer. Firstly, if a corrosive brine does form and persists on the surface of the waste package, will localized corrosion initiate? And, we have a couple of sub-points there. One addressing the concentration, relative concentrations of inhibitive ions to chloride ions. And, secondly, I'll spend a bit more time, this is new information, some analyses we've been doing on the ability of the dust deposit, or salt crust to act as an effective crevice former. And, in particular, we'll be looking at the ability of those deposits, the crevice forms, to create a differential aeration cell, and thereby induce localized corrosion. So, that's the first question about initiation.

The second question. If initiation does occur, we think it unlikely, if not impossible, but if it does occur, will it propagate to failure? And EPRI historically have done work on looking at the propagation rates, modeling the propagation rates of localized corrosion, and I'll say something about the stifling mechanisms at the end, and just show the results of some of our past TSPA calculations.

So, firstly, on the question of localized corrosion initiation. I just have a couple of slides here just to recap the effects of inhibitive ions and nitrate and sulfate ions here, and carbonate as well. This shows some data that was presented last year by the DOE. I'm just going to show the polarization figure without nitrate in a 5 molar calcium chloride brine, and then the effect of added nitrate on this nicely creviced sample that is typically used in the project experiments. And, the addition of nitrate, as we all know, shifts these repassivation potentials, both the breakdown potential and the repassivation potential, which is being used as a criterion for the difference between this repassivation potential and the corrosion potential, as the criterion for whether localized corrosion would initiate. Both of these potentials has shifted more positively in the presence of these added inhibitors.

Next slide. And, this just shows again Don's figure here, comparing the nitrates to chloride ratios in the ESF dust and the wind-blown dust, compared this zone of susceptibility of Alloy 22, in this triangular part. And, this just shows the same data in a simplified format that a simple electrochemist can understand, comparing the ratios in these dusts to those ratios shown experimentally to initiate localized corrosion. So, just to reiterate what Don mentioned previously, and I'll say again, and we'll hear more about this tomorrow, I'm sure. So, as shown earlier, the nitrate and the sulfate dominate over the chloride in these Yucca Mountain dusts. And the ratios in these dusts far exceeds the ratios required to initiate localized corrosion, as demonstrated in the experiments.

So, that's one of our initiation arguments. The other argument, and I'll spend a bit of time on this, is that these crevice forms, these permeable dust deposits, will not be suitable crevice forms. The sequence of events required for the initiation and, finally, the propagation of localized corrosion. So, the first thing you need to do in order to cause localized corrosion is to deplete oxygen in the occluded region. That leads to the spatial separation of anodic and cathodic reactions, localized dissolution of metal within the occluded region, which leads to a hydrolysis of local acidification.

Then, and only then, once the localized corrosion has initiated, does propagation proceed, and that's supported both by the reduction of oxygen outside the occluded region, as well as that approach inside the region. There's also other processes which don't bother us here.

But, in the case of permeable dust deposits, we don't believe that these will support localized corrosion for a number of reasons. First, you've got permeable to oxygen, and this will prevent the creation of this differential aeration in the first place, and thereby, the separation of the anodic and cathodic reactions, which is the definition, of course, of localized corrosion.

In addition, as we've just heard, there's a huge buffering and neutralization capacity of these dusts, and that will prevent the local acidification with the "occluded" region. And, intentionally here, I put occluded in quotation marks.

So, we've addressed this issue by a simple conceptual model, and this just shows the surface of an Alloy 22 waste package. We have a dust deposit, a thick dust deposit, sitting on top, which is permeable to oxygen. And, at the bottom of that, we assume that a thin deliquescent film. And, what we're going to look at is the rate of consumption of oxygen at the deliquescent solution/metal interface, and compare that with the rate of replenishment of oxygen to this conceptual, through these layers, to see if we can replenish the oxygen faster than we can consume it. If we can do that, then we don't create a different aeration cell. We can't initiate localized corrosion.

So, to compare those two processes, I'm just pointing out here that this is a simplified conceptual model for calculation purposes only. We believe that this deliquescent film will be sort of isolated in small pockets on the surface.

So, again, what I'm going to look at is mass transport through these porous media. Now, in general, and my background is from a country where we're considering of disposing in a saturated zone, and so we've looked, as other countries have, in a lot of detail looking at the diffusion of oxygen and other species through compacted materials. And, it's that expanse I'm drawing on here to make these calculations.

Source of interest in the agriculture and soil sciences. There's a lot of information in the literature, which is also of use in unsaturated soils, looking at the effect of the diffusion of oxygen through porous media.

So, the effects of porous deposits on corrosion processes are two-fold. Firstly, porous deposits restrict mass transport of reactants to, and, of course, corrosion products away from, the corroding interface. And, that's typically taken into account using effective diffusion coefficient, where the diffusion coefficient of bulk solution is multiplied by porosity, and a tortuosity factor, to take into account the tortuousness of the porous network. So, the porous layers obviously inhibit mass transport.

