UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING MEETING

May 13, 2003

The Watergate Hotel
2650 Virginia Avenue, N.W.
Washington, DC 20037

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. Daniel B. Bullen
Dr. Thure Cerling
Dr. Norman Christensen
Dr. Michael Corradini, Chairman, NWTRB
Dr. Paul P. Craig, Session Chair
Dr. David Duquette, Session Chair
Dr. Ronald Latanision
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
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NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
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Linda Hiatt, Management Analyst
Linda Coultry, Management Assistant
Davonya Barnes, Staff Assistant

CONSULTANT

Dr. William Melson

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PROCEEDINGS

8:05 a.m.

CORRADINI: Good morning. It's my pleasure to welcome you to the spring meeting of the U.S. Nuclear Waste Technical Review Board. My name is Mike Corradini, and I'm Chair of the Board.

As many of you are aware, the full Board meets three to four times a year. We hold many of our meetings in Nevada to provide the citizens there with an opportunity to observe and question the material that's presented. We also try to hold one meeting a year in Washington to make it more convenient for our federal congressional decision makers to attend, and that's why we're here today.

As many of you know, the Board was created in the 1987 amendments to 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.

The Board is required to report its findings and recommendations twice a year to Congress and to the Secretary of Energy. The President appoints Board members from a list of nominees submitted by the National Academy of Science, and the Board is, by law and design, a multi-disciplinary group composed of eleven members with a wide range of activities.

Let me introduce the Board members again to you today. As I introduce them, I would like them to raise their hand to be identified. Let me remind you that we all serve in a part-time capacity. We have other jobs. In my case, my full-time job, I am Chair of the Department of Engineering Physics at the University of Wisconsin, Madison. My area of expertise is in nuclear industrial safety, with emphases on subjects involving multi-phase flow and heat transfer.

Mark Abkowitz is a Professor of Civil Engineering and Management Technology at Vanderbilt University in Nashville, Tennessee, and is Director of the Vanderbilt Center for Environmental Management Studies. His expertise is in the areas of transportation, risk management, and risk assessment.

Dan Bullen is an Associate Professor of Mechanical Engineering at Iowa State University. His areas of expertise include performance assessment, modeling, and materials science. Dan Chairs our Panel on Repository System Performance and Integration.

Thure Cerling is a Distinguished Professor of Geology and Geophysics and a Distinguished Professor of

Biology at the University of Utah in Salt Lake City. He is a geochemist with particular expertise in applying 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 University. His areas of expertise include biology, ecology, and ecosystem management. Norm Chairs the Board's Panel on Waste Management Systems.

Paul Craig is Professor Emeritus of Engineering at the University of California at Davis, and member of that university's graduate group in ecology. His areas of expertise include energy policy issues associated with global environmental change.

David Duquette is Department Head and Professor of Materials Engineering at Rensselaer Polytechnic Institute in Troy, New York. His expertise is in physical, chemical, and mechanical properties of metals and alloys, with special emphases on environmental interactions.

Ron Latanision is a Professor of Materials Science, Professor of Nuclear Engineering and Director of the H.H. Ulig Corrosions Laboratory at MIT. His areas of expertise include materials processing and corrosion of metals, and other materials in different aqueous environments. Ron is also a founder and Chairman of the MIT Council on Primary and

Secondary Education. He Chairs the Board's Panel on the Engineered System.

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

And, Richard Parizek is Professor of Geology and Geoenvironmental Engineering at Penn State University, and he's also President of Richard Parizek and Associates, Consulting Hydrogeologists and Environmental Geologists. His areas of expertise include hydrogeology and environmental geology.

So, that's all of us, and the staff. I'm not allowed to say that, but it's an excellent staff, so I want to make sure you're aware of them all, and you will meet them as we go along.

Let me now turn to our meeting agenda. First this morning, we'll hear from Dr. Margaret Chu, Director of the Office of Civilian Radioactive Waste Management. Dr. Chu will update us on the status of the Yucca Mountain Program.

Following her presentation, John Arthur, Director of the Office of Repository Development, will present an overview of project activities, including long-range plans and project priorities for science and engineering.

After a brief break, we move to the central purpose of today's meeting. As you may be aware, in various reports and public statements over the past decade, the Board has expressed concern about the uncertainties created by the effects of high temperatures on the repository system. In the last year or so, several things have happened that are relevant to the issues of thermal effects on repository design.

For example, in February 2002, the DOE submitted its site recommendation to the President, which included options for both high and low temperature repository operating modes. More recently, new data related to the corrosion of Alloy 22 at elevated temperatures have become available. These things have sharpened the Board's attention on this issue.

So, today, the Board has asked the DOE to describe clearly the thermal aspects of their high-level nuclear waste repository design and operating mode, how the thermal aspects of that design and operating mode were analyzed for waste isolation, and the results of those analyses.

To address that question, the DOE has assembled a connected series of in depth presentations. Particular speakers and topics will be described in more detail by our Session Chairs, both today and tomorrow.

And, now, for a few important business items before we begin. First, the Board values public participation, and, so, we have given the public a variety of ways to comment during the meeting. 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 Linda Hiatt are seated. Where are they? At the very back outside. Outside, okay. I was looking for them.

Most of you that have attended our meetings know that we try to accommodate everyone, but as you can see, we have a relatively tight agenda, and depending on the number of people who wish to speak, we may be forced to ration our time. As always, you are also welcome to submit your comments in writing for the record. If you have questions that you'd like to have the Board ask and that relate to topics being discussed, please give them to Linda Hiatt or Linda Coultry, and we'll ask the questions if time permits.

Second, I want to invite any of you who are interested to provide comments on the Board's updated strategic plan for fiscal year 2003 through 2008, and its performance plan for 2004. Copies of both are available on the documents table in the back of the room, or on the Board's website, www.NWTRB.gov. We welcome your suggestions for improving the way the Board conducts its meetings, and the topics on which it will focus in the coming year.

And then, third, I have to offer our usual disclaimers 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 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. But I have to emphasize that when we speak with our, I say extemporaneously, they're speaking on behalf of themselves and not on behalf of the Board. When we are articulating a Board position, we'll be sure to let you know. Also, when Board positions are stated in our letters and reports, they will be available at the Board's website.

Finally, I'll ask all of you who have not already done so to please switch your phones and pagers to silent mode. Or else.

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

Margaret?

CHU: Good morning. Thank you for giving me the opportunity to provide an update on our program.

One important activity that I've engaged in since our last meeting with the Board is what else, budget, is the support for the program's fiscal year 2004 budget request before the Congress. In my budget testimony, I stressed that the program's key overall objectives are to submit a high quality license application in December 2004, and to begin receiving and emplacing waste at an NRC licensed Yucca Mountain repository in 2010.

And these goals remain, despite the final resolution of the fiscal year 2003 budget under which we have received $457 million, which is $134 million less than our request. Coupled with five months of continuing resolution at the beginning of fiscal year 2004, it was a very drastic reduction and impact to our program.

So, the Department of Energy and then our contractors have reviewed the program priorities, and are re-aligning the activities to reflect the available funding. Obviously, some activities will have to be reduced or deferred. Later, John Arthur will summarize for you the results of this replanning and what it means.

For fiscal year 2004, we requested the same amount as '03, which is $591 million. This request includes funding for completing the technical products required for licensing, developing the license application, performing the work for Nevada and national transportation, and conducting scientific and technical work to achieve cost reduction and systems enhancement.

In my testimony, I also indicated that the program would not be able to meet the 2010 waste acceptance objective should funding fall below our requested level for '04.

We're continuing to work towards submitting the license application in December of 2004. The key activities include designs, total system performance assessment, preclosure safety analysis, and then addressing the key technical issue agreements.

Another key activity is the certification of the license support network six months before we submit a license application. You all know this is the electronic database of all the work, all the relevant work from the program. We now have a contractor on board to support our license and support network effort. This company has extensive experience in preparing electronic documentation and databases, similar to the LSN. So, I'm optimistic about meeting our schedule in this area.

Since I briefed you in January, I have also continued to implement our plans for strengthening our program organization as a disciplined culture that values safety and embraces quality. Our program's new leadership team is almost in place. At the last meeting, you met John Arthur, our Deputy Director of Repository Development, based in Las Vegas. Since that meeting, John Mitchell, who's somewhere in the audience--yeah, John Mitchell back there has taken charge of BSC as the new President of our M&O contractor. And Mike Mason has taken the position as BSC's new Quality Assurance Manager.

In addition, we have Malcolm Knapp recently joining our DOE team. Malcolm, whom some of you may have known, has extensive experience from the NRC. Also, I'm looking forward to announcing in the very near future the selection of individuals to fill the final three important positions, Deputy Director at Headquarters, Transportation Director, and a Systems Analysis Director. I hope to announce them in the very near future.

I intend this management team to represent a significant new way of doing business, and to lead the program successfully through license application and beyond.

Last month, I issued the Phase 3, that's the final phase of our program manual, which defines roles and responsibilities for each functional area in the organization, and emphasizes formal relationship between DOE and the contractors.

In a related area, DOE and BSC initiated a Rolling Quality Focus process to ensure that the leadership team's vision, expectations and commitments on quality are understood by the whole organization. John Arthur will be telling you more about this initiative, and other management tools he's using to ensure that our licensing activities are progressing well.

Now, I'd like to touch on some of the topics that our speakers will be addressing in depth for the meeting. As you are aware, the project is in the process of updating and finalizing our technical basis and support of the license application. This includes updates of analysis and model reports, AMRs, and the Total System Performance Assessment in the postclosure period. Along with updating each of these pieces, we are paying special attention to putting them together into an integrated "story" of system performance.

While it may be natural or necessary at first to analyze discrete elements of performance, it is very important to be able to eventually assemble all the related pieces to develop an integrated and a realistic picture of, for example, the near field environment. We intend to continue to mature these types of integrated story for the total system as we move forward to license application to present an integrated view of system performance, and to communicate effectively to a variety of stakeholders.

I believe this integrated approach will also provide insights to the resolution of the KTIs. Accordingly, the majority of today's presenters will focus on an integrated discussion of performance of the unsaturated zone in-drift, particularly focused on coupled processes, in-drift chemical environment, and localized corrosion. I'm sure there will still be questions after today's presentations, but I'm hoping this integrated approach will begin to provide the logic, explanations, and the assumptions for the evolution of the near field environment.

As I said previously, our priority is to submit a high quality license application by December '04. I want to emphasize the importance also on the ongoing science in our program. Mark Peters will provide an update on the ongoing testing program tomorrow. The performance confirmation program will continue throughout the preclosure period, and it will be a condition of the NRC license. Detailed planning for this program is ongoing, and we stand ready to provide updates to the Board in the future meetings.

You are well aware of my commitment to a long-term science and technology program. We're making good progress in developing the science and technology program. You will hear more on this topic from Bob Budnitz tomorrow. The Board has previously noted the challenges we face in working within a very constrained budget environment, in which trade-offs and reductions must be made somewhere.

While we are focused on the preparation of the license application, I recognize, and in my congressional testimony, I have stressed the importance of providing adequate support for all of the pieces of the puzzle that we need to come together, including transportation, science, technology and others, as well as, of course, the license application development. As we continue to adjust our plans to reflect our current budget outlook, and look forward to the FY '04 appropriations, I will keep all of these priorities in mind.

Finally, let me update you on some of the topics that are not on today's agenda, but in which I know you are interested. In the areas of design, I want to mention that we are making good progress toward finalizing the design, especially for the surface facility. We are also moving forward rapidly in the area of performance confirmation. I hope to be able to brief you on these topics in the very near future.

Now, we're currently looking closely at our transportation plans. You know the development of a transportation system is one of the most critical elements for a successful repository program. It is also one of the most challenging, as it involves development of the infrastructure, fleet acquisition, cask requirement, logistics, and support management and emergency response readiness.

I want to thank the Board's Panel on the Waste Management System conducting a productive meeting with us in February. As the Board's Panel has noted, engagement with stakeholders in the development of the transportation plan is essential, I agree. Our transportation plans must maintain flexibility and offer the ability to handle contingencies. We're working to advance these plans and communicate the plans in a way that will make stakeholder engagement most productive for all concerned, and then actually I'll make sure the stakeholder involvement is one of the budget elements in the '04 and '05 and beyond budget in the transportation space.

And then I hope to report to you more definitive activities in the transportation area in the near future. And I think when the new Director comes on board, and it will be about the time we finish a lot of the scenario analysis we're doing right now in transportation, and like I said, the important thing is given all the uncertainties down the road, the timing and everything, I want to make sure we do the right planning, with enough flexibility and contingencies so that we always get there.

I appreciate the Board's continued involvement as we move forward, and I will be happy to answer any questions.

Thank you.

ABKOWITZ: Abkowitz, Board.

I have a couple of questions on transportation. I know that it's still undergoing some sort of big picture thinking with the agency. You mentioned at our meeting I believe in January, and I believe it was reiterated at the

Panel meeting in February, that there was a strategic planning process underway, and that a strategic plan was going to be prepared and available I guess sometime this summer. So, I was curious to know whether that is still the schedule.

And, you also mentioned a commitment to, you know, greater and continuous stakeholder involvement, and I was also curious to find out when those stakeholders were going to be invited to start participating in this process.

CHU: The strategic plan, our plan is to issue by the end of the fiscal year, so it will be the September time frame. That's our plan. Okay? And then we will get the stakeholders involved before we finalize that, so they will have a chance to work with us on the draft. And then that's one of the things we're looking at. There are a couple of opportunities between now and the next couple months, we'll probably start that process even before we write the draft, and get some of the key topics out in some kind of form. That's what we're planning right now to get stakeholders' input on these.

And part of the institutional plan is--not institutional--the strategic plan is we're hoping to have some companion documents, like institutional plan, which is really the stakeholders' involvement on that, and then hopefully, there will be a business plan, which I can't promise you the timing, you know, how it relates to the strategic plan, but we would like to have a business plan and talk about acquisition plan, like how do we plan for the procurement part of the transportation system. Because we're going to involve a whole bunch of procurements from DOE on casks, on fleets, on, you know, HAZMAT, so we want to make sure all these are thought through. And we call that the business plan. On the side, there's an institutional plan that's the stakeholders' involvement, who and what and when and how, how they become part of the eventual emergency plan. And then they sort of get synthesized. At least that's how I view it, they get synthesized into a strategic plan. So, that's a rough plan right now, and then our plan is still get the strategic plan out by the end of the fiscal year.

NELSON: Nelson, Board.

Margaret, you spoke about an integrated story of system performance, having a realistic picture, accenting on integration and putting the pieces of the puzzle together. I'm wondering what does a safety case mean to you? And will there be something called the safety case created?

CHU: I wouldn't use that term, safety case, because there's so many interpretations of what that term is. To me, what I call the story is really how do you communicate a complicated system that has so many pieces, and we have put our work and scientific work together, and how do you pull all that together and explain. And, for example, I'm hoping today you will get a feel of what our approach is, is what is the evolution of the near field, what are the pieces that support that picture.

So, I'm hoping, you know, eventually, if not by the end of this meeting, is, you know, down the road, and people understand, yeah, okay, this is how the, from the unsaturated zone down to the in-drift environment, these are the influencing factors as a function of time, and all the thermal conditions, chemical conditions, we pull that all together. These are the dominating things, and these are the things that may or may not happen, but we believe these are important things to happen. Therefore, we can't ignore it, and these are the things that will be more important as time goes on, and that you describe the whole evolution and then present the logic and the evidence behind it.

I'm sure there's still issues in the whole approach, and in the details. You know, there may be gaps here and there in the story, but I'm hoping to pull it out together so we all can talk about it, because I know there are a lot of issues related to the near field environment. That's why we picked that topic, and it addresses a lot of the thermal issues, and it addresses a lot of the corrosion conditions, and what is a realistic condition in the near field from day one to 10,000 years, and why we believe that's the evolution, and what is the evidence that supports that picture.

And then I don't know if you want to call it a safety case or not, but I wouldn't call it a safety case, because safety case has its own meaning, I think, outside. This is just the integrated story.

NELSON: Nelson, Board.

Many other projects are using the concept of a safety case and a document that might be called a safety case. Would there be a document that would do substantially the same conveying of information? I mean, the TSPA by itself doesn't tell the whole story. So, will the project produce a document that effectively does that?

CHU: We haven't thought through that. You know, the way I look at it, I'll see how it goes. And it could be in some kind of summary document, or it could be part of the license application. I don't see why it wouldn't be there, some kind of summary, you know, at certain chapters. I really don't know right now, depending on how it gets pulled together, whether it does convey the message, as it should, and whether it communicates well with this approach. So, that's where we are. We'll see whether it works well or not, and then the Board would be telling us whether it's working well.

PARIZEK: Parizek, Board.

I was just going to pick up on the same theme, and that is I think the international community always looks for a safety case, and it's more than TSPA, and without one, will we have the same credibility as a nation as if we had one and it was well put together. So, I would sort of endorse it.

The other question, though, was related to the budgeting for the stakeholders. Do you visualize money being provided to stakeholders so they get involved in this process actively, or do you feel they will be there in any event, just as long as you allow time to do this?

CHU: You know, in the past few years, we have been providing a small amount of money, what we call cooperative agreement funding to groups of folks. And then these just attend meetings and talk to us. These are like $50,000 per year range. They are a small amount. But we are revisiting all of that to see what it means. You know, do we want to extend to a broader community? This is all part of that institutional plan.

PARIZEK: There are many people who have a problem attending the meetings where they're held, and even for the Board to go to different locations to seek out people's concerns is expensive for people. So, you're thinking about some budgetary help along these lines.

CHU: Right. Yeah. So, this is all part of a plan. We going to see what interactions we need, you know, what's possible, will these people be able to come, and then this is all part of that.

PARIZEK: Thank you.

CHU: Thank you.

DUQUETTE: Duquette, Board.

Margaret, would you be willing to share with us how you anticipate taking the 20 or 25 per cent cuts that you're talking about? That is, will it be across the board, a reduction in funds for all of the programs, or are you planning on cutting out some of the programs, and so on and so forth?

CHU: It's 22 per cent. Yeah, John Arthur will actually give you a rundown, a detailed, fairly detailed rundown, which areas we're going to be cutting, which areas will be deferred. And, so, John, right? You're going to give a fairly good summary.

DUQUETTE: Thank you.

CHRISTENSEN: Margaret, Bill may be covering this, but in many of your past presentations, you've emphasized your support for maintaining a science program, and I just wonder in this general mix of things, where is that among your priorities? How is that likely to fare in the current budget situation?

CHU: You know, for '03, we requested only $2 million for current year, because we've just barely started. And we're basically maintaining that. I think it's very close to $2 million. Bob may be able to tell you, 1.8 or 1.7 that we are keeping. And for the '04 budget request, we're putting in a $25 million request. And it then depends on what happens with the appropriation, and so on. And for '05, which we're actually starting that planning process, we are thinking of even up to maybe $30, $35 million range. So, we'll see how reality impacts our decision, but it is, like I said, it's a very high priority for me. And then I will do the best I can to keep a healthy science and technology program going.

CHRISTENSEN: Thank you.

CORRADINI: Dan?

BULLEN: Bullen, Board.

In your comments, you mentioned that you value safety and you embrace quality, and I wonder if you could comment on the recent articles in the press about the stop work order and the QA challenges, also realizing that it's beyond the purview of the Technical Review Board to worry about quality assurance. But, I guess it just harkens back to the ability of the public to accept the program. So, could you talk a little bit about the QA problems or challenges, I guess you said?

CHU: Yes. Let me first, you know, explain this quality assurance program a little bit. Some of you may or may not be familiar with it. The Yucca Mountain program is under a very special, highly rigorous quality assurance program, which is for nuclear activities, like reactors or nuclear disposal programs. And what it entails is, you know, the technical work has to be we call it R2T2, okay, from my WIPP days. It really means all the technical and scientific work, and analysis and data have to be retrievable, reproducible, traceable, and transparent. I'm kind of using the layman language. That's the spirit of the nuclear quality assurance program.

So, that means the scientific work has to be documented at a very highly rigorous procedural fashion. And, so, one point I want to make is when scientists and engineers, when they do not conform to these procedures, may times they don't necessarily mean there's a defect in the work itself, but these other actions that need to be corrected, because it is required as a licensee for NRC. That's one point I want to make.

And the second thing is because this rigorous quality assurance program, and then we have a program that has been approved of by the NRC, and that we have on-sight daily, almost daily oversight from them, and as part of this program, it's a requirement actually by the QA program that there will be a lot of inspections and audits, and it's expected to have a lot of deviation from the procedures. I'm not saying a lot of deviation is good, but as part of the program, you will self-identify issues, and you correct them.

And since our program for like 20 years was a scientific program, and then we're transitioning into this highly rigorous I call it nuclear culture, yes, we do have problems, we do have problems for people following this rigorous procedure, and we are trying very hard making sure they understand that, and making sure it is essential and necessary, absolutely critical for them to comply with these procedural requirements.

And then what we are doing, that's really, you know, in a way one of the reasons I did the reorg and bring in new leadership, because it's a cultural thing. So, I don't like the Bandaid approach, because I don't think it works. So, I want to have a more sustained, longer term cultural embracement of this nuclear culture, because there's just no other way I know how to do it, except for cultural change. So, that's what we're doing.

And then are there deviations from compliance? You bet. But is it getting better? You bet. We see encouraging science and trends, that it is getting better. Is it getting better at a fast enough pace? I don't know. We're trying very hard, and John Arthur and John Mitchell, and our other new QA managers, they got brought in to make sure it gets turned around fast enough. But, does that mean our technical work is flawed or incorrect? I don't believe so. Because we have a lot of ongoing review oversight from the Board members and from the National Academy of Science and from a lot of peer reviews, domestic and international. These are the technical oversights.

And then, you know, there are issues, technical issues, yes, they are open, they are being addressed. So, I don't think the QA issues have compromised our technical work. But, there are a lot of remaining cultural things I need to fix and improve, and it's very high for the management team, and I'm optimistic that we'll get there. I am hoping there will be less violations of the procedures.

Also, another important thing I want to stress to everybody, we will not deliver a license application until every piece of data, every software are QA'd according to the NRC requirements. So, that's very important to all of us. I think the whole troop understands that. We'll get there, you know, when we're ready to deliver. If we're not there, we won't deliver, because I can't deliver something that doesn't comply with NRC's requirements.

ABKOWITZ: Abkowitz, Board.

I want to follow up on the comment, Margaret, that you just made. I appreciate your concern for the QA, but if the license application is still planning to be submitted in December of 2004, there's a point in time before that where you basically say we go with the analytical work that we have established to date. Do you see that you'll have enough time to implement the changes that you're looking to do to have the confidence that you're looking for, so that when you draw that line in the sand, you'll still have sufficient time to put the application together based on that information?

CHU: Like I said, you know, we have to monitor the situation very closely between now and then, and then we have to make a management decision down the road and say are we there or are we not there. And I can assure you we're going to make a high integrity decision when the time comes. If we're not there, we're not there. When we are there, we're there. And we will report to you and then, you know, like I said, we get a lot of oversight from people, so I think the situation we get will be monitored by a lot of people, not just me alone, and it will become apparent whether we are improving rapidly in the QA area, or are we not.

And then, like I said, I believe we have the new team in, they are extremely experienced people, and we're going to walk the talk, and I'm optimistic. I'm always cautiously optimistic. Of course, you know, you have to always go in that direction. If you don't try, you never know whether you'll be successful. And that's our attitude.

Thank you.

LATANISION: Latanision, Board.

The corollary to Mark's question also affects the science and technology program. Much of the work, and especially the ramped up budget, will occur post-licensing application. And, so, I'm wondering is the philosophy to incorporate findings that will emerge from that program as amendments to the application if they're appropriate? Or how do you envision making use of the information that will flow from that program?

CHU: You know, this is a topic we talk a lot about internally. The answer is yes, if it's appropriate. If I may give an example? For example, the saturated zone, which we haven't taken a whole lot of credit, we all know that, and then if there is some new information after we submit the application, we feel if it's appropriate as part of the review and defense of the license application, we might put in an amendment. You know, it probably wouldn't be an official amendment. It would probably be additional information, and we'll put it in for NRC to consider.

And then whether it's positive or negative, whatever the new information is that's relevant to the license application, I think will be sent in as appropriate. That's how I view it. But I don't know how NRC will view it, officially how they receive this information, I'm not sure because I don't think they have mapped out a detail on how they receive additional information.

LATANISION: Thank you.

CORRADINI: Other questions?

(No response.)

CORRADINI: Thank you, Margaret.

CHU: Thank you.

CORRADINI: We'll now have John Arthur talking about an overview of project activities, including the long-range plans. John?

ARTHUR: Good morning, and I'm very pleased to be here in Washington today. And as discussed by Dr. Chu, our highest priority remains submittal of a high quality license application in December of 2004, but also keeping focus on the longer term goal of maintaining an opening date of the repository in 2010.

Included in this is completion of the necessary design work, and most importantly, demonstration of an operating environment, and in my words, management culture appropriate for a licensee.

I want to today just start with some successes since the last quarterly meeting we had in Las Vegas. First of all, the Department and NRC have now established an aggressive, we have actually seven meetings scheduled through the end of June, aggressive interaction calendar to continue critical discussions on KTI, the key technical issue agreements, one in June on the repository design, preclosure safety, and also the environmental impact statement process.

Margaret mentioned some of the personnel changes in both the Department of Energy and also in Bechtel, and I'm very pleased to have John Mitchell as a counterpart. John and I worked across the table from each other in the National Nuclear Security Administration years ago, so we've worked together in the past, and we have some pretty big challenges, as Margaret said, here on the horizon right now.

Also, in my office in Las Vegas, I'm in the process of interviewing and hope to select soon a permanent licensing manager for our office in Las Vegas.

One of the bigger areas we've done over the last three months is start a monthly operating review. In a project of this caliber, be it the scientific endeavors, the engineering endeavors, operations or planning, you need to have a clear summary on a monthly basis of where you stand associated with the costs, the schedule, and also the technical aspects of the program.

This is one right out of our last report, and actually this is out of a report we did about two weeks ago that actually shows percent complete on a license application. First of all, this is our management assessment from Joe Zigler and Nancy Williams, the DOE and Bechtel counterpart of license. What we've done is broken out the major components of the license, put the percent completed.

In each of these areas when we say complete, it includes not just the technical assumptions and the planning, but also the appropriate level of quality required. And then, also, we have a weighting for each of those areas, anywhere from 10 to 30 per cent. And, right now, our assessment is 16 per cent complete on the license application that we're targeting for December of '04.

If you move into the next graphic, and this will have some colors. Across the whole business, everybody has a different way to grade this. But, green means everything is on schedule, within the right cost categories, no major variances, and also technical aspects are working very well. Yellow means that there are some concerns, but they are resolvable, and you can get back into the green with proper management emphasis. Red is not a failure mode. It means a lot of management attention is required. There's either a significant cost schedule, and most of these cases I'm presenting, it's a combination of cost and technical issues.

I just want to talk on a few of these here. Time won't permit me to cover all these, but I just want you to know that I applaud our Department of Energy and Bechtel managers, because I think they've done an honest assessment of what's working well and what's not working well right now. And I just want to cover a few areas. On your left there, if you go into Commitment Management at the bottom of the

License category, this has to do with the realignment. We're behind schedule on some of the critical key technical issue reports to NRC. And the replanning that I'm going to talk about in a few minutes, we're trying to repackage those to be better for how they're presented to NRC in the future, and also to align them to a better schedule. But, right now, we're carrying that one in the red because it requires a lot of discussion and action underway.

If you look down the Safety Analysis column, total systems performance assessment, TSA work and model input are behind schedule. We've had some challenges with the engineering barriers system and waste package degradation areas, inclusion of the localized corrosion model. And these are the words you see right in our report. Again, it's not just to raise the issue, but also I expect each of our managers will say about a path forward, who's in charge, and what we're doing to try to get things back on track.

One other area I would cover would be Surface Facilities. And our design is proceeding, but with the continuing resolution and reduced funding for this year, we're trying to get things back on track. We just, through Bechtel announced word of a surface design support contract last week to bring in some unique expertise to help us on the surface design. Tied into that, if you look at the preclosure safety analysis, the majority of that analysis is associated with surface design. So, you see a close tie between getting the design back on track, and the preclosure safety analysis.

So, in summary there, I want you to know we're looking at it monthly, trying to put some management discipline into it, accountability, and also recognize where things are going well and where we have challenges.

A couple other areas I'd like to discuss. First of all, another couple successes, we did issue the quality assurance requirements document, Revision 13, and that has been accepted by NRC. That's the guiding document for our entire program, and all aspects of implementing NQA 1. And, also, we've put added management emphasis through John Mitchell and his team at Bechtel on working off actions, two major corrective actions that have been behind schedule for a long period of time, one on the model validation and the other on software qualification. We do now that schedules for the August and September 2003 time frame to close those out. Those are critical. Those are the heart of the license. In other words, instead of trying to review and approve these models and data, we want to make sure that we have the right processes so they're coming out of the pipeline with quality built in the first go around.

Again, rest assured before anything goes into the license, we're going to make sure that the right pedigree of quality is built into those aspects.

Two other success stories. We successfully completed an evacuation and reentry of the exploratory studies facility in January. But, we had some degradation of some of the electrical systems, a short in one of the lines that required three different, first of all, we had visitors at the site that required an evacuation, and I was very proud of our mine rescue team. They did an outstanding job to go in and do the necessary actions after the fact to determine the cause, and we have actions underway to improve that right now.

Also at the site, we did a full stand-down of all the electrical safety. We had some issues and concerns, and timely management action by Bechtel/SAIC definitely improved safety and technical qualifications of the workers. Work is back proceeding in that area.

Okay, now onto the big topic of discussion, and that's the budget and planning and what is a replan and why does it occur. First of all, if you look at the program, as Margaret said, we originally requested $591 million for the fiscal year '03. We operated for the first five to six months of the year essentially at a flat-line level of what we had in fiscal year '02. So, you had a wave of work being deferred. No matter what you do, you have some work they were planning at a higher level, and that was deferred.

Also, when the funds finally came in, we were about $130 million, or so, in the red. So, I think it's been a major task between our federal staff, the labs, the USGS, Bechtel and all others involved to really take a hard look at the program, first of all, to keep a quality license application first and foremost, but also not do anything that impacts post-license application. And what I tried to do was give some realistic planning assumptions to Bechtel/SAIC. Instead of planning at a high level and then coming back down, plan, and then if Congress decides to fund at a higher level, I'll be glad to accelerate work. And I'm sure John Mitchell and others would be, too.

So, the numbers I gave them was $295 million for this fiscal year, and I believe it was $330 million for fiscal year '04. I expect to make final decisions when I get back to Las Vegas on this this week, because you have to remember we have four months left in the year, and if we're going to let go of some things in order to get the funds for other areas, we need to move very promptly, because time is of the essence.

Areas that DOE and Bechtel will maintain and support include, first of all, rigorous health and safety program compliance with all applicable permits, preparation of a complete, high quality license application, with all the requirements of 10 CFR 63, and also the Yucca Mountain review plan, addressing KTI agreements by the time the license is submitted, and I'll be discussing that more a little bit later, ensuring that the repository and the waste package design information is fully adequate for submittal. The total system performance assessment and preclosure safety analysis both will be technically defensible and with proper quality assurance controls. The license support network development in support of certification six months prior to LA submittal is one I had read a little bit earlier, because of trying to align some of the relevancy criteria. This is of major magnitude of assessing all the key documents. But, again, we will make sure we have the proper funds to do that work correctly.

Probably one of the most important ones, and you probably read in the media some of the challenges we have, and I know Margaret mentioned that on corrective action program, maintaining a safety conscious work environment, nuclear culture and QA program. We will not sacrifice one dollar in that area. We will make sure everything is moving in the direction to maintain the right operating environment.

Site operations, we will maintain a minimum site safe operations, and limited site access. We are going to reduce and shrink the footprint of the site to that that is critically needed to do the work. I know at one time, we were offered options as far as shutting the whole site down, which really no one wanted to do. We know the importance of all the work that's gone on. We're trying to get more efficient in maintaining and managing the site.

Also, with that, we'll still have a goal to try to keep the site open for the public and critical tours, but it won't be at the rates that we're doing now. We'll reduce that to a lesser amount of days per week. I just can't get everything, I mean, we've been down to the $100,000 level, or even $10,000 level, in this replanning effort, so we're trying to make sure everything fits into the right package, and we maintain the maximum priorities.

Areas that will cease and resource reductions, there will be some personnel reductions across the system with this, some reductions in force. And, as I mentioned, at the site operations, we'll reduce the experimental footprint, partial but limited site access for tours, and also access for critical experiments. But the site operations, we're going to defer new tests, again hopefully in '04, '05, depending on budgets, we'll get some of those started. Most cases, these would be areas that are needed for our performance confirmation. These tests are not directly tied, in our mind, to either the key technical issues agreements or the license application. But, in doing this, we do accept a moderate risk, and we fully realize that.