They also block a fraction of the surface, and they electrochemical reactions from occurring. And, as it turns out, for randomly oriented, randomly sized porous network, the ratio of the area exposed at the bottom of these pores, the active surface area on the base of the pores, the geometric surface area is equal to the bulk porosity. This bulk porosity appears in two places here, and that's an important parameter for us to try and estimate.

So, the required input data for this calculation, firstly, the rate of replenishment of oxygen is going to be a simple mass transport calculation. The rate-determining step here is the rate of oxygen diffusion through that thin water film currently in contact with the waste package surface. Even though the dust layers may be much thicker, because it's unsaturated, the rate of diffusion coefficient through unsaturated soils, are many orders of magnitude higher than that in solution. And, so the rate-determining step is diffusion through this thin water film, which is in this porous matrix.

So, we need to know the porosity and tortuosity factor of that water film, which is in this porous deposit, which is the same as that porous deposit, and in the absence of data of dust on waste package surfaces, use data from a compacted clay, and I'll show that in a second. Also, the porosity and tortuosity factor in simulated steam generated deposits, we've also drawn upon that.

We'll also need to know the concentration of oxygen, and, of course, that's a function of temperature and the salt concentration. An important parameter, the thickness of this water film that could form on the waste package surface. That's the rate of replenishment. The rate of consumption we're equating to the passive current density. And, this is prior to the onset of localized corrosion.

And, so, the rate of consumption of oxygen underneath this deposit is equal to the passive current density.

So, the next slide shows some data for the porosity and tortuosity factor. Again, this is taken from data on compacted clays. So, as a function of density, the porosity in these pink squares, and the tortuosity factor in these blue diamonds, as would be expected, decrease with increasing density.

And, I should point out that up to density of about 1 gram per cubic centimeters, it's possible to compact these clays by hand. Above this sort of density, though, you need a hydraulic press, pressures below just several tons per square inch. So, these are highly compacted systems, yet they retain a lot of porosity, and although the tortuosity factor decreases with the increase in density, quite a significant tortuosity factor.

So, another set of data that we've used to try and get a ballpark on these numbers for our calculations, are some hand-compacted magnetite powders, which we used to simulate steam generated deposits, and there, they had a density of about .5 to .6, and a tortuosity factor, these were highly compacted, of .64 to 1. So, for our calculations, based on these two sets of data, we've conservatively assumed the porosity of .5, which is below that we believe we can achieve on the waste package just by simply wind-blown dust, and a tortuosity factor of .2. So, that's our porosity and tortuosity data.

The other input data, as I said, the bulk oxygen concentration, so we have a salting-out effect of this, and for purposes of calculating the salting-out effect only, I've assumed that the deliquescent solution is the 5 molar calcium chloride solution. So, salting-out factors have been 8 times lower oxygen concentration due to the salting out. Of course, the oxygen concentration is also a function of temperature, and that's taken into account in the calculations.

For the thickness of the deliquescent film, which is also part of our calculation, we base this on data from the TGA analyses which were reported last year by the DOE, and as we saw earlier, there was a mass gain initially when those experiments were done of 1.7 milligrams due to absorption of moisture from the atmosphere. The area was about 17 square centimeters, and, so, that gives water layer figures of almost exactly 1 micron. So, that's our water layer figures for our mass transport calculation.

The diffusion coefficient is obviously a function of temperature. It's typically equal to that of the discussed waters, that's 19 kilojoules per mole. So, these input data for the calculation relate to transport, the rate of replenishment of oxygen to the waste package surface.

And, that oxygen is being consumed at a rate given by the passive current density, and for that, I'm using this data from the Center. And, I should point out here that that has a higher activation energy compared to the diffusion rate, and, we'll see that the data converge at higher temperatures as a consequence of that.

Next slide. So, again, just to reiterate, what we're going to do here is we're going to compare the rate of consumption of oxygen on the waste package surface, given this rate of replenishment given by Fick's first law.

For the thickness of the water film, we're going to use this valued 1 micron derived from the DOE data. And, for sensitivity analysis purposes only, we're going to use 10 times the 100 times thicker water layers.

And, so, the point here is if we can replenish oxygen faster than we can consume it underneath this dust deposit, then it doesn't add to the very efficient crevice former, and won't initiate localized corrosion because of the separation of anodic and cathodic science.

So, here are the results of those calculations. The rate of consumption is shown in blue as a function of temperature. I'm showing these as current densities in both cases. This is the rate of oxygen consumption converted to a current density, and a function of temperature, and obviously, with increasing temperature, the rate of consumption increases.

And, in comparison, the rate of replenishment by diffusion through this thin water layer, and these are the data for that 1 micro thick water layer, which we think best represents the thickness of the deliquescence solution, of the order of, in the case of this water film thickness, 4 to 6 orders of magnitude higher than its rate of consumption. Even for much thicker water film thickness, 10 times, 100 times thicker, there's still a wide margin of higher rates of replenishment of oxygen than its rate of consumption.

So, the bottom line here is that these crevice, dust deposits, do not act as good crevice forms. They do not result in oxygen depletion. There's no differential aeration associated, and, therefore, no separation of anodic and cathodic sites.