I just want to give you an example of some of those tests. The cross-drift heater test in Alcove 10, seepage test in Niche 6, crest to alcove in Alcove 9, the Calico Hills Unit drilling and testing, again, these are not directly tied to KTI, but would provide additional confidence with parameter distribution concept models later. So, some of this we hope to get back on track in the '04, '05 time frame. And then, also, the alluvial tracer complex. This one also right now is tied into a key permit issue.

In the preclosure safety, we have had some slippage in our schedule, as I mentioned a little bit earlier, tied into the design, but we feel we can still get that back on track.

Environmental safety and health, we'll reduce some of the support levels required. But, again, we're still going to maintain safety and health, but have some reductions in the area. And information technology, we'll have to eliminate any new systems development, reduce planning.

As you look at it, it sounds pretty challenging, but, again, we feel that we have the right blend of the program to proceed, and I guess we'll have to wait and see where we are again in '04. Please recognize that, you know, final decisions are being made on this, and I hope to get a letter back to John Mitchell either late this week or early next week. With that, in a meeting with the Nuclear Regulatory Commission we had in Las Vegas several weeks ago, there was a request to show, you know, what the current program is, and then what this replanning does as far as the future. And we have committed to share that with them, and I will share that with you also, offer it to the NWTRB. It should be available in another week or so. It will just show, you know, all the critical work originally planned versus what's deferred and how that ties together, because the NRC obviously has a lot of interest in this also.

Okay, if I can move away from replanning for a few minutes, I want to talk about another success that's happened. And when I met you at the first meeting in Las Vegas, I said we were going to start some efforts, something similar to what we did at the Waste Isolation Pilot Plant back in the Nineties. A project of this complexity requires a fully integrated schedule with all of your logic, all of your assumptions, not just the license, but transportation, science and technology, land acquisition, interface with other agencies, and I'm pleased to say we've started those efforts, and right now, the schedule, and you see just a snapshot of the schedule, it's in your handouts there, we're about 60 per cent complete. And I hope within the next month and a half, to have that completed and then be able to share it with the Board, NRC, other critical stakeholders, because we want to receive input on this.

This schedule, at one time, we had a noble goal of trying to lay it out to 2034. My staff told me it would be a little bit too ambitious. How about let's just focusing on a 2011 date, and that's where the initial focus was. You can look on the schedule that you have there, and it will just give you a snapshot. This is probably about one-thirtieth of the total. This sheet would probably fill about the size of the whole panel here.

And, again, I'll just emphasize a few things. If you go down on Engineering and Design, and you go across the bottom line, you'll see one that says Freeze Design Products for the License in March of '04. Our plan is to complete the license application design in May of '04, and then you can see a feed that goes up to Licensing and across, and all this goes up to the top line of NRC receives a license application submittal and LSN recertification, December of '04.

Obviously, this is one critical area of emphasis and focus, but there's a lot of other areas underway, transportation, other pre-areas, site planning. You know, as we had this meeting, we have to run an environmental analysis parallel to our license application to make sure that we have the proper NEPA planning. Our original FEIS was either correct or we have to do a supplement. And, in the meetings, it was an eye opener to us, our NEPA expert said, well, I need to have this information by a certain date, and then we said, well, we need to have a site construction schedule in order to analyze those impacts. So, at least this analysis is forcing us as a team to bring the logic and assumptions together and realize where we're connected, and also where we have a disconnect, to try to timely work that.

There's a lot of other background. When I do present this to you, there will be hundreds of assumptions and logic in other key areas, or the foundation by which we plan this program. So, again, it's coming together well. I look forward to receiving comments once we put this out. It will probably be really July.

And then after that time when we feel we have it correct, then we'll do what are called resource loading of the schedule, which is the major effort where you really align this to your budgets and other key areas. And then we've fully achieved one of our major goals of a fully integrated program from our schedule planning and resource planning. But, I can't overstate how complex it is to do this, but we'll get through it, and I look forward to receiving comments as we proceed.

Another area I want to talk about for a few minutes is safety conscious work environment. And it's a terminology in the NRC environment, I'm getting re-familiarized, having spent the last 24 years of my career in mainly the DOE projects, but I take seriously all of the concerns, and I speak for John Mitchell, Margaret and our entire team here as

I say this, I take very seriously the concerns that were reported in the media, and we do have several investigations underway right now. I should get reports on this in the next few weeks. Based on what comes out of that, proper actions will be taken. Or, if there is no requirement, if nothing comes out, then nothing will occur.

As Margaret said, you know, the cultural changes take time to do. When you look at the best-in-class in industry, it could take anywhere from a year to two years to really ramp up. And I would say that the first trigger, a lot of the changes occurred probably about last summer with the site recommendation report, you know, a major hurdle from 20 years of real detailed scientific data and collection and models and characterization, to really we're going ahead to proceed into licensing.

And then on top of that, we overlaid a new management structure, and we're trying to stabilize right now, is the best I can say, have to put the new management systems in, also to work with our team. I like to have them on success, but when there's an issue that has to be dealt with, be it disciplinary actions or other areas, rest assured that Margaret or John Mitchell or myself will take proper action.

So, it's going to take some time, and that's really the challenge that the Nuclear Regulatory Commission gave us the other week, to have the right operating environment by the time the license goes across the threshold in December of '04. And, we are planning to achieve that. Again, it will take a lot of emphasis as we continue to proceed.

A couple other areas, key technical issues and DOE and NRC interactions. And I believe this week, there's one key interaction occurring with NRC. As I said earlier, we have a very aggressive schedule. But, we do plan and schedule for addressing KTI agreements, and we are re-evaluating that in our replanning efforts, because we are behind schedules as we proceeded into this year. We want to try to get things on track, but also not create such bow wave that's impossible for NRC to review those critical ones before the license goes in.

We respect all the commitments that were embodied in the original KTI agreements and the significance and the importance of having these agreements addressed. Again, we're trying to look at the most efficient way to do that.

We have renewed an aggressive interaction schedule, because I think it's safe to say that both NRC and DOE are learning as a licensing agency and a licensing applicant as we work here, and we are trying to clarify and formalize goals and expectations for submittal of the license.

For example, four key technical exchanges are planned and occurring in May, and it's geomechanical issues, which has been completed, data and the quality assurance requirements document Rev 14, a volcanic hazards characterization, and also the one I believe that's underway this week is the use of risk information to address KTI agreements. And that's a real critical one, because we have some that are on hold pending adequate resolution of that particular area.

Let me now proceed into my summary. First of all, the design and understanding the thermal effects. And I could see in the first meeting, there was a lot of interest in the operating mode associated with the repository design and operations, and the license for construction authorization will be based on the design and operational parameters that maintain below boiling temperatures in the pillars between emplacement drifts following repository closure. But operational flexibility will be preserved.

Boundary conditions for the license application design will be established in June of '03. We have some interactions going on between Bechtel and ourselves right now, and the license design, as I mentioned up there on the schedule a little bit earlier, is planned to be complete in May 2004.

However, final decisions regarding ventilation, for example, the duration and use of natural versus forced, and other operating variables to determine the range of postclosure temperatures, and the associated in-drift environment, will be made after issuance of a license and adequate information is obtained through performance confirmation. So, the point I'm making is there's a lot of decisions we can move through, and I hope in time as we mature this decision plan more, you know, we may not know everything today, but we can show in the future where information would either provide it from confirmation or other key areas that might in the future trigger other license amendments required associated with the repository.

We recognize the importance of being able to demonstrate an adequate understanding of the environment in the drift, and on the surface of the engineered barriers and other factors affecting these environments. And, again, the presentations you'll see, I know our teams have been working real hard to try to lay out everything, and I look forward to the dialogue as we proceed.

In summary, we have completed, or are in the process of completing a major program realignment, and I think it's based on realistic assumptions. With that, as I said earlier, I know we accept more risk than what we would have had if we'd have had the full budget alignment. And, again, with that, we will have to see what happens in '04 as to what further actions may be required, or what further work can be accelerated.

Submittal of that high quality license application is our critical goal for December of 2004, but quality will be built in there, and we have had a number or series of meetings with our senior managers over the last six weeks to state that it's quality first. You have to have a proper balance of quality and schedule, but at the same time, we will not proceed with a license until all aspects of quality are built in.

And, again, as I showed you, I tried just to give you a glimpse of it today for our monthly operating reviews. We are trying to do an honest assessment of where we're doing well, and where we have issues. I know a lot of my staff and others have said, boy, it's red, it's red, it's failed. I said it's not a failure. It recognizes there's an issue, and it recognizes who's accountable to try to make the right level of improvements. So, we are trying to get systems in place to give us a better I call it a finger on the pulse of the overall program.

So, again, I thank you for the opportunity to talk with you today, and I'll look forward to entertaining any questions.

CORRADINI: Dick?

PARIZEK: Parizek, Board.

For clarification, on Page 3, you didn't define the white boxes. You've got green, yellow and red.

ARTHUR: Thank you. In our areas, no assessment currently is underway in those areas. And some of those will come into that category as more work starts to occur. So, white means no assessment is currently underway.

PARIZEK: Then I didn't know if I could recognize which was the transportation plan. Is it under the engineering section?

ARTHUR: I'm sorry. That's not included in here. This is just the license and the areas under us in the Office of Repository Development, Las Vegas. We are maturing data on transportation and other key areas on a parallel path to this.

PARIZEK: So, there's another chart that would include the whole system?

ARTHUR: There will be. There isn't right now, but there will be in time, yes.

PARIZEK: And I didn't understand when you mentioned something about the drift scale experiments being deferred. I mean, there's certain projects underway that you don't want to interrupt because you're in a cool-down phase. Did you say that there would be certain work not done on the drift scale experiments at this time? Because, I mean, here's this cooling down, and you don't want to lose data because it took four years to heat it up, four years in cooling down. And that's for the alluvial testing, which you haven't started, you know, experimenting, and you need that data also.

ARTHUR: Our plan, and Nancy or someone can correct me I'm wrong on this, Nancy Williams, but our plan is still to collect critical data on those experiments. Nancy Williams, if you could just stay up to the mike and help me? Nancy is in the heart of all the details of what I'm presenting.

WILLIAMS: In fact, the cool-down testing data is still being collected. So, ongoing tests are being collected and continued. It's the new testing that's being deferred into '04, '05.

PARIZEK: Thank you.

DUQUETTE: Duquette, Board.

I just want to understand a couple of things on your presentation. It really has to do with the question I asked earlier. I got the impression from what you said is that you've been able to absorb most of the cuts in your proposed budget at the site, and that the science and engineering background that will be required for license application and after that probably will not be very much affected. Am I correct? Is that a correct interpretation?

ARTHUR: There have been other impacts, too, David. One has been we're reducing some of our indirects, some of the indirect work force associated with the program. So, it's not just at the site. Other areas are being impacted also. But, we are trying to transition into the right level of engineering and design disciplines required for the license.

DUQUETTE: Okay, thank you. The second comment I had is it seems like the date of the license application hasn't changed independent of budget implications. Does that mean that you think you already have all the data that you need at this point for the license application?

ARTHUR: The answer is yes. I mean, most of the critical data we need right now is in analysis and other key areas. Other areas will be confirmation or other areas to support validation later.

DUQUETTE: Thank you.

ABKOWITZ: Abkowitz, Board.

John, I was interested if you could talk a little more about the expenditures for transportation during the current fiscal year. I gather it's probably one of the resource reduction areas. I've heard that the number is around $5 million, which I guess represents just a hair over 1 per cent of the DOE budget for the year for the Yucca Mountain project. Could you give us more details on that, please?

ARTHUR: Let me just give a snapshot, and I'm going to have Margaret, if you can, just add, why don't you give the specifics on transportation.

CHU: For '03, our original request for transportation was $25 million. We only got $5 million for this year.

ABKOWITZ: Abkowitz, Board.

I gather that that's an internal decision to allocate only 5 based on the reductions and the overall budget?

CHU: Exactly. It's an internal decision, yes.

ARTHUR: And if I can add onto that, I spoke in the replanning about essentially $295 million of a $460 million budget, $457 million. We are still looking at the remainder. Out of that, I have roughly about $75 to $80 million associated in various cooperative grants, funding to counties, other key areas. We have a good amount with the federal, our program direction for our feds for salaries, travel, other areas, but all the analysis isn't done yet. I mean, we still do have some option in the remaining money to make some other changes if required.

Again, what we're trying to do here as we move across from '03 into '04 is set a foundation, and if we get additional funds, to accelerate other key work and lay out a budget that shows how we can do that.

LATANISION: John, two maybe points of clarification.

One of your comments related to the I guess it was slow-down or not full activity in terms of the preclosure safety issue. And I may not be using the right language there, but I know you said it was not at the pace you had expected, and you also commented that that was related to design. And I'm curious if that means the project is re-thinking the closure design, or just what the implications of that comment were?

ARTHUR: What we're trying to do is stabilize some of the design areas. And in all fairness, Bechtel has been proceeding on this, but at the same time, with the funding, we had some work being deferred that otherwise we would have had done by now. So, one of the critical areas was award of a surface design subcontract to Bechtel that was issued last Friday to bring in some unique expertise to help with one critical aspect of how we handle the materials when they're in the waste handling building at the facility.

And when you really look at it, as I said earlier, it's a high majority of, or a majority of our preclosure safety analysis is associated with the surface design, so you need to have all that laid out before you can continue all the analysis.

So, I see, and Nancy can correct me if I'm wrong, I see that now at least we have things aligned with the design contractor that we can now complete that and keep things, get them back on track.

LATANISION: I see the point. And then, secondly, just one other comment. This refers back to the issue of the high temperature/low temperature operating mode issue. Your comment, as I understood, was that you expect it to operate at temperatures below the boiling point. And I just want to be clear on what that means. Are you talking about the boiling point--in the pillars?

MR. ARTHUR: And we'll be covering more on that I think in later presentations. Go ahead.

LATANISION: That's the clarification I was looking for. Okay, thank you.

NELSON: Nelson, Board.

Let me just follow up because I was going to ask the question about the sacrosanct 81 meter spacing, which was established some time ago, I believe on 2-D modeling, with very little knowledge of the lithophysal properties. And even now, I myself don't believe we have a very well defined knowledge of the bulk lithophysal thermal conductivity properties, and other characteristics, yet we continue to see the 81 meter spacing. So, this may be a bit closer to velocity over the next 18 months and beyond, but for those two time frames, what is the project's attitude towards the 81 meters? It could have cost impacts. It could have TSPA impacts. And maybe something to say about uncertainty, in addition to bottom line performance.

So, I'm wondering what is the project's attitude towards that 81 meters?

ARTHUR: I'm going to have to ask, if I can, Nancy to assist on that. In my mind, it's part of our baseline as we proceed, but let me have her give you some words.

NELSON: Does that mean not to be visited until LA is done, or what?

ARTHUR: Nancy?

WILLIAMS: Yes, Priscilla, it is part of the current baseline. But the baseline will continue to evolve. We are still doing thermal analysis and will continue to do thermal analysis. What happens in the first panel layout, for example, is not necessarily how it's going to end up in years out, in Panels 2, 3 and 4. So, to the extent that we have greater insights, we'll modify the design, optimize it, et cetera.

NELSON: Nelson, Board.

At what point, just philosophically, is there an iteration is the design change substantial enough that it's going to trigger some reconsiderations of license application of NRC consideration? I mean, you may not change the basic purstical, but you may change the uncertainty related to spatial issues. And it may be also on site investigation costs as well. I mean, there are many impacts. So, just is this--would you expect told hold the fort until the first panel is--performance confirmation is producing understanding, and then maybe make a change if it's warranted? What's the philosophy here?

WILLIAMS: I think you stated the philosophy well. I think that right now, we are trying to stabilize the first three drifts, and NRC does have a process in 6344 to review changes to that design.

CRAIG: Paul Craig.

Over the last couple of years, as you moved closer to licensing, it's become clear that you're increasingly taking the position that the science is under control, and that it's stabilizing, which I must say is not in accord with my own observation. But, that's not what you're talking about. What I'd like to do is ask for something very specific in terms of budgets.

As you're making these major changes in budgets, the way in which you make those changes tells us a lot about how you're thinking about the program. I wonder if you could give us some budget documents that would break the budget down into major categories so that we can see how these have shifted and will shift in the next couple of years? You've given us overall budgets, but I'm looking for the whole package so I can put into a context.