Indeed, you can convert these data into the ratio of the interfacial concentration of the solution on the waste package surface to that in the bulk, and that ratio is 99.996, a very small depletion due to the very rapid rate of replenishment to these unsaturated dust deposits.

Another way of considering these data is that in terms of the critical potential that should be used to judge whether localized corrosion would initiate, we shouldn't be using the repassivation potential for crevice sample, we should be using that for a sample which has free access to the environment, such as that that we derive from the passivation potential for pitting type corrosion, which are typically many hundred of millivolts more positive than those for repassivation potentials for crevice samples.

So, that covers what I have to say on the initiation of localized corrosion. Now, let's go on to look at the time dependent localized corrosion, should it initiate.

In the unlikely event that initiation occurs, there is strong evidence to suggest stifling will take place. And, here we list very stifling mechanisms. In the case of dust deposits on the surface, there are additional reasons to believe that stifling will occur, largely associated with the loss of the critical crevice chemistry, both the ion-exchange of aggressive doubly charged cations, and less aggressive sodium and potassium ions, but also because of the neutralization and buffering of the localized acidity that will be generated within a propagating crevice by alumino silicates and carbonate minerals, which Don has talked about previously.

There's also mechanisms, as I've discussed, involving the loss of the separation of the anodic and cathodic sites by the increasing permeability and oxygen diffusion through a dust deposit on the surface, ion for all types of crevices that diffuse iR control of the propagation rate.

Regardless of the mechanism for stifling, the net effect that is observed is often described by this expression, and this time exponent N is typically less than 1 in the stifling case. And, EPRI in the past few years, have gone to modeling studies on this, and these results show some previous TSPA calculations. Just comparing here the wall thickness of 20 millimeters for the Alloy 22 waste package, with the penetration depth as a function of time for two time exposures of that expression, these show data of a 2000 year period to cover both the time of the thermal pulse, and any continued propagation when temperatures drop below the repassivation potentials. So, again, taking that 2000 year period for that calculation.

The value of the B coefficient for the power expression is based on data from a very aggressive solution for less corrosion resistant alloy, and it's, therefore, conservative. I'm using the two bounding values for this time exposure N of .1 and .5, which is a theoretical value for an iR, for diffusion control process.

But, as you can see, in both cases, especially for the time exponent n equals .1, there are very limited propagation, even continuing with time, a rate that's decreasing with time, and within this period when localized corrosion, should it initiate, might be feasible, the penetration of the wall is less than 25 per cent.

So, just in summary, EPRI's corrosion analysis, the two questions we've addressed, if this brine forms and persists on the waste package surface, can localized corrosion initiate? Our answer to that is no. We believe that the concentration of inhibiting ions, deliquescent solutions, far exceeds that of the aggressive chloride ions.

A second reason for non-initiation is that these dust deposits that might form are permeable. They will allow oxygen to diffuse through, and our calculations suggested there will be no separation of anodic and cathodic sites. And, even if there is localized events, then no localized acidification could occur because of the buffering and neutralization by the basic minerals in the dust.

The second question is if localized corrosion does initiate, will it propagate the failure? Again, our answer is no. And, our belief is that there's a number of stifling mechanisms that will prevent through-wall penetration within the period of localized corrosion propagation.

And, that covers our corrosion analysis, and I think Mick is going to finish up with some TSPA stuff.

APTED: I feel like this is a trial for American Idol here. Everybody sort of rotates up to the front.

Well, we're well within about 15 minutes into our free beer time, so I'll try to be quick and wrap this up.

This is the last question we're up to, and one thing I should say about this decision-tree, or chain of logic. I've been involved in a lot of international programs that have been very successfully used for looking at some contentious technical what-if issues, where people love to speculate, issues on glaciers and colloids and microbial survivability and so on, and it's been used by a number of international programs very successfully, and I think there's a record to be learned here in terms of trying to follow this kind of approach in breaking down some of these issues that have been very difficult for us to come at.

Do we come at it technically and launch an R&D program? Do we try to solve it all by a QA Resolution, or a PA resolution? Something like that. So, I recommend it all to keep it in mind as a way to try to put some of your questions that come up not only in this case, but in other technical areas as well.

Okay, next slide. So, if waste packages are locally penetrated, will the releases exceed regulatory compliance criteria? The first point I want to point to our approach, basically is to apply a total systems approach, and I think that's the key word. If you're at all a believer in multiple barriers as a fundamental strategy and approach to geologic isolation, then if you're not thinking of a system, you're doing yourself a discredit, if you're focusing just on one barrier. If I'm a geochemist part-time, if I'm focusing just on the chemistry, I can really miss some of the other connections where other barriers, other processes begin to really dominate.

So, we followed a total system TSPA approach to evaluate the sensitivity and relative importance of this postulated scenario.

The second point is much as I hate to agree with Redwing fans, I must agree with Tim McCartin here. TSPA is, I believe, as he said, really valuable to provide some risk-based insights into this type of repository system. We've all heard many people say how complex it is, it's hard to unravel, all of this complexity. But, PA is the one area where we can bring this sort of Tower of Bable together among hydrologists and geochemists and corrosion people, and begin to sift through the true relative importance of items.

Lastly, of course, the National Academy is on record during their very important 1995 technical standards report emphasizing the key role of the performance assessment in placing any technical issue into the proper context.

Okay, so what have we done? We've looked at regulatory compliance analysis. Basically, we've done, despite what Roger said earlier, I think we have really bounded this. We've said at the time of repository closure, all of the packages are failed. All of the canisters are failed. So, that's hard to go past that in terms of the canister performance. We've sort of done a barrier neutralization. Of course, barrier neutralization has been done, again, very widely by all of the repository programs in Hargro in Switzerland, SKB in Sweden, JNC in Japan. Everyone approaches it in very much the same sort of approach.

So, we've assumed all waste packages fail by local penetration at t=0. The drip shield is still intact in this particular variant. And, the results, we find that the release is dominated by Iodine 129, technetium 99, so-called instant release fraction nuclides. But, that compliance with the EPA and the NRC regulatory criteria is shown for a 10,000 year period, and beyond, all the way out here to fast peak dose where we're looking at time scales on the order of a million years. Just for those in the back who can't see, the EPA standard of 15 milligrams is right up along this wavy line of mine.

Okay, next slide. Now, we've got to look at regulatory compliance in even further conservative space, where the container and the drip shield are initially failed at the time of closure. So, those are conditions to equal 0. Results, again, we see the release is basically dominated by the Iodine 129, technetium 99. Compliance with EPA and the NRC regulatory criteria is shown for the first 10,000 years, which is right here, and that the maximum dose at later times is always basically below the comparable natural background radioactivity at Yucca Mountain. So, for a set of barrier neutralizations here, we've shown that yes, there is going to be compliance within the safety assessment.

Last slide. Okay, so the question posed. If waste packages are locally penetrated, will releases exceed regulatory compliance? No. Even assuming localized corrosion of the packages, resulting in release rate of radionuclides five times faster, complies with regulatory safety criteria for all times, and even assuming the loss above the waste package container and the drip shield, we still show demonstration of compliance with the safety criteria.

So, for the long-term safety for nuclear waste repository, Yucca Mountain is robustly assured by a multiple set of barriers. The message isn't that oh, we don't need the canisters, the message is we have really what we've set out for here, is achieving a set of multiple barriers and processes, because it's not always just a physical barrier you can point to, but a process, mass transport. Tim mentioned something sorption, these are other barriers that all contribute to the isolation successfully of nuclear waste at Yucca Mountain.

Last slide, conclusions. I'm going to go to Number 2. I want to stress again the merit of this approach is that all decision points in the speculative scenario that's been set up by the TRB must be answered yes. You can't get down here unless all these decision points chained together are all answered yes. If even one were no, the issue is dropped out. It's not of importance.

In our analysis that we've just gone through, we've looked at each of these questions. Will the proposed divalent pure deliquescent brine form? Highly unlikely. If it forms, is it stable and persist? No. If it does, is stable and persist in some sort of speculative closed system environment, will it retain a corrosive composition? No. If the brine remains corrosive, can localized corrosion be initiated? No. If localized attack is initiated, will it continue to propagate? No.

And, finally, if all of that--all of this--wonderful R&D were actually to be needed, or something, and we look at this from a safety compliance point of view, if early localized corrosion occurs, will the repository fail to meet the safety standard? The answer is no.

Based on this, multiple lines of evidence, and I come back to Dr. Latanision's initial presentation where he mentioned multiple lines of evidence, indeed, being important, we find and we conclude that there's no technical foundation, nor safety compliance basis, for continued concern about this deliquescent brine leading to early failure of waste packages by localized corrosion.

Thank you very much. We can ask questions, or we can ask questions over beer, or we can leave it up to Dan.

BULLEN: There's one thing about following Mick on a presentation. You never actually know where he stands on an issue. Okay, we'll take questions from the Board first, and I'll start with David, and then we'll go with Ron and any other Board members that have questions.

David, go ahead.

DUQUETTE: Duquette, Board.

I'm not sure where to start. First of all, I'm glad we're going to make the containers out of polyethylene. But apart from that, I would like to read the document that you're apparently passing out to us, because I have a number of problems with what I think is--well, first of all, I want to compliment you on doing a lot of work in a short time, and follow that was a reaction to our letter. That's number one.

Number two, there are a number of things that I found overly simplistic in some of the things you presented. That doesn't change your decision-tree and you may convince me that your decision-tree is correct, even if I change those things. But, my students would be very surprised to learn that if they make a saturated solution of calcium chloride, because they deal with potassium chloride all the time, and take it up to about 105, 106 degrees before it starts to boil, that they would get hydrochloric acid off as a gas. It turns out that I'd have to look at the thermodynamics, but I don't think that calcium hydroxide is more stable than soluble calcium chloride in the temperature range that we're talking about if you get a saturated solution.