ARTHUR: Remember the point I mentioned earlier about that NRC had some questions the other week about what is the current planning versus what's the impacts of this replanning. So, that's why I offered to share the information that shows some of the dollars associated with the programs and what's currently being deferred, and that will include the various scientific or test programs also.

CRAIG: So, you'll provide that to us?

ARTHUR: Right. It will probably be about, I'm believing it's about two weeks until we owe it to NRC. So, I want to make sure, because the replanning documents, as we currently show it, doesn't reflect I believe what you're looking for, but we'll get that.

The other area I guess I'd add onto that, I hope by what I'm doing here, I'm not saying that test programs and science isn't important, what we're trying to do is get a balance. Everything couldn't be going on its current path. As I look at this, we had to make some pretty significant trade-offs, and you need to have staff to do the engineering and design, you need specific sub-contractors, you need to still get critical tests. So, what we're deferring is some things into the next several years that hopefully will pick up. But, also, as Margaret said, the work that Bob Budnitz will be presenting tomorrow, I mean, I hope that some day we get the right level of funds so we can accelerate some of these areas, or start them, I guess in some cases, and proceed. Because I have a vision of the future that through some of these programs, will have various outputs from the test programs and other scientific programs that will help us mature and learn things that right now, we can't even predict.

BULLEN: Bullen, Board.

Could we go to your first figure, which is the management assessment of progress towards license application? I just have a couple of quick questions about that one. Specifically, I'm interested in, you know, the most heavily weighted factors being total system performance assessment and design. What level of completion would you expect to have attained by license application for each of those, and why do you think that's sufficient?

ARTHUR: First of all, by the time the license goes across, we would expect 100 per cent in both of those areas. And when I say percent complete on design, that's the amount of design that we will require for the license application. That's one of the key areas that we have a technical exchange with NRC this summer. You know, clearly, we don't expect to do 100 per cent design of the repository. We're trying to develop the right amount that's required for the safety analysis and other supporting for construction authorization. But to have that figure complete, will show 100 per cent design, but it might be of 30 or 35 per cent of the design itself. Is that clear?

BULLEN: Yes. And, actually, Bullen, Board again. The followup on the last question, if you'd look at your last figure where you talk about essentially in June, you're going to freeze the design requirements and boundary conditions for engineering and design. Does that essentially freeze or lock in the operating mode that you're going to use, LTOM versus HTOM?

ARTHUR: Well, first of all, what we're trying to do is freeze the functional and operating requirements. I mean, design is still progressing up through, as I mentioned, May of '04, of next year. But, this will set some of the requirements. Some of the key decisions we will be making over the next six months will be decisions on things like omni directional transporter versus traditional rail. There's a number of areas that BSC/Bechtel put over to DOE either for decisions to be made or rendered over the next five to six months.

BULLEN: Bullen, Board.

Is there critical data that will be lacking when you make those decisions? I guess specifically, my concern is with respect to the cross-drift heater test, where you're not going to have the thermal conductivity of the lithophysal zones. And without that data, it's going to be real difficult to determine whether or not you've made the correct choice. And, so, how are you going to justify those kinds of decisions, I guess is the question?

ARTHUR: We'll have to defer more specifics to Nancy Williams.

WILLIAMS: Dan, we're really focusing on the surface facilities when we talk about that. Is it one weld cell, is it two, is it three? What do the modules look like? What's the construction schedule for them? What's the waste stream, and those sorts of inputs.

BULLEN: Bullen, Board.

Maybe to followup. Nancy, maybe I misunderstood. I was looking at the design freeze for boundary conditions, and so if I'm freezing boundary conditions, aren't I freezing the design for the subsurface facility?

WILLIAMS: Well, right now, we're still at the conditions that you saw in the last layout. They have not changed. It sounds like you're thinking about boundary conditions more in the sense of the natural system boundaries. But, really, what we're focusing on here is trying to stabilize the surface facility design, of which there are many options to be considered.

BULLEN: Thank you.

ABKOWITZ: Abkowitz, Board.

John, you and Margaret have made extensive reference to quality assurance and culture building and integration, and so forth, and I think I'm a strong believer in all of those concepts, but I'm also aware that these things take time to gestate, and they're often sequential rather than parallel activities. And, so, as I think through the size of the organization and the complexity of the problem and where you currently are, you've already kind of referred to this being pretty ambitious, I was wondering from your management experience, if it's unprecedented what you're dealing with now in terms of what you're trying to accomplish in that regard relative to the license application schedule? And if it's not unprecedented, could you cite another application where that type of success has been achieved?

ARTHUR: I think the five issues were laid out pretty well in the NRC meeting, the DOE/NRC management meeting the other week. And one issue that was referenced, first of all, was having 100 per cent quality built into the license application, good defensibility, models validated, all the right technical assumptions before the license going in in December of '04. And that's about what we've been talking about over the last half hour.

But outside of that, there were several other critical areas. First of all, safety conscious work environment. John Mitchell and myself issued, we started our first of a quarterly survey here recently from 25 per cent of our employees, asked about 10 questions about where things stand in their mind across the program. And it still shows that employees still have a real concern with the effectiveness of our corrective action program right now. It also shows a lot of employees, a majority have a problem raising a concern to their next level of management because of fear of, you know, schedule or quality or other key areas. So, that's the kind of changes we're trying to make, and that's a benchmark. That's the first quarterly survey, and we're going to continue to watch our trends, and hopefully things improve.

Another area that was brought up is it was stated that, you know, there's been expectations set by previous managers, and many good folks throughout the program, but a lot of times consequences aren't felt if people all adhere to that, such as procedural compliance. Those areas can definitely be rectified. But, as I said earlier, it takes time, and then the way that Margaret and John Mitchell and myself decide to proceed is about six weeks ago, I guess it was, we called a meeting of all of our first and second line managers, you know, the federal people, the Bechtel/SAIC, the laboratories, everybody involved in this program, there's about 175 people, and set expectations. And we said we realize it's a tough challenge, but it can be done, and it has to be done. We get one chance to do this, and we want to do it correctly.

They then went back and talked to everybody over a one month period. All of our over 2,000 employees on the program had met with their supervisors. And then the last meeting, which occurred last week, John Mitchell and myself received feedback on some of the barriers. There still seems to be this challenge about schedule and quality, and John did something very good this week. Out in Las Vegas, there's essentially the managers and others are seeing what their folks are talking about, you know, trade-offs, and how we build quality in the first time.

So, there's a lot of actions underway. There also have been some disciplinary actions. I always like to come out of an environment where I'm awarding the successes, but also when some just don't do it correctly, or there's violations, there has to be disciplinary actions. And that's starting to occur.

So, to go back to your point, I would say we're still upwards of a year or a year and a half to even show where things start visibly moving, and we're going to aggressively try to pursue that. That's what NRC said to us at the meeting the other week. They would like to not just see 100 per cent defensible license, but also the operating environment up to a higher level of quality by that time.

What I hope to show you, as well as NRC and others, in future meetings is where we stand on those metrics, some clear outcome based measures of what we're trying to do to monitor success. I have worked, I benchmark with others in industry. I have been involved in major changes like this. In some cases, it's taken upwards of three years. So, you know, we're trying to bring the best tools and the best people in to help us through it. But, it means all 2,000 people changing the way we operate.

CHU: I want to add a little bit about my personal benchmark. I came from the WIPP project, and I can only talk about the quality assurance program itself. It took WIPP about a year and a half to totally, you know, bring it up to where it's supposed to be. And then the starting point, my personal view for WIPP was probably lower than where we are now. So, that's the quality assurance, and this is my personal benchmark.

So, that's why I'm cautiously optimistic, and I think it's achievable when we all put our energy together, and make sure the troop understands its achievable.

Thanks.

ABKOWITZ: Abkowitz, Board.

As a followup question, could you describe the different challenges that you're facing with this program compared to the WIPP program, and how that may influence the comparison between the two?

CHU: One thing, I will say this program has orders of magnitude more external oversight. And sometimes it's good, sometimes it is distracting, to be real frank with you. So, with the WIPP program, I feel the challenges are very similar, the stuff that you're supposed to improve, you're supposed to improve, and then redo the procedures, or making sure people follow, those things themselves are quite similar. Actually, it's the same in QA-1, QA requirements, exactly the same requirements, the same set of procedures, and stuff. So, I think the work itself is very similar. But the external things are quite different.

ARTHUR: I might add a couple. Margaret, if I could add just a couple other points of comparing them? The level of complexity on this program, having worked both, is much greater, I feel. I mean, the interactions, as well as the design and other areas is much higher complexity that what we dealt with at WIPP.

The other area I wanted to leave you with, what I'm talking about today is not another plan. It's not another procedure. Granted, we have to change some procedures to make them a little bit more user friendly in a few areas. But everything is right here, and it's behavioral based. It's cultural change. That's what we're really trying to do, and we have some mighty fine people. I mean, I see them every day in my office. Everybody's heart is in the right place to make this program succeed. But, we're trying to just implant the fact that following those procedures to a T is very important. If you don't line that procedure, or have a problem, we have note, please call that person for either guidance or what's meant, but skipping it in this environment is unacceptable, because that just is like the weak link in a chain on a fence, and it leads then to a credibility issue. So, I know we can get over it. It's just a time challenge that's pretty big.

CORRADINI: Can I just echo what Mark said? You mentioned five things, I thought I caught three, you said in the discussions with the NRC. Can you run through all five so I'm clear? Because that was actually very interesting.

ARTHUR: First of all, see if my memory is there. If not, my people will help me. The first area was the license, 100 per cent quality into the license by the time it goes across to NRC. The next area, and these aren't in order of priority, I put this next one up to the highest priority, safety conscious work environment. Every employees feels they have an environment where they can raise issues and concerns to their supervisors, and they're dealt with in a timely manner.

A corrective action program was a key one to get one integrated plan program. Right now, we have several programs and we're trying to get those integrated. So, employees shouldn't have to worry when they have a concern about how the--does it go into what's called a SERS program, or is it a DR or corrective action. They should be able to get it out there, and then there should be another team that helps them broker it into the level of importance. So, it's a pretty complex program. The other part with corrective actions, again, is timely closure of those.

So, license, safety conscious work environment, corrective action program, accountability, managers are held accountable for their actions, you know, awarded for successes, but also disciplinary actions occur where required. And then the last one is procedural compliance. That's the five areas.

CORRADINI: Thank you. Other questions?

DIODATO: Diodato, Staff.

I wanted to just follow up on Dr. Bullen's line of questioning with regard to the items closing June 3rd, in a couple weeks, two, three weeks now, freezing the design requirements, boundary conditions. And from what I interpreted from Nancy Williams' response, that was really mostly related to surface facilities, and didn't necessarily talk so much about thermal loading in the repository itself. That was still a matter of some discussion. Is that correct?

ARTHUR: That is correct. I mean, we could show to the letter what's in roughly the number of boundary conditions we're in the process of approving. But, Nancy?

WILLIAMS: That's correct. There are limitations right now on the heat output from the waste packages at 11.8 kilowatts per package. We will be evaluating that going forward for operational flexibility purposes. The drift fall temperatures and the profiles that you're going to see when the panel talks this afternoon still take you to the higher temperature operating condition after closure for about 1100 years.

DIODATO: I see. So, the AMRs that are going to follow that are due August 3rd, I guess, the final, some of those have temperature dependencies; is that correct? The analysis and modeling reports.

WILLIAMS: It's still the same profile that you see up there; correct.

DIODATO: So, you don't need to worry about recalculating it if you change your thing, or--

WILLIAMS: Not at this point in time. But, if we so choose to do that downstream, we'll amend the license.

DIODATO: Okay. So, these are actually calculations, this is supplemental science and performance assessment temperatures here; is that correct?

WILLIAMS: Well, and followed up by the set of AMRs that are going through the system now to support the license.

DIODATO: But we shouldn't expect any change in these temperatures. These are the temperatures you're going to go to LA with?

WILLIAMS: That's correct.

ARTHUR: I might make a point, too, on this decision plan, on some of the dates. We are in the process of realigning that against this replanning. So, everything isn't, you know, what you'll see in the July time frame. It may change over what I've presented. This one here, not over on the boards there.

CORRADINI: Okay, any other questions? Staff questions? I have one last one that you said, and you kind of bounced between--it goes back to one that Mark had asked, and that was you mentioned that transportation wasn't on your diagram of the red, yellow, white, green, yet the surface facilities are. And, I guess now I'm going to take an operational mode, I'm not going to worry about things 1000 years from now, I'm going to worry about things in ten years from now. It seems to me they're integrally linked, the surface facilities and the transportation mode and how all these fit together. Is that the purview of the new Director relative to waste management system, or is that within the purview of the design of the surface facility? Because just my own personal concern is that you take from Point A, you bring it to Point B, you unload it, you do something with it. You have to store it, you have to decide what to do with it, and all the logistics associated with that are not obvious. So, in whose bailiwick is that under the current arrangement?

ARTHUR: First of all, federal-wise, what the repository, the operations, including transportation, you know, into the repository, is under my purview at Las Vegas.

But, the Deputy Director that Margaret is in the process of hiring in Washington, the transportation national program, as well as Nevada Transportation will be under that individual.

But the point I want to make is regardless of people's responsibilities, the interconnects are going to be on the schedule. We've had a lot of discussions recently about if you look at how long it takes to build, should rail be selected, rail lines? You could assume in some cases the first year to two years would be truck transport just by capabilities to, you know, develop the right rail system. So, we're doing a lot of internal discussions.

Now, that's some of the systems optimizations that Margaret mentioned a little bit earlier. There's a whole lot of evaluations underway right now. But, our planning at the repository is, be it by truck or train, to have the right flexible facility to receive and emplace the necessary waste for disposal.

CORRADINI: Okay. So, just one followup, and that is that the--Mark said it better than I will, so I'll do it imperfectly, the human factor of properly explaining and allowing people to respond and then re-explaining and allowing people to respond, that whole aspect is a dynamic that the other part of the engineering systems won't see. So, have you figured in the time that that's going to take? Because the surface facilities and the ability to get them from Point A to Point B at the surface facilities is very important. And actually the ability to do that and build confidence there by actually doing something rather than planning something or calculating something goes orders of magnitude.

ARTHUR: Right.

CORRADINI: So, is that being discussed?

ARTHUR: Right. And we'd be pleased to present that at a future meeting. I mean, there's a lot of discussions underway right now, as Nancy said, to get the surface design, you know, stabilized, get that completed and then the preclosure safety analysis. But with that, you know, things vary quite a bit, and that's a big complexity of WIPP. I mean, you looked there, we've have three true packs come in. They'd go in. We'd lift the lids, and out would come the containers.

Here, a lot of flexibility depends on is it Navy fuel, is it commercial fuel, and other areas about whether it goes into a staging area, whether it goes directly into the facility. So, we're doing a lot of optimizations right now to look, regardless of the transportation mode, that we have the flexibility in that first leg, and I say first leg of that repository surface design, that's the first one that we could, you know, design and fully construct to handle the material at the right ramp-up rates. And we'd be glad to share that with you in time, because we're still maturing. I mean, every day there's a lot of trade-offs going.

The good news about that is, you know, that's past the license. We are looking at some of what it takes to operate this facility.

REITER: Leon Reiter, Staff.

John, you mentioned before something about seismic, and I think Budnitz, Bob, was going to do some stuff and you'd be looking at this after LA. At a Panel meeting several months ago, at which DOE presented some of its work, many of the people, DOE, consultants and DOE people themselves, presented the fact that the ground motions being used for the postclosure were very high, to the point of being physically unrealistic. And the intention was--my question is are you going to proceed with those kinds of motions to the license application, or are you going to modify them?

ARTHUR: Let me ask Nancy. And I'm deferring to her because they have responsibility on the license application.

WILLIAMS: Right now, we're taking parallel tracks on that. We are doing the analysis for the truly unrealistic motions, and we're doing what we're calling the saturation study to look at how much energy really can move through the system, and that's going to continue into early '04.

REITER: The question is do you intend to modify the analysis to take those studies into account before LA or post-LA?

WILLIAMS: I would anticipate that that's information that will be available after the formal submittal. It will not be able to be factored into the AMRs. That's just due to logistics of the schedule and where we are in time right now, because that's an input. The information clearly will be available in time for discussions with the NRC.

CORRADINI: Other questions?

(No response.)

CORRADINI: I think we have a break. Thank you, John, very much.

We have a break for 15 minutes--20 minutes, and we're going to have the audio visual fixed.