That's at the beginning of it, and I'm not going to go through slide by slide, but there are things like that that bother me about the presentation, and I do want to take a look at some of the mathematics and so on and so forth. I may come to the same conclusion you do.

The bottom line, however, is that we have agreed that perforation of the containers will not compromise the performance analysis. We've said that right along. As a conservative engineer, if I can give you a barrier that will not fail, I don't even need TSPA at that point, if I can guarantee it won't fail. And, so, what we've been trying to push for is a container that doesn't have to depend on even the possibility of a localized corrosion.

Apart from that, we could get into a several hour discussion on the models that were used for oxygen permeation. That's assuming, of course, that it's all the same through all of the dust, and that it's not differential, so that you can't have a different cell in that situation. It's assuming that the data that was collected on the nitrate, the chloride concentrations, or ratios, rather, that were collected, at typically about 95 degrees celsius, is true up to about 150 or 160 degrees celsius. There are a lot of assumptions in the models you've thrown out, and while I don't want to address them here, I think you will be getting some response for us on it, and I think I'll let it go at that.

BULLEN: Bullen, Board. There wasn't a question in there. That was just a monologue?

DUQUETTE: Duquette, Board. You're lucky it wasn't 50 minutes.

BULLEN: I understand. Did EPRI's team want to make a comment or two? Don Langmuir, go ahead.

LANGMUIR: They talked about the possibility--I didn't really intend you to believe that we were going to have calcium chloride brine in the presence of--with calcium hydroxide and HCL gas. That's not happening. We're going to go from one thing to the next in a small micro environment on the surface. So, you're going to have your calcium hydroxide by itself once HCL is gone. I'm not sure I exactly understood. Maybe you could rephrase what your question is about how I presented that.

DUQUETTE: Okay. Duquette, Board.

I'm not sure where the HCL is going to come from, given the reaction you've put up as a chemical reaction.

LANGMUIR: Oh, the HCL comes from the breakdown of the calcium chloride. There's water shown in the reaction as well, giving you the calcium hydroxide. There's water in the deliquescent brine.

DUQUETTE: Duquette, Board. I don't want to get into a discussion on that. But, again, my students would be surprised. Yes, saturated solution of calcium chloride will produce HCL and calcium hydroxide.

LANGMUIR: Yes. What's the problem? This has been done and you're going to hear about it tomorrow, we've been told by the DOE, this is experimental work that DOE has done. Greg Godowsky has done this work. With a film on the surface of a canister which was kept moist and allowed to evaporate and generated a deliquescent film, and the product was calcium hydroxy chloride, and calcium hydroxide, and HCL was driven off as a gas. This has been done. It also applies to--this is a theoretical calculation here, but it matches the experimental work that's been done. The product is an alkali residue that dries up.

BULLEN: Bullen, Board. We'll move on to the next question. Ron Latanision?

LATANISION: Latanision, Board.

To follow on Mick Apted's comment. That sounds like a very good conversation for the beer period we're apparently in right now, and I'm sure the acid will become even more concentrated as the even wears on.

I want to, first of all, share David's comment about I guess I would say my pleasure in seeing EPRI commit the intellectual and fiscal resources to leap into this. And, so, I think if there is no other conclusion that EPRI is really involved with this whole discussion at a level that I haven't seen before, I'm very pleased.

But, having said that, I need to get--you know there's "but" right? There's always a "but." I need to understand the implications of some of what you've said, and I do share some of David's reservations. A lot of what you presented sounds very speculative, but, not withstanding that, comment. I need to understand a few other broader issues.

We know that the project and the folks at CNWRA have both demonstrated in testing that they've done that crevice corrosion will occur. We know that welds and aged material are even more susceptible in the testing that they've performed. So, the question is what is the implication? Is the implication that they have just done some very misguided tests, and after all the years of effort and public funds that have been used to support those tests, do we now conclude they've done the wrong thing? That's the first point.

Then the second point is what environmental tests should be done, or are we really dealing with the slam dunk that is shown on this last slide? Is this just a non-issue and there's no point in doing testing? Is that the conclusion we should come to? And, if it is, I'd like to hear your comments on that.

If that is the case, then it just seems to me that this sort of analysis has come very late in a very long process, which has committed millions and millions of dollars of public funds, and it would have been a monumentally important thing to have gone through an exercise like this very much earlier. I've asked a lot of questions, so I'd be happy to get your comments.

LANGMUIR: I can't respond to the last point you made. That's more for the program. But, specifically with regard to the salts issue and the corrosion fracture issue, I don't think anyone until us has really focused on what the dust is all about, and what its reactions will be with the salts and the deliquescent brines, and with the acidity. That's not been an issue that's been raised before. It's a very important issue, and I focused on the acid base aspect of the dust, but I'll hand it over to Fraser to talk about its application to the corrosion, and fracture issue.

KING: Fraser King.

So, in terms of you have two questions. One, the first what has been done wrong in the experiments. I should preface my remarks by saying our focus here is on the issue of deliquescence and the possible localized corrosion under the dust deposits.

Our issue is that using--in order to get that crevice corrosion, which is being seen by the DOE and by the Center, they have had to go to not metal to metal crevices, because you can't even initiate localized corrosion with metal to metal crevices, they have used crevices formed in a piece of metal and a piece of teflon, or other formable crevice former. And, those are, for some crevices on the waste package, we don't believe that they are characteristic of crevices that will form by permeable dust deposits.

And, so, the application of those repassivation potentials, which it measured on those highly conservative type samples, don't represent the conditions under a dust deposit. So, there's nothing wrong with what they've done. It's just that in the case of a permeable dust deposit, we think there are other approaches.

And, to answer your second question, the sort of experiments that could be done, and I believe are being done, would involve a crevice former, which isn't an impermeable sheet of PTFE or teflon, and would allow access of oxygen to the salt water occluded region.

The expectation would be there. The repassivation potential is that if you could do experiments under those conditions, which would be far more positive than those that you measure with an impermeable crevice former like a piece of PTFE.

LATANISION: Latanision, Board.

The tests that they've performed are really industry standard tests. I mean, if someone is interested in exploring the possibility of crevice corrosion using the device, technology that has been used by both the project and CNWRA, is not an unusual test.

KING: Correct.

LATANISION: So, I mean, I don't see your point. I mean, I understand that the dust issue is an issue that has to do with the question of whether or not deliquescence will occur and whether that will generate a locally concentrated environment. What I'm asking is have they chosen, in your view, to use the wrong environment to explore this question? Should they have looked at--what should they have looked at, if not 6 molar chloride?

KING: Well, I think the issue here is that under freely coding conditions, oxygen will permeate through these crevice walls, and, so, experiments under those conditions would be useful.

LATANISION: You wouldn't consider, for example, a lack of penetration, weld as being a crevice?

KING: As I said when I prefaced my remarks, we're focusing here on the issue of the dust deposits. Certainly there are metal to metal crevices elsewhere on the waste package, which aren't addressed obviously by that oxygen permeation argument.

LATANISION: So, would it be of importance from the perspective of your analysis, collective analysis, to look at welded structures or to look at aged structures from the point of view of the same kind of a decision tree that you looked at here?

KING: Yes. Again, the arguments about separating the anodic and cathodic sites here applies to permeable deposits and crevices formed under those.

LATANISION: Right, I understand that.

KING: And, so, for the crevice that forms on the stand, between the stand and the waste package itself, we can't use that argument, and we have to use arguments based on the chloride to nitrate ratio or the nitrate to chloride ratio, which is a second reason we believe that localized corrosion will not initiate under these conditions.

LATANISION: Latanision, Board.

Understood. But, I'm suggesting that we're talking about more than just a question of dust. I mean, there may be other crevices, other origin in a welded structure that perhaps play a role, too. We've seen in the data that's been presented by the Center that welded structures and aged structures have a different response in terms of crevice corrosion than do mill-annealed materials. So, in terms of your sense of an experiment, would that be an important issue to look at?

KING: You mean in terms of looking at the--

LATANISION: Welded structures.

KING: Those measures have been made.

KESSLER: Maybe we should wait to see what's said tomorrow, how much this is gone into. My guess is you're going to get the answer, I don't know what DOE is going to present tomorrow, but I suspect they're going to cover these issues, in terms of we were talking about general criteria for localized corrosion, and they apply as well to base metal versus weld affected metal, whatever.

So, I think that our general analysis still holds, whether you want to look at what is the extreme case, and if you want to do things by trying to be bounding, I see that's what DOE has been doing. You know, a lot of their chemistry, even our arguments here, was okay, we think that we're going to have a combined nitrate/sulfate/chloride system rather than a pure chloride, but let's set that aside, let's be bounding maybe. DOE is doing the same thing. I see often that their experiments are driven that way. Does that mean they're the wrong experiments? No, you start there. Those are the experiments you do first, and you sharpen your pencil as you need to. That's what I see DOE doing.

LATANISION: Latanision, Board.

I just, one last comment, and then I will stop. I'm just making the observation that if I took what's shown right here in the extreme, there would be no need to look at the issue of crevice corrosion. I think that's clearly the implication. Right?

APTED: I think it's absolutely wrong in that sense. Look at the title of our presentation, high temperature deliquescent brine. What was your initial, you know, you setting the scene today, you said the issue is deliquescence to high temperature condition. I turn attention to Bobby Pabalan's slide Number 4. Bobby took a much broader view. He set up those four stages. So, we've been addressing very much this stage one in our analysis, and I think you said at the beginning, the Board's report in November was focused on that same period.

All right, now your questioning is about these other tests. Certainly all these other test periods, the type of test data that's been collected, are very relevant to those kind of later conditions, temperatures of 105 to 195, looking at failure of the materials during these other conditions. So, don't take our analysis too far. We were pushing back exactly on one particular time, temperature interval, and not across the whole range of issues on corrosion.

LATANISION: Thank you.