(Whereupon, a brief recess was taken.)

DUQUETTE: Mike introduced me earlier. I'm Dave Duquette. I'm a member of the Board, and I'll be chair of this morning's technical session, in contradiction to the management session we had earlier.

As Mike said, and as indicated on the program, the Board has requested that the DOE describe the thermal aspects of the current repository design and operating mode, and how these aspects have been analyzed, and the results of those analyses.

To begin today's session, Bill Boyle from the DOE's

Office of License Application and Strategy will present the logic for evaluating engineered barrier systems. Subsequently, Bo Bodvarsson--I'm going to pronounce it, I hope that's not too bad--Director of the Earth Science Division at Lawrence Berkeley will describe the character of the unsaturated zone. There will be a brief, a very brief question and answer period after Bill Boyle's presentation, and a longer one after the second presentation. We'll break promptly at noon and reconvene at 1:30. There will be an extended session for questions later this afternoon.

Our first speaker is Dr. William Boyle. He's the Director of the Postclosure and License Acquisition Division in the Office of License Application and Strategy, the Office of Repository Development. The Division is responsible for the preparation of the license application, the development of the total system performance assessment, and development of the underlying technical bases. At one time, Bill sat on the other side of the table and was in the NRC. He holds degrees in geology and in civil engineering from the University of California at Berkeley. And, Bill, I'll turn the session over to you.

BOYLE: Thank you for that introduction and the opportunity to make the presentation this morning.

I'll introduce a series of talks, and in these talks, as a project, we hope to show our logic for understanding the engineered barrier system performance and why we are confident in our understanding of that performance.

Although the title focuses on the engineered barrier, you will also hear talks related to the natural system, because the natural system helps create an environment in which the engineered barriers must perform. And whenever I make a presentation like this, it's usually based upon the help from many others, and I'd like to particularly recognize Martha Pendleton for helping put not only this talk together, but the others, and also the people in BSC Graphics.

The objective of all these talks is through a series of integrated presentations. Dr. Chu this morning referred to it as an integrated story, to demonstrate our technical basis for the evolution of the in drift environment and the effects on metal degradation during the postclosure period.

Now, the talks today are going to focus in only on part of the problem, but it's most of the problem. We're not going to talk today about some of the events or processes that are certainly relevant, and we'll certainly have to talk about them in the license application. But some of the examples are listed in that sub-bullet. We're not going to talk today, for example, about the effects of seismic events on the natural system and how that in turn might affect the environment that the engineered barrier sees.

This has already partly come up this morning in earlier talks. The license application will be for a hotter postclosure condition. However, we do maintain the flexibility to achieve cooler conditions in the postclosure, and I think most people know this, but I'll repeat it. We can, with the current design for the subsurface make it as cold as anyone wishes simply by running the fans for as long as necessary to achieve that condition. That's the prime means of achieving the flexibility, but there are other ways as well, including staging of the waste at the surface before it goes underground. But, the fans alone can do it.

A little caveat. The date and conclusions presented today are preliminary. As always, the final real story will be in our licensing basis documents, the analysis and model reports, and license application itself.

This is a busy slide. You're welcome to examine it at your leisure. All I really want to get across with it is that heat affects the natural system, which in turn affects the environment of the engineered barrier, in this case, the waste package, and it will affect the corrosion thereof and the performance thereof.

Next slide. Can you go past this one? No, actually we changed it. I think they're out of order in the handouts. I want to talk about this slide first, and its cousins are shown throughout the room. There is one to my left, your right. There's another one over there. There's one right here to my right. And there is another one back there. At least, it's the center portion of these big charts that is reproduced here with minor variations. There was a little version control issue as we were rushing to put these together, but actually the differences might lead to very stimulating discussions later on.

The overarching purpose for these charts, if you will, and there's actually two different charts, was to use them as a means for communications, and also help our understanding and help others understand what it is that's happening in the drift as it evolves as a result of the heat affecting the natural system, which in turn affects the engineered barriers.

Now, I'll spend a little bit of time talking about this chart, because it is in everybody's handout. It's around the room, and the other speakers may refer back to it.

The Y axis is a linear axis of temperature. The X axis is a logarithmic axis of time. And the main thing that's being presented here, and as Dr. Diodato recognized, it is from the supplemental science and performance analyses, it's a plot of temperature versus time for two different waste packages, a hotter one, which is a hotter waste package, it's the upper curve, and the cooler waste package, cooler because it has different heat output, is shown right there.

Now, these calculations were done for a specific location. This one chart in and of itself doesn't contain all knowledge with respect to the temperature of waste packages versus time. For example, we in this one chart do not represent the differences in temperature due to differences in location within the repository, you know, on the edge versus the center, but for a means of the purposes today to get across our understanding, it will certainly work.

So, the region between the hotter waste package, the upper curve, and the lower curve, is in purple here. It's in dark gray on the black and white slides, if you will, and it defines a rhythm, if you will, that goes across the chart, a purple ribbon.

Now, also what's shown on this slide are three temperature regions, an orange region that's, and again in the black and whites, all the colors, the different colors for the temperature regions look the same, so it's not only orange, but it's at the upper part of the diagram, and it represents hotter conditions that are typically dryer, and also typically where we are focused more on the natural system performance.

The blue, or lower part of the diagram, is cooler temperature, and generally speaking, wetter temperatures. And intermediate, in between is a tan color where it's intermediate in temperature and also intermediate in terms of moisture conditions we might expect. In the blue region, we're focused most, or more, on engineered barrier performance, and in the tan region, it's a mixture, if you will. We need more precise knowledge of both the natural and engineered systems.

Now, I'll say a little bit about each region. As I think you can see right here, the basic concept is is up in this orange region, we have protection of the entire waste disposal system largely through dryout. The heat has driven the water away, and we also lost a lot of moisture simply due to the ventilation before closure.

The blue region is an area in which the water will come back. It's cooler. The relative humidity will be higher, and there's increased chances of seepage in the drifts, if you will. But, our protection is supplied largely here simply by the characteristics of the Alloy 22. It's in a region, it means cooler temperatures, that it's based upon our knowledge to date that the corrosion is rather insensitive to our expected water conditions for these temperatures.

It's the tan region where there is a possibility of water coming in, and the temperatures are such that we do need to understand the chemistry of the water in order to determine the corrosion behavior of the Alloy 22. That's the region of most interest.

You will notice that the color schemes here are gradational, that the orange tends to grade into the tan, and blue tends to grade into the tan, and you will also notice, and here's the difference between what's in your handouts and what's shown here, are these lines, these dash lines. They're shown as dash lines for a reason in that their absolute location is not known with certainty. And that's the purpose of the color gradation as well.

If we lived in a completely deterministic certain world, we would be able to draw some of these boundaries as straight lines at a known temperature. And one of the upper lines would represent a temperature such that any temperature above that we would be able to say there's no seepage. That's a line that would be located right about here. But one of these lower lines, and it's a question of, and this is the difference between this slide and the one in your handouts, is it's a temperature that below which we don't need to really be worried about the chemistry of a aqueous solutions sitting on the Alloy 22. It will largely be corrosion resistant. And the question is is does that line plot here, does it plot up here, does it plot at 100 or 10, just where does it plot? And it's actually covered--it's all wrapped up in these slides right here, and how one wishes to fit a line to those data points, or draw a boundary line to those data points.

Now, I discussed with respect to a deterministic world, and if it were deterministic, if we could show that the no corrosion line was at a higher temperature than the no seepage line, then everything would be fine forever, essentially, if you will, that either both mechanisms would be working all the time, or would be working together some of the time, but in all cases, one or the other would be working for all time. And that is either the heat would be keeping the water away, or even if it weren't, we wouldn't need to really understand the aqueous chemistry of the water because for the given temperatures at which the seepage could occur, the corrosion wouldn't be an issue.

Now, we don't live in a deterministic world, which is why we shaded the colors and drew the lines as dashes, but the principle is still the same. The higher we move the no corrosion line and the lower we can move the low seepage line, the extent of this region in which we need to know both the chemistry and the temperature becomes smaller and smaller, both in time and also with respect to concern. Now, the remaining talks will deal with these three regions.

All right. How many people have never heard of that word before, ternary diagrams? I'm here to explain what a ternary diagram is and how to interpret it and how to read it, because it will be used during the remainder of the talks. You can even see one on the chart right here. It's a graphical technique that geochemists use to help communicate their understanding of aqueous solutions, what's dissolved in them, and what might precipitate.

In this case, we're only showing three species, if you will, sulfate, bicarbonate, but in this case, it also represents all the carbonate present, you know, although the symbol is bicarbonate, we've also got actual carbonate accounted for there, and calcium.

Now, there's much more going on in the water at Yucca Mountain than just sulfate, bicarbonate, or carbonate, and calcium. But, through the use of this simple diagram, we can get most of the story, just using these three species. Dr. Bodvarsson will mention in a later slide how many we actually keep track of in our calculations.

For those that are more mathematically inclined, instead of geochemically inclined, you can think of this diagram as like the first ordered term of the Taylor Series Expansion. It's a lot of the answer, but it's not the complete answer.

So, what are we showing here? There are three vertices, sulfate vertices, bicarbonate/carbonate, calcium.

There is also opposite each of the vertices is a base, if you will. You're just going back to simple geometry. So, opposite the sulfate vertex is this base over here. And parallel to that base are a series of lines. At the vertex itself, it's 100 per cent sulfate in this case. At the base, it's zero per cent sulfate, and each one of these parallel lines represents an increase in 10 per cent of concentration of sulfate. And, similarly, there's 100 per cent calcium, 90 per cent, 80 per cent, 70 per cent, all the way down to zero per cent.

Also on this chart right here, equal parts calcium and carbonate, right there gives you calcite, and there's a blue line that goes over here to the sulfate vertex and what we need to know is is that in this triangular region down here, precipitation of calcite alone will remove all the calcium from the system.

If we are above the blue line but below the red line, this point here is gypsum, equal parts calcium and sulfate. This over here remember is calcite. Below the red line, we can remove all the calcium from the system simply by the precipitation of gypsum and calcite. Above the red line there is excess calcium, if you will. We cannot remove it simply by precipitation of calcite and gypsum.

Now, calcium itself is really of no concern for us. It's just a proxy, a stocking horse for the things that remain. If we have precipitated sulfate and carbonate, bicarbonate, the things that remain include chloride, fluorine, things that are of concern to us, but also all the other species that aren't represented on this chart, nitrate, phosphate and many others. But, as you proceed up here and you've lost the sulfate and bicarbonate, this region tends to be more corrosive.

Now, what else does the chart show us? What we have here is, for the most part, pore waters from two different broad geologic units out at Yucca Mountain. The green squares are pore waters, and their composition is from the Paintbrush tuft, non-welded unit above the repository horizon. The red triangles and blue circles represent pore waters from the Topopah Spring unit, the repository horizon itself.

What you'll see in the later talks is from these initial conditions out of the waters evolve through evaporation and dryout. An initial water will tend to migrate on this chart as various solids are precipitating.

Now, getting back to--this chart doesn't show everything--you'll see that some of these repository waters are plotting up in the seemingly more corrosive area, and in later talks, you'll hear why these really aren't corrosive for our conditions, and it's because of the other species that aren't shown on this chart, in particular nitrate, they help take care of how corrosive the water actually is.

Another important point to remember from this chart is the starting points of the non-welded unit pore water is different from the Topopah Springs pore water starting point. And it's important to remember you can't make a sow's ear out of a silk purse in this case. That is, where you end up on this chart is, in part, determined by where you start on the chart.

This is what you're going to hear for the rest of the day. You're going to hear a talk from Dr. Bo Bodvarsson from Lawrence Berkeley National Lab, and he'll talk more about the natural system, the character of the unsaturated zone, and how it's affected by the heat.

You'll hear a presentation by Dr. Mark Peters of Los Alamos National Laboratory. This is the marriage of the natural system and its effects on the engineered system. And, finally, you'll hear a talk from Dr. Joe Farmer of Lawrence Livermore National Lab on the materials' performance, that given the conditions that are created in the drift, what happens to the engineered barrier.

With that, that's my last slide.

DUQUETTE: Thank you. Because these are introductory comments and most of the other talks are going to be keyed to this, I'm going to ask the Board to only ask questions of clarification at this point, because there's an extensive discussion period after each of the other papers. And, so, are there any questions from the Board on clarification of what Bill has said?

CERLING: Cerling, Board.

On your ternary diagram, which was the last one?

BOYLE: Right. The triangle diagram?

CERLING: Yes, the triangle one. Presumably those are molar concentrations. And, so, what I'm wondering, because you take--I guess my question is are these molar concentrations or equivalent concentrations? Because it takes two moles of bicarbonate to get a calcium. And, so, I'm wondering if the blue line is really correctly plotted.

BOYLE: Right. Well, to tell you the truth, it used to be plotted somewhere else. And what Professor Cerling is getting at, and this is really off into the details that interest the geochemists, there's a difference in these charts if you keep track of moles. That will be a challenge for people to go back to their chemistry. Or you keep track of the species in equivalents, and they're not the same.

And the original charts, actually, as it was explained to me, were plotted in terms of equivalents, and the calcite point was up here two-thirds of the way along this base between bicarbonate and calcium. So, it does make a difference apparently if you plot these as moles or as equivalents, and the experts in this case, and I am not an expert in this diagram, have chosen to plot it here.

But, whether or not it's correctly plotted I will defer to the experts, but for some of the purposes of the conversation today, whether it's here or up here, then this blue line would shift, but what wouldn't change is if you are below the blue line, you could get rid of all the calcium, if you will, simply by precipitation of calcite.

Now, it's also my understanding that although these can be drawn differently, they actually would tell the same story in an understanding sense. One can chose moles or equivalents, apparently, and in this case, they have chosen this representation.

Does that help? I hope that helps for the audience, and I hope it helps for Professor Cerling. The experts who drew this aren't here, and I actually recreated this by hand myself, the earlier version when they were using equivalents when the calcite point was up here, and I was quite comfortable with it, but they decided to switch to this, and I have not bothered to recreate it by hand.

CORRADINI: Corradini.

I had a question about the other figure you had, the multi-colored, multi-faceted. That one. The purple band, you said it quickly and I didn't catch it. I was trying to write it down. You said the upper part of the purple band is one canister power, and the lower part of the purple band is another canister power. Did you tell us what those were? I forgot.

BOYLE: No, I didn't. I would have to ask Jim Blink.

CORRADINI: Is it a factor of two? Is it approximately--what are we talking about there?

BOYLE: You know, I don't know. Jim is in the audience. There he is. Jim Blink of Lawrence Livermore National Lab. These results are from the supplemental science and performance analyses published about two years ago.

BLINK: The top one is the design basis waste package in the SSPA, of the order of 12 kilowatts, as I remember, 12 or 14, something like that per waste package. And the bottom one is a high level waste package, which is a kilowatt or so. So, it's considerably different.

CORRADINI: So, this is an order of magnitude?

BLINK: But the waste packages share heat with each other. So, it's not fair to treat them as the whole repository being one or the other.

CORRADINI: Just so I'm clear, so the lower part of--so, two questions, or two clarifications. So, one, this is totally power uncertainty. This is not engineering uncertainties?

BLINK: That's correct.

CORRADINI: That's question one.

BOYLE: There's a whole lot of uncertainties not represented here.

CORRADINI: Okay. And then question two is the power uncertainty as described, this is the whole field of these things at one kilowatt versus 12 kilowatts, or an inter-mixture would create the variation?

BOYLE: My understanding is is that in the SSPA calculations, it was a line load of some given power density per meter, and that given power density per meter was generated by an inter-mixing of waste packages with different heat outputs.

CORRADINI: Okay. All right, thank you. And then second--or the third clarification is the peaking, the characteristic of the peaking is totally driven by the fact this is a closure at 50 years?

BOYLE: Yeah, whenever the--if it states that on there, then that's right.

CORRADINI: Well, I just assumed it since you started everything at 50 years.

BOYLE: Right. It's the higher thermal operating mode results.

CORRADINI: Okay, thank you.

BULLEN: Bullen, Board.

Since I'm limited to questions of clarification, I do have a question of clarification. If you look at the dotted lines that you called our attention to, and compare the projected version versus the hard copy here, you have a couple of boundaries, one with threshold, that one exactly where you set it for the project, and the project sets it at, what is that, about 135, 140?

BOYLE: I think it's 140.

BULLEN: 140. And yet over here, it's set at 110.

BOYLE: Right.

BULLEN: And, so, can you describe for me the evolution of the process of where you set that threshold?

BOYLE: Discussions, and I don't even know that it's finally set yet, and Joe Farmer will bring it up.

FARMER: Some of this has to do with materials, measurements that have been made. I think the older version that I think Bill showed you on the chart, shown on the viewgraph projector, or the lap top projector, those are older data and those correspond to measurements that we showed you back around the first of the year. And, frankly, the charts shown on cardboard are the more recent data.

One of the criticisms that we received in January was that we needed to fill in the data. So, during break, or if you want to come up, we can show you we collected substantially more data since the January presentation, and we now are able to identify things such as the threshold temperature for crevice attack of the Alloy 22 with greater certainty that we could in January. So, the chart that Bill has on the overhead projector is a much older slide with older data. The ones on the posters are the more recent and probably the ones that you should really pay attention to.

CORRADINI: Are there other Board questions? Paul?

CRAIG: This is Paul Craig. This is more in the nature of an observation.

This morning, we heard about how programs are being shut down and the science programs are being shut down in particular, and now we're seeing data which appears to me to be critical data, which is very recent, within the last six months or so, which suggests to me that maybe the science remains unstable even at this time, and there's a real risk of not collecting new science because there may be a lot more to be learned. This is not a new message. This is a message with the program for many years, and I don't know whether you have a response to it. Because if you don't collect knowledge, then you don't learn about problems. That's certainly a way to be safe.

BOYLE: And I'd just like to point out that although we did suffer a large cut in funding, $134 million, that still left $400 plus million to look into science, engineering, and everything else, pay federal salaries, travel, everything else that's done, but there still is a lot of money left for science, not only this fiscal year, but in all the intervening, you know, the succeeding years, there is a commitment. You've heard from Dr. Chu I think first a year ago this month at this meeting, her commitment to science, but also in the regulations, there's a commitment to science and further understanding.

CORRADINI: Staff questions? Carl?

DI BELLA: Yes, I think you said that this is not the latest chart. The latest chart is the ones on the easels, and that we are going to be referring to those later on in the presentations. But, still I have a question about this particular chart. There's a note there sort of in the center of the chart way over to the right saying threshold temperature of the localized corrosion in calcium chloride brine, conservatively recommended by others. Who are these others that are doing testing in calcium chloride brines? I don't want to know what their temperatures are, but who's doing this testing?

FARMER: Well, I will try to answer that question. First of all, again reinforcing that this is an older chart, and actually this is the original graphics draft that we put together, and when we said recommended by others, frankly, we put in numbers that were recommended to us by this Board in January.

So, for example, we came to you and said, well--

DI BELLA: Based upon testing?

FARMER: Exactly. If we plot these data points up in a straight line, you know, they intersect at 140 degrees centigrade, and you very correctly pointed out that there were relatively few data points, and that a slight shift in the slope could have a dramatic impact on one's conclusions.

So, in trying to put together our draft chart, which Bill is showing you, we wanted to reflect the fact that our simple minded linear plot was not the grand sum total of all human knowledge having to do with the subject. So, we put that on the chart, and what you'll see in the later versions, and we actually have handouts that we'll give you that each Board member will get a commemorative copy of these posters, but basically what we show you in these later versions of the chart is that we have in fact gone and collected more data and we now have actually identified these temperatures with a much greater degree of confidence than we could in January.

So, a lot of hard work and a lot of effort has gone in between January and this point to try to better define these thresholds. And as a result of that, you see that those dash lines that are in the original draft that Bill was showing you on the screen have moved, and they're now located, again to the best of our knowledge today, as they're shown on the easels.

LATANISION: Just a followup. Latanision, Board.

I think in terms of the timing, after the January meeting, the Board did visit the folks in San Antonio at CNWRA, and my recollection is that they had shown us some data that did indicate a much lower threshold. Now, I suspect we'll hear some of that tomorrow from Gustavo Cragnolino.

FARMER: That's correct. And what we will show you today is we've collected similar data. We'll also show you today some long-term corrosion potential data that extends out over one and a half years, and I think as we converge on what we believe to be the correct answer, we believe you can operate certainly the base metal, the unwelded base metal, up to around 100 degrees centigrade, which coincides with the boiling point. And Bo will show you that above the boiling point, we expect relatively little seepage into the drifts.

So, we would like to lay out for you today this whole story and tie the environment to the waste package.

LATANISION: Latanision, Board.

I'm sure we'll come back to this, Joe, but you said something very important, and that was unwelded.

FARMER: That's correct. We'll show you data today in the fourth presentation in the series of four that will illustrate for you the change in corrosion potential with time for both base and weld metal.

LATANISION: Good. Thank you.

DUQUETTE: I think we're violating my game rules. We're getting beyond questions of clarification, and, so, I'd like to keep us on schedule and go to the next speaker, please. And that's Dr. Gudmundur Bodvarsson, known as Bo. He is the, at Lawrence Berkeley Laboratory, he is the lead for the Yucca Mountain project, and Director for the Earth Sciences Division at Lawrence Berkeley. His research specialties are geothermal reservoir engineering and nuclear waste disposal. He holds degrees in mathematics, physics, civil engineering and geological engineering.

And, with that, Bo, I'll turn the floor over to you.

BODVARSSON: Thanks a lot, and good morning, everyone.

Like the Chairman said, my name is Bo Bodvarsson, forget the long name here, it's just Bo, very simple, and I'm from Lawrence Berkeley Lab, and I'm going to give the first of the three technical talks. I'm very pleased that Bill, Dr. Boyle, I guess introduced us very properly.

My job is to take a look at the rock, and then Mark Peters is going to look inside the drift, and then Joe Farmer is going to look at the waste package.

Why is all of this important? Why do we need to look at the rock or waste package corrosion rates? And the simple answer to that obviously is that the pore water that may go into the drift contains chemical components that are found in the pore water in the rocks. So, we have to start by looking in the rock, then look at the water as it goes into the drift and vaporizes, and then look at the conditions around the waste package. So, that's my job.

I am going to talk about the unsaturated zone. I'm going to give you a little background about the unsaturated zone, and then go more into the specific issues, which is basically how much water is going to seep into the drifts and when. And what is going to be the chemistry of this water that's going to affect the environment around the waste package?

This work represents a lot of people that work on the unsaturated zone, both at all the labs, and the U.S. Geological Survey.

My outline is very simple. I'm going to tell you a little bit about the unsaturated zone, the geology and what data we have collected. I'm going to tell you about the conceptual model understanding of seepage and coupled processes, because this affects water going into the drifts and the chemistry of those waters. I'm going to tell you about thermal hydrological processes and seepage during the thermal period. Then I'm going to talk to you about the chemistry, starting with the chemistry of the pore waters, how they evolve during boiling and condensation, and what can possibly go into the drifts. And then I'm going to summarize and conclude.

So, first before we look at coupled processes, we must understand how much seeps during the ambient non-thermal period, and what is the chemistry of those waters, and I'm going to start by telling you a little bit about that. Then later on, I'll tell you the chemistry of those waters, because the chemistry of those waters may be different from the in situ pore water chemistry. Then I'm going to tell you about the thermal period and the boiling and condensation effects, and I'm going to argue and I'm going to tell you that very little water will actually seep during the thermal period. And I'm going to show you evidence why we think that's correct.

Then I'm going to tell you a little bit about if something would seep into the drift, what would be the chemistry of that water during the thermal period. So, those are the things I'm going to tell you about. And then, of course, Mark will take this chemistry and this water inside the drift and look at the thermodynamic changes, and then an overlook of actually the corrosion rates next to the waste package, at the waste package.

Now I'm going to tell you a little bit about the unsaturated zone. And here, you have our site scale model, which is a three dimensional numerical model that calculates flow and transport and thermal and chemistry in the unsaturated zone. You see it's very fine-gridded, in the repository area, which is located here. You see also the exploratory studies facilities, the tunnels and where we get all of the data from. Then you take a cross-section, north-south cross-section, and you get something like that where the model actually reflects, of course, the topography, the layering, faulting, and all of the details of the unsaturated zone.

You see in this north/south cross-section the proposed repository is only in the lower lithophysal rock. If you could take a cross-section east/west, you will see part of it in the middle non-lithophysal and other rock units. But this is by far the most predominant, about 80 per cent, of the repository is going to be in the lower lithophysal.

This is just to give you a little view about how small volume of this rock mass the drifts actually occupy. The drifts are about 5 1/2 meters in diameter. It's about 81 meters between drifts. So, in this very small unit here, you see that the drifts occupy a very, very small part of the rocks after you have drilled the emplacement drifts.

This just shows you the tunnels where we do a lot of testing, and just to point out the heater test, which is really the most important test for the coupled processes, is located in Alcove 5 around here. But we have done seepage tests that I'm also going to talk about in various niches located in different repository units.

Now, I'm going to start with seepage and coupled processes, and our conceptual understanding with reference to that. And if anytime you have questions, I don't mind, if the Chairman doesn't mind, you can ask. Feel free to ask.

The seepage into the drift has been studied for about five to six years now where we do very simple but very effective tests. Here, you have a niche with a drift. You drill bore holes above the niche. You put water into the bore holes, and then we measure how much of this water we put into these bore holes actually seeps into the drifts, and what fraction then goes around the drift.

Why is this concept important? It is important because the drift is a capillary barrier. Water does not want to go into big openings. Water wants to stay in the fine grain material. That's what surface tension is all about. That's what capillary pressure is all about.

Why is that important? It's important, if no water enters the drift, then we have much less problem with corrosion rates. We have great difficulty mobilizing the waste when the waste packages fail, and we have great difficulty actually transporting the waste to the environment. So, water going into the drift is a key.

Water, we find out, you can look at this graph here, first of all, our average water flow through the mountain is about 5 to 10 millimeters per year, which is a tiny amount, just like that per year, a very small amount. We cannot test seepage under those conditions because (a) nothing is going to seep, and (b) at these low rates, the tests just take way too long. We cannot wait thousands of years to see if something seeps, which it won't.

So, what we do, as you see here, we carry out this test at much higher rates, and you see here 1.2 million millimeters per year, going to 300,000, going down and down and down. What this demonstrates clearly is that you have a capillary barrier effect, and a capillary threshold below which if you have a percolation flux much below 8,000 millimeters per year, you will get no water into the drifts, no seepage of any kind.

We find that for most of these units, the lower lithophysal and the middle non-lithophysal, the seepage threshold is on the order of a thousand millimeters per year, which is 200 times, roughly, the current percolation flux. So, that says that under current conditions, we will have no seepage into any of the drifts unless you have a huge fault where a lot more water is going through them. So, this is a very important concept.

BULLEN: Bullen, Board. Just a quick question to follow up on that.

What kind of heterogeneity did you have in the rock structure when you did these kinds of experiments? Did you have an active fracture that was flowing, and the matrix?

BODVARSSON: Yes.

BULLEN: Basically, so you had both matrix and fracture flow that was occurring in these tests, and so that's where you came up with the numbers?

BODVARSSON: The degree of heterogeneity at Yucca Mountain covers four orders of magnitude in the fracture system. Basically, whenever you take permeability measurements, they range from an upper limit of from 100 darcies, and a lower limit of a millidarcy. Four orders of magnitude. The same variability we see in our seepage tests. We do air-K tests at different intervals so they reflect the degree of heterogeneity that the whole mountain reflects, which is four orders of magnitude.

We do seepage tests at intervals, some of it is very low permeability, other ones are very high permeability. We have seen, as you suggested, a definite indication of where we can actually map a fracture directly from one of these intervals straight to the ceiling of the niche, and see actually seepage through that feature. So, I think most or all degrees of heterogeneity that we see in the mountain are reflected in our seepage dataset. So, this is a very important concept, a thousand millimeters per year, 200 times more than the current one.

So, one can argue that perhaps no water can ever seep into the drifts. It's very important. Here's some of the instrumentation that we actually used to test seepage.

Prior to, during and after each test, we do predictions, we do calibrations, and we do validations of the models. Prior to the test, we predict where we expect seepage to occur. This is the model run that shows a heterogeneous permeability field. Those are the different colors reflecting different permeabilities. The four orders of magnitude we measure are reflected in our models. You see here progressing timewise, a test where we inject in the beginning, and you see it starts to spread, and then it starts to go around the drift because of the capillary barrier effects.

CORRADINI: Corradini, Board.

So, let me ask, so how did you get the orange and the light orange? Did you just, what shall I say, randomly select it? You didn't measure this?

BODVARSSON: This follows up a question by Dr. Bullen, and what we do is we measure systematically in all of the boreholes with packer tests air permeabilities. We get from that a frequency versus permeability diagram that says this is what this is, the percentage of the rock that has this permeability, this is the percentage of the rock that has this permeability. You get the correlation length.

Correlation length says that the distance between an area of low permeability to an area of high permeability is this many meters, or this many feet. Using all the statistics, we randomly occupy this volume of rock with those statistics that realistically then reflect the heterogeneity of the medium.

CORRADINI: So, to say it back to you so I've got it right, so you create a unit cell, which you think you've characterized, and then you replicate the unit cell randomly?

BODVARSSON: Yes, except a unit cell implies one permeability.

CORRADINI: No, no, I meant the unit cell may have heterogeneity in it, but you try to characterize the unit cell, and then you replicate that unit cell throughout the rock?

BODVARSSON: Yes.

CORRADINI: Okay. Second question then is what do you do about the surface of the tunnel?

BODVARSSON: The surface of the tunnel here?

CORRADINI: Yes.

BODVARSSON: What do you mean what do you do about the surface?

CORRADINI: I mean, is it perfectly smooth? Does it have roughness? What's the length scale of roughness?

BODVARSSON: The surface of the tunnel, because this is drilled out, it has some roughness to it. This roughness of the tunnel is reflected on our test results that we showed on the last slide. So, all of the data reflect the roughness of the surface of the tunnel. We have not found that this roughness of the surface matters a heck of a lot in the seepage calculations.

CORRADINI: Oh, you haven't?

BODVARSSON: No.

CORRADINI: So, let me turn the question around and then I'll stop. So, if I take a ping-pong ball size length scale, tennis ball size length scale, or basketball size length scale, that doesn't affect the seepage on the surface?

BODVARSSON: No. You are very correct about that. What I was saying is that if you had a tennis ball, and if you have a very fine smooth surface versus a little coarser, the seepage is not significantly altered. When you go from a golf ball to a tennis ball to a basketball, what's bigger than a basketball? Beach ball. The seepage characteristics are different, because the scale is 1 over R, where R is the radius of the ball, so the bigger the opening, the more percentage seepage you will get. But that's all taken care of in both our analytical and numerical work.

Did I answer your question okay?

CORRADINI: Yes.

BODVARSSON: Yes, sir?

PARIZEK: Parizek, Board.

While we're on that diagram, the solid orange on the bottom, these explanations, because you have a scatter of points on the right and above, but the solid seems like it's all drying out, is that based on measurements?

BODVARSSON: Which one are you talking about now?

PARIZEK: Underneath the--

BODVARSSON: This thing here?

PARIZEK: Yes. Right. Is that a drift shadow, in other words?

BODVARSSON: This is just a little--I think it's just the color scheme. This has the same heterogeneity, and I apologize. There should be nothing different there from the rest of the rock. So, I apologize for that figure. It's a good question.

CERLING: Cerling, Board.

Does this mean that in the initial conditions, that the saturation of the pores is near zero per cent, that is, you're starting out with a perfectly dry rock?

BODVARSSON: I apologize for that again. This is actually the fracture system. The fractures themselves are very dry. We estimate that about 5 per cent saturation in the fracture system. This is the heterogeneity of the fracture system. But, actually, the rock mass itself has 80 to 90 per cent saturation. So, it's almost fully saturated with water. These calculations, since our testing is so close to the drift, are dominated by the fracture continuum, and the matrix continuum has very little effect on these measurements, because the matrix permeability is orders and orders of magnitude lower than that of the fractures.

Does that answer your question?

CERLING: Yes.

NELSON: Just to close the circle there. Nelson, Board.

The application of the capillary barrier concept is classically based on certain assumptions that have to do with homogeneity and matrix porosity. And, so, you're running this test and interpreting it where the saturations we're looking at, this is all fracture porosity, and saying that the seepage that you see actually is related to capillary barrier effects. So, you're assuming here that the capillary barrier effects, the concept is valid for fractures as well as for matrix? Yes?

BODVARSSON: Yes.

NELSON: Is there any other work in the literature that has dealt with this idea of developing capillary barriers in fractured media as opposed to matrix porosity, homogeneous continuum materials with smooth surfaces?