BULLEN: Before I go to Richard Parizek, I noticed there's some Morse Code from the microphone with Mick there. Did you want to say anything else before I let it go, or is the Morse Code enough?

APTED: No, no.

BULLEN: Okay. Dr. Parizek?

PARIZEK: Parizek, Board.

In that spirit of just looking at the deliquescence issue, I'm looking at your figures on Page 50 and 50, which gives really a TSPA type analysis, without the drip shield and without the container, in order to do that, obviously, there's a lot of other things involved here beyond just this position; right? So, John, is this lately run data for this DOE data, or are these EPRI data? I'm looking at the two figures.

KESSLER: These are our model using data that we think are appropriate from whatever source. A lot of it is stuff we got from the project that we think is good data, and we'll use data from outside the project, and the combination of the two that are EPRI, TSPA analyses.

PARIZEK: So, Parizek, Board, again.

What's in it? I mean it's truly the rocks matter is in it, I mean, the rocks are performance. But, then other things about the waste package other than corrosion?

KESSLER: Yes. I mean, the point is is that we're not assuming that these waste packages go puff. I mean, they're still there. We can maybe still have diffusion controlled release, even though we may have some penetrations of the container. So, when we say failure, what do we mean by failure? Okay? We can have a penetration through the container, we still have a lot of other processes that work in favor of mitigating release. And, all those things are still in the analyses.

PARIZEK: Well, I guess from the Board's point of view, it would be useful for us to have an update, what goes into all of this. I mean, it's heartwarming on the one hand. On the other hand, it's beyond the point of deliquescence.

KESSLER: It was in our December '03 report.

BULLEN: Bullen, Board.

Actually, I think EPRI provided us with ample quantities of that. I have one of my own. I don't know if Richard has one. I know that the Board does have that report, so it's available for us to look at.

PARIZEK: Okay, because I mean just with the "no's", it goes all the way down to the bottom of the box, and that's the last couple of "no's" sort of depend on TSPA, part of it, and that's beyond what we were looking at.

KESSLER: Exactly.

PARIZEK: The other question is for Don. You had like I think three things that helped reduce or neutralize the reactions, and one is the role of the nitrate, the sulfate, and so on, as a way to counteract the adverse effect. Did you consider processes that might consume, say, the nitrate? You've heard the question about bacterial activity, or something like that. Or, did you just sort of not pay any attention to that part of it? You obviously have a lot of other chemistry here that can overwhelm the acid problem, from what you've been showing us.

LANGMUIR: The question has been raised why wouldn't the nitrate be consumed by bacteria, and our feeling is that at the temperatures, in fact, there's experimental work on this that I think Fraser can speak to. But, my understanding is that at the temperatures we're dealing with here, the bugs aren't active. So, the nitrate will not be consumed by nitrate removal, by bacteria under these--under repository conditions. Other things may get rid of it, but that's not one.

PARIZEK: Parizek, Board. Not necessarily on the waste package, the temperatures, but some distance into the--beyond the rock wall, you're going to have a temperature that's suitable for bacteria, perhaps.

LANGMUIR: Yes, you will.

PARIZEK: So, at least in that part of the story, you could consume it. And, so, the question is has anybody looked at the consumption of nitrate at any location?

LANGMUIR: Well, we have data on nitrate in the unsaturated zone, and the ground water is moving down through the zone. We don't know exactly, though, where, if you're in that profile, you'll find the nitrate decreases a little bit, bicarbonate goes up, which is consistent with nitrate reduction. And, the sulfate is dropping just a little bit, too. But, these changes may reflect differences in infiltration as a function of time. It's not entirely clear that they represent reactions with depth.

PARIZEK: Parizek, Board, again.

It's water samples, say, right a meter into the rock wall, or nearly at the rock surface?

LANGMUIR: Well, these are USGS samples taken from the unsaturated zone as a function of depth through the whole profile from the surface on down.

PARIZEK: That's in a drill hole?

LANGMUIR: Yes.

PARIZEK: Not necessarily the repository tunnel?

LANGMUIR: These are centrifugally collected samples and squozen samples from Al Yang and the team in Denver.

PARIZEK: My point is that it would be nice to have a water sample near the tunnel, say emplacement drifts example, to see whether it's still there.

LANGMUIR: I think there is such data.

PARIZEK: I don't know, I've never seen it.

LANGMUIR: From a USGS report from last year.

BULLEN: Fraser, did you want to make a comment?

KING: Yes, I was just going to say that we believe there is evidence that nitrate is there now, and the only effect of emplacing these waste packages, which are radiation sources and heat sources, the latter will dry out and desiccate the rock, and that's going to preclude microbial activity for some distance for some time. So, there's no effect there which is going to further deplete the nitrate.

So, in fact, we have a conservative case now where we have ambient conditions, and those are as good as it's going to get for microbial nitrate depletion.

BULLEN: Okay. I saw a couple hands in the audience. Maury, did you want to make a comment, or do you want to wait until public comment, or do you want to address this, whichever is more appropriate? Okay, identify yourself, please.

MORGENSTEIN: Maury Morgenstein, GMI.

Although I appreciate the fact that we could have a dust deposit with a precip underneath it, and that precip might be an active one, I would also--have you looked at, for example, what might happen to dust if it was wetted and you formed a silcrete or calcrete, or a gypsum halide deposit encapsulating the dust particles? Which is probably much more likely if you consider a dripping environment on the waste surface? Dry dust with a deposit underneath it sounds like it's an extreme condition.

KING: Fraser King.

I assume you're making arguments about the permeability of such crusts?

MORGENSTEIN: Oxygen production, yeah.

KING: Production or permeation?

MORGENSTEIN: Permeation.

KING: Permeation. So, the answer to your question is no, we haven't considered that. I think our answer would be, though, that we have such a huge difference in the, three to six orders of magnitude difference between the rate of consumption and the rate of permeation, that we can't conceive of a deposit that would have three to six orders of magnitude lower porosity. And, so, I think the same arguments apply. The margin may be smaller, but I think the same arguments still apply.

MORGENSTEIN: Well, let me backtrack. Maybe you misunderstand me. If we're dealing with a silcrete, your permeability on that silcrete would start to approximate the permeability on the metal.

KING: In which case?

MORGENSTEIN: You'd be looking at a crevice.

KING: Yes. So, in that case, if the oxygen permeation is going to go down by more than six orders of magnitude, then it might be possible to cause a differential in aeration zone.

MORGENSTEIN: Okay. I propose that that would be a more normal situation than what you guys--

KING: I think Don is going to answer that.

LANGMUIR: I'd like to comment here. If you're asking for what represents a few percent of the total dust to encapsulate the whole thing, I don't see it happening. You're talking about less than 10 per cent, maybe 5 per cent in the case of wind-blown dust, of salts, and that has to somehow fill all the void spaces in the other 95 per cent and create an impermeable value. I don't see it physically happening.

MORGENSTEIN: No, okay, if you're dripping on a hot metal surface that has dust on it, and you form a deposit underneath that dust, and you react that salt with the metal, this is what you're proposing. What I'm proposing is that's a unique situation that probably will not occur. What will probably occur is that you will precipitate a solid that will encapsulate that dust, and that solid will be some combination of a silcrete or a calcrete or a gypsum and a halite combination, which encompasses the most--the least soluble ions in the water. And, this is what we normally would see, for example, in a fracture that had evaporation.

This is what we normally see at Yucca Mountain. Why would we not see something normal in your case?

LANGMUIR: So, what you're saying is that the fracture walls are totally impermeable, Maury, is that what you're saying?

MORGENSTEIN: Many of them are, yes. Well, not totally, but yes, much more so than dust sitting there with void space.

LANGMUIR: That's not my understand, but maybe DOE can provide some information. You're also, Maury, talking about a period that's not within the 100 year--I'm sorry--the 100 degree thermal period. You're talking about something after that. If you're going to have dripping on the system, we've gone beyond the period we focused on.

MORGENSTEIN: No, I totally disagree. I think you have dripping on the system as soon as you have closure. If you have a climate event which produces enough water to give you--in a fracture that focuses, you will have dripping. And, you can have dripping at thermal peak. We discussed this earlier.

BULLEN: Last call for other questions from the audience. Dr. Shoesmith, you get the last word, and then we have public comment, led by Dr. Duquette.

SHOESMITH: David Shoesmith, consultant to Bechtel.

I just wanted to address that last point and what the significance of being able to say that the dust can initiate localized corrosion is.

Dust cannot be the source of the initiation of localized corrosion and the drip shield comes back as a barrier. If the dust can bypass the drip shield, then the drip shield is not a barrier. That's a big feature of this repository. So, the drip shield becomes much less significant if it is the only source by which you can produce the corrosive environment that may start localized corrosion. If you can't do it with the dust, then the drip shield is a good barrier.

BULLEN: Well, I want to thank all the presenters. I'd like to thank the EPRI team for being patient and being last, and I will turn the meeting over to Dr. Duquette.

DUQUETTE: Yes, Duquette, Board.

We have two people who want to make comments on the public presentation. The first is Mr. Cleary. And, if Mr. Cleary is here, he can either use the podium or the microphone here at the front of the room.

Apparently, Mr. Cleary decided that the cocktail hour was more important than his comment.

The second presenter is Mark Peters.

PETERS: Mark Peters. Oh, believe me, I'm not going to stand in the way of beer. This is going to be very, very, very brief.

I wanted to make a very brief comment for the record, in line with the comments and questions from the Board related to the State of Nevada experiments earlier, I wanted to make it very clear that DOE's position is that their experiments are not representative of what would happen in a repository. And, you're going to hear a lot more about that tomorrow from our scientists. But, again, not representative of what will happen.

Thank you.

DUQUETTE: Thank you, Mark.

That concludes this afternoon's meeting. We'll see you all at 8 o'clock tomorrow morning.

(Whereupon, the meeting was adjourned.)