BODVARSSON: That's a good question. Let me answer it this way. The capillary barrier phenomenon was developed by Phillips in the Sixties. For a homogeneous medium, you're absolutely correct, for a homogeneous medium. Since then, it has been applied to all types of mediums, including coarse sands, to very fine grain material with different capillary characteristics, ranging the whole spectrum, if you will. It is, without a doubt, proven I think that the capillary phenomena works regardless of what the medium is, but of course the more and stronger capillary forces prevail in the rock mass the stronger capillary barriers you will have.

You can look at the fracture system as a coarse grain sand or gravel that has capillary suction much, much less than that of the matrix, which has tiny, tiny pores and capillary suction potentials of tens to hundreds of bars. The fracture medium has very little capillary suction potential, but it still is a very effective capillary barrier in spite of that.

Going to the last slide, just one more sentence, this thing here clearly verifies that the capillary barrier is in effect.

NELSON: I think it clearly indicates that something is going on. But the conclusion that this is a capillary process for fractured material with very uneven surface and open fractures, to me, I still resist it. I resist that assertion.

BODVARSSON: Okay.

NELSON: I mean, I can see your data, but I'm thinking about all the other ways water can find to move through a system like this. It's a very hard experiment to do. It's not easy.

BODVARSSON: The other thing that might help you also, Priscilla, I don't know, is the following. We have drilled kilometers and miles and miles of tunnels at Yucca Mountain, as you know. We have never seen anything seep into any of the tunnels. And the only forces that I can think of that prevent you from getting water into the tunnels if low capillary forces are present, because gravity certainly is present, is the capillary barrier concept.

NELSON: Well, have you seen any evidence that the mountain in situ right now has water in fractures?

BODVARSSON: Yes.

NELSON: Or is the water in matrix?

BODVARSSON: We have evidence that there are waters in fractures from Niche Number 1. This is the only one where we actually dug the niche up without using water. When we looked at the end after digging up the niche, we saw a flowing fracture, which is the only fracture we have seen flowing. But it was a clear fracture flowing.

NELSON: Flowing, or was there a dark zone?

BODVARSSON: There was a dark zone around it, which is--

NELSON: This was a fractured zone; right? This is not a discrete--this was, as I recall, a fractured zone?

BODVARSSON: It was a single fracture. It was different from a zone. Actually, the fracture had different shapes and sizes, but there was more a single feature where the water actually was flowing down.

NELSON: And, so, the dark zone, was there water moving from the fracture into the matrix? Is that where you got a broadened--

BODVARSSON: Some of it had actually it looked like the matrix were more permeable because some of the colors you saw lighter. That included some of the matrix also, yes.

NELSON: I can see a whole lot of things happening there.

PYE: Pye, Staff.

Bo, you've indicated a roughness doesn't appear to have any effect on capillary barrier?

BODVARSSON: I said it doesn't seem to have a big effect on the capillary barrier.

PYE: Okay. My understanding of the capillary barrier is you get a layer probably several centimeters thick, and if roughness factors essentially breach that capillary layer, then you have a diminished capillary effect; is that correct?

BODVARSSON: In the tests we have seen so far, the roughness of the niches is not uniform. So, the first order effect for seepage is the radius, because of 1/R dependence. And it seems like actually the roughness is not the real critical issue in our measurements, or in our modeling right now.

PYE: Okay. Let's take roughness a little further forward. If you start to have degradation, is there some point where degradation, i.e. increased roughness, breaks down the capillary barrier?

BODVARSSON: The capillary barrier will not break down, but it will be different, depending on the size and shape of the opening. For example, if you take the extreme of a square opening, and one would think a square opening would be very, very conservative, a square opening will also have a capillary threshold to it.

PYE: Okay. You indicated, you know, the project has driven several miles of tunnel, and observed no seepage. How much is that due to ventilation. And, typically, in experiments, what is your mass balance?

BODVARSSON: The answer to the first question is that there is ventilation in a lot of the tunnels. In one part of the tunnel, we have kilometers we have closed for years actually, and we have not seen seepage. We have seen some condensed water that the measurements and the chemistry so far indicate that that water is void of silica. Pore waters are never void of silica. Silica is generally equilibrium with those pore waters, and that strongly suggests that that's condensate. So, in the areas where we totally shut off ventilation, I don't think there is any evidence for any seepage into the drift in those areas.

PYE: Again, a hypothetical question. If you put a temporary bulkhead at the north portal and one at the south, and you turned off ventilation, what would you expect to see?

BODVARSSON: This is getting into, let's see, what do I expect to see? If you close up the south and the north portals, I would not expect to see any seepage into any of the drifts.

PYE: Okay, thank you.

DUQUETTE: Please continue.

BODVARSSON: So, that's my opinion.

This one shows actually after we do all these calibrations of the models with all of the data we collected, we then go into total system performance space, and we actually calculate all kinds of different curves that they can sample and use in the total system performance assessment. And that's what this shows. The red one at the bottom here is the seepage threshold. This controls the seepage threshold based on the permeability. And interestingly speaking, the higher the permeability, the less seepage, because it's easier to go around the drift the higher the permeability.

Also, the capillary suction factor, one over alpha, the lower the value of this factor, the more seepage again.

And then, of course, percolation flux. The more water that goes down through the mountain, the higher seepage fraction.

Now, having demonstrated that the seepage works under ambient conditions, now we are interested in finding out what happens during the thermal conditions. And the Yucca Mountain Thermal Test Program is aimed to address the thermally driven coupled processes, and I'm going to tell you a little bit about that.

The project has gone through a series of heater tests, all aimed towards understanding the effects of thermally driven coupled processes on the near field seepage water chemistry of the seepage water.

The three major tests, that's the single heater test, the large block test, and then the drift scale test that has been going on for four years heating and is now in the second year of cooling down. And this is a collaborative effort of many of the research institutions involved in the project.

I'm just going to go rather quickly through this. If you have questions, please ask them. As you know, during the drift scale tests, and I'm going to concentrate on the drift scale tests because that's the largest and most comprehensive test, we measure all kinds of things. In the beginning, we characterize the heater test area with pre-test characterization, where we measure the thermal, hydrological and mechanical properties of the rock mass, minerals and petrology, pore waters, and everything to do with what is in the area, air permeability.

During the heater and the cooling test, we have continuous measurements of a lot of parameters, temperature, displacement, strain, humidity, acoustic emissions, and others. And then we have periodic measurements during heating and cooling, geophysical measurements, air permeability, gas and water sampling. And I'm going to show you some of these results a little later on.

There are lots of bore holes around this heater test. This is the heater test area. You have heaters here, if you haven't seen this test, and then you have bore holes, measurements above, below and beside the test.

We do model prediction prior to the test, and these are blind predictions. All of what I'm going to show you now from the heater test, there are no calibrations involved. These were blind predictions. I'm going to show you how they agree to actual measurements, no calibrations. So, you can look at these as validations.

The main physical processes that go on when you heat a rock mass past boiling are as follows. You have extensive boiling of the mass, rock mass next to the drift, with steam going from the matrix into the fractures, and then flowing out to cooler regions, where actually the steam condenses. And you have higher liquid saturations in the fractures. You develop an area where you have total dryness, both in the fractures and in the matrix, that is, the liquid saturation is zero. The gas saturation consisting of non-condensible gases and steam is 100 per cent.

Then the time, of course, as the heat output goes down of the waste packages, this thermal area will shrink and eventually disappear, and you get back to the ambient situation. A lot of moisture distribution processes in place with boiling, condensation, imbibition into the matrix, gravity drainage in fractures, drying front, and condensation fronts.

BULLEN: Bullen, Board.

Before you leave this, this is a great example of the types of predictions and calculations that you do, and a lot of your calculations are done to map this. Are they done in 2-D or 3-D? And how would you expect this type of figure to vary first along the line of a drift where we've got waste package variations, and then from the center of the repository to the edge of the repository?

BODVARSSON: We do calculations in 2 and 3-D. Most of them are done in 2-D to get the main physical processes, and then we do 3-D calculations. The dominant heat transfer mechanism in this test is thermal conduction, as Priscilla mentioned, for example, the importance of knowing the thermal conductivity of the lithophysal rocks, which is correct. So, that's the predominant factor. When you have waste packages that have different thermal outputs, three dimensional representation is absolutely necessary because you get different temperatures in the waste package that then has different temperatures in the rock walls, and then going into the rock.

If you are only interested then in how the boiling front moves with time, you don't need 3-D, because it all smears out inside the rock pretty much. But if you are purely interested in the crown temperatures, you must include three dimensional.

Did I answer all of your questions?

BULLEN: The edge of the repository versus center, though, the same?

BODVARSSON: The edge of the repository is going to be cooler because of your lateral cooling outside. The basic physical processes are all the same.

Now, again, let me emphasize these are blind predictions. This is the heating during the 48 months of the drift scale tests at different sensor locations. This is probably the best statistics that indicate the agreement. The mean error between model predictions and measurements for 1700 sensors is less than 5 degrees throughout the heating phase. So, they match very well going from 20 degrees up to

240 degrees.

The cooling similarly is right on track with respect to our predictions versus actual measurements. So, we have done I think very well in making blind predictions. There are no calibrations involved here.

We also had quite a lot of success in electric resistance tomography done by Livermore, where we actually have the dryout zone. This is shown here in a model calculation, and this is verified by the ERT data that also indicate the size of the dryout zone.

PYE: Question. You talk about the redistribution of moisture.

BODVARSSON: Yes.

PYE: What can you tell us about the relative humidity inside of the drift?

BODVARSSON: During the heating phase?

PYE: Yes.

BODVARSSON: Well, it's way down, of course, because you are drying everything out.

PYE: Okay. What is the source of that water? I mean, you're saying the vapor moves out. So, some portion of that vapor stays in the drift, or moves to the drift?

BODVARSSON: No, no, no. I'm talking about, when I talk about the vapor moving out, it moves out in the rock. It doesn't stay in the drift. The drift becomes filled with steam pretty much. The origin of the water is pore water in the matrix that boils. So, you can imagine a single block of rock, say one meter away from the drift wall, and if you look into that block, in the beginning, we'll just see an increase in temperature. So, the temperature increases just because heat is emitted from the drift wall. Then, when it gets to 96 degrees to 100 degrees Centigrade, it boils because the pressure is one atmosphere in the gas, in the air, and then it starts to boil. When you boil, pv is equal to nrt, you increase pressures because you're increasing the volume, and basically what happens is that steam goes into the fractures and flows away from the drift and condenses away.

PYE: Okay. I think you indicated it was steam in the drift. Did I mishear you?

BODVARSSON: Yeah, there will be steam in the drift mostly because of the boiling process. But some CO2 and other gases.

PYE: So, you're saying you--okay, we have a constant pressure system, we have an open system?

BODVARSSON: Open.

PYE: So, what's the total pressure inside the drift, plus water vapor?

BODVARSSON: Total pressure inside the drift is going to vary. You're always going to be very close to one bar. Maximum gets to one and a half to two bars of pressure, and that's because the permeability of the fractures is so high you can't build the pressure.

PYE: Okay. So, steam displaces air. Where does the air go?

BODVARSSON: Air goes away with the steam in the gas phase, and gets condensed far away. I'll show you a plot of the CO2 in a moment.

PYE: Okay, thank you.

CORRADINI: Can I ask a question?

BODVARSSON: Yes.

CORRADINI: The way you explained it was different than I thought you were going to explain it. So, can I say back what I thought I heard you say? Do you mind?

BODVARSSON: Yes.

CORRADINI: Can you go back to the previous slide? So, there's two things happening simultaneously, and the pressure is going to send the vapor in both directions. So, if you're heating up into an open hole, you actually have mass transfer into the hole and into the far field; is that correct?

BODVARSSON: Well, you have most of it is going to be pressure, highest pressure in the drift, because that's where the heat source is. Again, pv equals nrt. But then you have diffusive components also working, where you have diffusion in the gas phase, allowing steam to get into the drift, and air to go out of the drift, because of the diffusion in the gas phase.

CORRADINI: Okay. So, just bear with me for a moment. I'm going to give you a very simple test, since I've seen these tests run for a totally different application.

BODVARSSON: Okay.

CORRADINI: If I put a heat flux on a rock or a ceramic body that's essentially saturated or partly saturated with water, the water will essentially evaporate, not boil, but evaporate and will come towards the heat source, and go away from the heat source because of a pressure distribution. Is the pressure distribution calculated in these simulations?

BODVARSSON: Yes.

CORRADINI: Okay. So, then the physics of it is is you're going to have vapor coming into the drift, replacing air, or mixing with air, and you're going to have vapor going out because of the pressure distribution?

BODVARSSON: That's right.

CORRADINI: Okay. So, that's the reason, when you said steam, and he asked you back steam, I thought you were going to tell me I'm driving stuff in because of evaporation, and I'm driving stuff out.

BODVARSSON: Right.

CORRADINI: Is that correct?

BODVARSSON: That's correct.

CORRADINI: Okay. All right. Then the second part of the question is in the simulation for this test, what is the uncertainty in the ERT measurement? I'm astonished by the good agreement. Sorry for using that word. So, is there--I don't understand, I'm sorry I don't understand ERT, so tell me what's the inherent uncertainty in the resolution of that temperature or scale.

BODVARSSON: There is significant uncertainty in all geophysical methods trying to look at dryouts. So, I would say the uncertainty in this, without quantifying it, is significant. It's on the order of meters. But what we are trying to do here is to establish that actually a dryout zone exists. The size of it is not as important as it exists, because these are the only methods that can tell us it really happens, just like models predict.

CORRADINI: One last question and I'll stop. So, now I've created this thermal environment. I'm driving water in a vapor form towards the drift up to a point when I equalize pressure and concentration. I drive it away, since there's no equilibrium in concentration. Can I have seepage up this, up the temperature gradient?

BODVARSSON: No. And I'll show you that in a minute.

CORRADINI: Okay.

NELSON: Nelson, Board.

We've seen these plots before, and a couple of questions still arise, as I recall. Simulation and saturation where you end up with that high saturation zone underneath?

BODVARSSON: Yes.

NELSON: All right. The blue zone. As I recall, there were some measurements that indicated that there was ponding of moisture underneath the opening. That was not well explained at the time that we saw that information. Can you tell me why you're getting that zone of high saturation, if not ponding, that occurs underneath?

BODVARSSON: It's because it condenses in the fractures. If you take a look at an opening, then what you'll see is you'll start boiling, and steam goes out in all directions. If there is no gravity, you will have the same saturations all around. But because of gravity, water wants to go down, and it condenses more underneath, because you have a lot of boiling that sheds off, and you have increased saturation.

NELSON: But why does it reconcentrate underneath?

BODVARSSON: It's because you condense the steam. Steam condenses. And the steam condenses all around here. It condenses all around here, and the condensed water tries to go back into the boiling zones, gets re-vaporized again. Some of it sheds around, because it can't go anywhere else, either it tries to go through here and gets boiled again, or it goes around. And, therefore, you have a little bit more water below there.

NELSON: Is that really just a little bit more water, or is that a lot more water?

BODVARSSON: It's a tiny bit more water. I can't emphasize enough that the increase in saturations in the fractures, it sometimes looks like a big blue lake in some of our pictures. It's not a big blue lake. The increase in saturation is a tiny one, going from perhaps 5 per cent to 10 or 15 per cent, and we don't know exactly how high it is, because we can't measure fracture saturations because they're so small. But why do we know it's not so much? It's because the permeability of the fractures are so high, that when you start to increase saturations in the fracture, the relative permeability of the liquid phase increases so rapidly, you lose the water.

NELSON: Permeability of the liquid phase?

BODVARSSON: Yeah, because it's condensed water. The relative permeability of the liquid phase increases.

NELSON: Let me ask you a question about this simulated saturation. It has a very flat top, and it has a topography on the top of the dry zone and the boil zone. I'm wondering if spatially you could imagine there being a topography on the top of this line of waste packages, which might be significant enough that you could end up having some ponding of moisture, at least in significant high saturation, at various places above that, just because of the topography and the variability of the rock? Do you think that would happen?

BODVARSSON: No. And let me tell you why. The range in fracture permeability, the measures on a one foot scale, which is a very small unit, it's not like on meters and meters, but a one foot scale, varies from some millidarcies to darcies. Or if you're used to meters squared, from some 10^{-15} meters squared, to 10^{-12} meters squared, if you're used to those units.

In order to get ponding, that means the permeability is so low that the water can't go through it. You would have to go orders of magnitude lower in permeability to get that.

NELSON: Well, I'm talking about using that boiling front as a barrier, and if that has a topography, you could, and have the boiling front be the confinement.

BODVARSSON: The way I look at it, Priscilla, the condensation is on the fracture, you're right, and you look at blue colors, but the increase in saturations in the fractures is very small. What happens to that water is the permeability laterally is also high, so if it can't go down, it will go laterally and disappear, and you never build up full saturations in the fracture zones. The permeability of these fractures is just too high.

NELSON: If there is a topography on the top, because there are heterogeneities in time and space, that it seems like it would be possible to have isolated accumulations.

BODVARSSON: It would be possible if the lower end of the permeability we measure is three orders of magnitude, or two orders of magnitude lower than what we measured in the low end. But since it's still so high, any saturation you build up goes laterally and dissipates. So, I'm sorry, I can't see the real possibility of this, because of the permeability of rock is just too high.

NELSON: Well, that's why I'm suggesting the topography on the top of the boiling zone that may actually be enough to stop effective drainage, which is what you're talking about, drainage and leaving.

BODVARSSON: Yeah. But, you see, the water molecules here have two choices. It doesn't just sit there. It has forces placed on it. Forces are gravity (a), capillary pressure (b), either imbibed into the matrix or it tries to go down here or it flows laterally, and it doesn't go down or pond up here. We have seen it in none of our simulations, even though we include all the heterogeneity in our models.

DUQUETTE: As Chairman, I'd like to note that we're on Slide Number 17 of some 60. If we're going to get through the presentation, I'd like to ask that questions be limited to questions of clarification about the data at the present time, and that the discussion afterward can discuss opinions.

Thank you.

BODVARSSON: Okay, I'll try to move through quickly.

Now I'm going to talk about some of the modeling that we have done to look at exactly the issue of will we have seepage into the drift during the thermal period, as was asked before.

And this is the thermal-hydrology seepage model that looks at that issue, TOUGH2, 2-D cross-sections, dryout zone, heterogeneous permeability, and flow focusing factors.

This shows again the heterogeneous permeability we talked about here, and you see that in the different colors, because the saturations in the fracture systems, and let me point out again here is the condensation zone, Priscilla, and you have saturations on the order of 10 per cent, or so, in the fractures, because the permeability is high and it sheds very, very easily. You see a dryout zone after 100 years, dryout zone after 500 years. It goes about 5 to 10 meters away from the drift maximum.

After 1000 years, it becomes smaller. You see it's still dry all the way around the drift. After 2000 years, it's much less, or non-existent dryout zone along the drifts. And you're back to close to ambient.

These calculations show the seepage we expect. This is the current climate. We assume that we get into a monsoon climate after 600 years, and then to glacial, much wetter conditions after 2000 years. This is the ambient seepage that is calculated, none for current conditions, about 1 1/2, 1 per cent seepage of the total water moving through the mountain after 600, and about 9 per cent in the glacial period for this specific location in the repository.

When you include the thermal barriers, because now you have two barriers, you have the capillary barrier, which works under ambient conditions, but you also have the thermal barrier, which prevents the water from entering the drifts. And you will see here in this calculation for this realization, you have no seepage into the drifts for 2500 years under these conditions.

If you take a look at this graph here, that's very, very significant because that's getting close to the lower part of this graph where we don't have any problems with corrosion rates, as Joe will show you a little bit later.

So, water cannot penetrate through the vaporization barrier as long as the local temperature at the drift wall is above boiling. And we have done a lot of simulations as well as testing that seems to agree with this. After about 1000 years, temperature drops below boiling, and then you have a potential for seepage, in this case, not until after about 2500 years. Long term ambient seepage, of course, defines seepage during the whole period.

LATANISION: Mr. Chairman, I'm sorry, I've got to ask a question. This is clarification.

Despite what you've just said about the absence of penetration by seepage, the drift still is at 100 per cent relative humidity; is that correct?

BODVARSSON: It's dry. The drift is dry, because there is steam there. So, there is no liquid water present.

LATANISION: Okay. No liquid water, but there is steam present.

BODVARSSON: There is steam present in the drift.

LATANISION: Okay.

BODVARSSON: This is relevant to the questions we heard earlier raised through Dr. Chu and also John Arthur, and this shows something which I think is very important to keep in mind, and that is because of the 81 meters distance between the drifts, the thermally disturbed boiling conditions is very close to the drift, and you have no difficulties with drainage between pillars. And, therefore, the design, even though you have some differences in thermal conductivities or thermal parameters, it has really no effect on the design per se, because there is such a long distance between the pillars you are never going to get convergence of the boiling zones, even though you are way off in thermal parameters.

We did just very briefly, and I'm not going to describe this in detail, we did an alternate model just to convince ourselves our results are right, where we took the condensation zone waters here, and we just pumped them up, even though we never see that in the simulation, we pumped them up so we have a pond here where we instantaneously tried to get it through the boiling zone.

And the next slide shows the results, and you show, in spite of having huge capillary suction above the drift, which is this red curve, you never get any water to the crown. This is the water going to the crown that has succeeded to go through the thermal one, it doesn't start until after 400 years, and after about 800 to 1000 years, you only have a small amount of water making it to the crown, which is much, much less than that that the capillary barrier can take care of. So, even having a ponded thing there to try to force through the thermal zones, we do not get any seepage during the thermal period.

I know this is difficult to explain, so if you have questions later, because of time, I need to move through it fairly quickly.

Now chemistry. Thermal-hydrological-chemical processes. Now, in addition to boiling and condensation, we have a bunch of chemical processes that are occurring also. You have dissolutions, you have precipitation, you have changes in porosity and permeability not only of the fractures, but also of the rock matrix, and then you have reaction rates and pH effects, and others.

The numerical tool, TOUGHREACT, is one we use for this purpose that solves multiphase flow, advection, diffusion, and a lot of chemical processes, including dissolution, precipitation, and all of those.

Our model, just like the thermal-hydrological models, we do blind predictions. There's no calibrations here, blind prediction against measurements in the drift scale tests.

Before we actually match the drift scale test, we make sure that our initial conditions of the models are correct by matching the ambient geochemistry. That's the geochemistry we see in the mountain right now, including chlorides and strontium and other chemicals. This is a chloride model that shows the variability of chloride in the mountain, and we compare that to what's measured in the ECRB in the tunnels, and we find good agreement.

Some of you may say this doesn't look like a real good agreement because there's a lot of variability in this data, but all of this data to me is the same. This data suggests that the percolation flux is roughly 10 millimeters per year. If you go up here, it might be 6. If you go down here, it might be 12 millimeters per year. So, this, even though it shows a lot of variability, the numbers are pretty much the same. So, the matches are very reasonable with this dataset.

Then after we have made sure that the model is appropriate for the ambient conditions, we look at and try to match, or actually try to make blind predictions about the chemistry that happened in the drift scale test.

The first thing we look at is carbon dioxide gas, and one of these curves shows the measured data, and that's the black points, and the other one shows the blind predictions with the model, no calibrations. And, first of all, what happens? You have imagine a pore water system with air surrounding water in the matrix. The water has CO2 in it, because there is always dissolved CO2 in it, and other gases in it. When you start to boil this mass, the CO2 goes out of the liquid phase, because CO2 gas does not want to be in a liquid phase. It wants to be in the gas phase with the steam. So, you start to get an increase in the CO2 concentration due to degassing of the liquid phase.

Then after a while, you have completed all the transformation of the CO2 from the liquid phase into a gas, and you have no more left, but you are still boiling away the rock, you're still boiling and boiling and creating steam, so the fraction in the gas phase has to go down with time, diluted by the steam. So, that's what this shows, and our blind prediction of the model matches that very well.

Along with that, you also have other changes. Number one, you have changes with the chloride concentrations in the liquid, and you have changes in the pH because of the degassing of the CO2. When you degas the CO2 or increase the CO2 concentrations, the pH goes down. When you are deplete of the CO2, the pH starts to go up again.

Two points here. Number one, and this is by far the most important point, and that is you have seen calculations of chemical components with pH going to 2, pH going to 10, pH going all over the place, and you may have these very corrosive brines present because the pH goes all over the place. pH will not go all over the place. The pH varies only from 6 1/2 to 8 1/2, very little. And why is that? It's buffered by the rock, by the pore waters and the chemistry present. This is an open system. You cannot boil off in a beaker, get a pH of 2, stick it around your waste packages and calculate corrosion rates. You cannot do that. pH will not vary significantly.

The chloride, this is chloride measured in bore holes where we actually got condensate in the bore holes. This is the chloride in those blocks next to that, and it shows very much the same trend. There are also two things with respect to chloride. Number one, if you have a lot of interactions between the matrix and the fractures, pore water that condensed in the fractures, the chloride concentration would go up to the same as that in the matrix, which is roughly about 80 milligrams per liter. But because the interaction is so small, it stays very low, around 10 or 20 milligrams per liter. So, that shows the interactions between the fractures and the matrix blocks is very strong.

We also calculate precipitation and dissolution in fractures. We predict calcite deposition, although this is silica depositions, and we predict calcite depositions because of different processes occurring. When you boil off water, the concentration in the beginning is in equilibrium with the ambient silica concentration for 20 degrees. You boil the water off, the silica concentration has to go up. So, what happens then? Precipitates, obviously, the inverse is true with calcite, the solubility of calcite goes down with temperatures. So, in the condensation zones, then you reach the saturation of calcite, and you get a little deposition.

The important point here, though, again blind predictions, we drilled into the zone with bore holes, and we find the calcite and the silica we predicted would be there.

Now, using the confidence of our model after validation with the heater test data, we now want to predict what is going to be the chemistry of the water entering the drift, because that is what the corrosion environment around the waste packages is very sensitive to.

So, this model, thermal-hydrological-chemical model, again TOUGHREACT, predicts the composition of gases and water that could enter the emplacement drifts, and also how permeability and rock properties would change with alteration, or deposition of minerals. And we did a bunch of sensitivity studies. I will move a little quicker because I know I'm short on time, and stop me if I go too fast.

Choice of initial waters. This is very, very important because the pore water entering the drift comes from the pore water measured in the rock. So, we have to make sure we represent correctly the variability of the chemistry of the pore water in the rock. And I'll show you how.

This is the same diagram that Bill showed you before. It shows all kinds of measurements that Zell Peterman, who is sitting in the back there, has done and Al Yang and others at the U.S. Geological Survey, chemistry for these three components of the chemical environment. One thing that's important is that we cannot use all this data in our model for the chemistry of the water entering the drift. Why is that? Some of it is saturated zone water, which is not close to the repository, some of it is in the PTN, Paintbrush unit, which is not close to the repository. You can only use the ones which are in the repository rocks.

Those are the only ones in the repository rocks, in the middle non-lithophysal or lower lithophysal rocks. These are the ones we use in all our calculations of the chemistry of water entering the drifts. You can't use all of the other datasets because it's not applicable. You must use this one.

Looking a little bit closer at the chemistry instead of just these three chemical components, we have a cation diagram here pretty much, an anion diagram, and then the sulfates and sodiums and all the others here. And what I want to point out is that each of these diagrams, you see all this stuff here with all the datasets come from different areas, from bore holes or from the tunnels. The green ones are the five waters we actually picked that reflect the variability in this region. They reflect the variability in this region, and they reflect the variability in all regions. This is all the available data, and we think these five sets, because we can't handle them all, really reflect the heterogeneity and the variability in the pore waters. That's why we use these five.

In order to calculate this tough problem of chemistry entering the drifts, we have to have all the minerals, and the models include 20 minerals ranging from clays to feldspars to all the rock types that are actually measured. This is the real rock and it includes all of those minerals, because they can dissolve in the water. They can also dissolve in the gas, in the steam phase. We also must have all the major aqueous components, which we have, anions and cations and others, nitrates, iron, fluorides, and I'll only show you a few examples. You must also include all the gas phase components. There's going to be steam, water vapor, air, CO2, and other gases.

This shows you now the crown conditions, what happens at the crown. First, for different realizations, different thermal conductivities, different thermal parameters, different chemistries. Important is not really the variability, but the general trend that I'm going to go through fairly quickly. This just shows temperature at the crown. Of course, when you ventilate, you have low temperatures then. It goes up and it goes down, just like Joe's picture here.

CORRADINI: Can I just clarify? When you say the crown, exactly where are you talking about? I'm sorry.

BODVARSSON: Right on top of an emplacement drift.

CORRADINI: So, on the inner surface of the drift?

BODVARSSON: Yeah, in the outer surface of the drift, in the rock, it's in the rock.

CORRADINI: How far in the rock?

BODVARSSON: It's just next to it, it's the next grid block that is probably within centimeters of the drift.

This shows again similar things we expected from the drift scale test. You have degassing over long time periods, and then you get a little bit more CO2 because it's condensed up there and started refluxing, and you get higher values of CO2 or bicarbonates.

pH, this is the dryout period where you cannot define pH if there is no liquid present at the crown of the drift, you can't define pH. But the pH then starts to be very low because you're getting back CO2 refluxing when you rewet again, and then it goes back up to normal. But you see again the scale, 7 to 8 1/2.

Chloride concentration, nothing happens of course during the dryout period. But, here is a very important point. When you rewet again after the thermal period, there have been some issues with salts precipitation on the drift. Of course, it's extremely corrosive environment in the drift because due to the boiling process, when you dryout, you must have salts there. Right? Very, very little salt will concentrate there simply because the water is so dilute. The water at Yucca Mountain in the pores is 1000 ppm. There is not much salt there. Therefore, you see instantly going from a large concentration of chloride with the first water hitting the drift wall, to benign waters because of that.

The other one, just like we did with the seepage model, we now want to look at the chemistry of the water in the condensation salts that we talked about before. And, again, you see blue here. The blue is only about 5 to 10 per cent in the fracture system. So, we are saying let's look at the chemistry of this water here, and see if that goes through the boiling zone, what would it be? What would be the chemistry going through the boiling zone? So, I'm going to show you a series, starting with calcium to bicarbonate ratios in the fracture, and Bill Boyle pointed out very correctly that the calcite is the main buffer. If you have a lot of bicarbonate present, even if you have calcium present, calcite is going to deposit if you have sufficient carbonates present.

And this shows initial variability, which is just because we have the different pore waters present, initial variability going to very high calcium to bicarbonate ratio, which would say maybe I'll get calcium chloride brines. But, note this is during the thermal period here where we have no seepage into the drifts. So, this will never get to the drift. And you see that clearer in this picture here where we plot the temperature of the drift crown versus the calcium to bicarbonate ratio, and one sees that it goes below one at about 104 degrees at the drift. That's still boiling at the drift.

So, one would conclude that when you have a chance of seepage in this area here, your carbonates are enough probably to cause that you will not get any calcium chloride mixtures in the drift causing corrosion.

pH, again, we see the variability due to the CO2 concentration, but generally between 7 and 8.

Chloride concentrations, variability in the beginning goes down, and then up a little bit, but very similar to what we saw before.

So, I'm almost there. General findings from this chemical seepage model is the water in the condensation zone is dilute and the pH again is 7 to 9. Elevated concentrations are predicted for a short period during the rewetting, but that's an extremely short period. You will still have benign water, or very low concentrations. General trends of evolution for the gas phase do not differ significantly for any of the scenarios considered. Most of the five waters show very similar trends.

Fracture permeability has insignificant effects. The relative spread is basically mostly the natural variability of initial pore water composition. You don't have a lot of divergence after you consider this pore water distribution with the chemistry.

So, to conclude, I conclude the following. I'm concerned with what happens during the thermal period, how much water will seep into the drifts, what is the chemistry of this water entering the drifts, so that Mark can look at the in-drift conditions and Joe will then look at the environment around the waste packages. This is what I conclude with respect to this.

A significant database exists for evaluation of ambient and thermal seepage and the chemistry of the seepage water (a). We have significant amount of data that I have shown you here. The relevant models, TH and THC coupled processes models are well validated using blind predictions, again, high quality seepage and drift scale test data. No seepage into drifts is expected to occur at above-boiling rock temperatures. This is very important. If the drift wall temperatures at the crown are above 96 or 100 degrees, we expect no seepage in the drifts.

Ambient pore waters in the repository units are dilute, total dissolved solids about 1000, and their variability is well represented by the five different initial waters chosen. We chose the right water based on the data that we had to look at what is the chemistry of the water entering the drifts.

Fracture water above the drifts is more concentrated during the boiling period when no seepage is anticipated. And, obviously, that's the case, because you boil off some of the water and you leave the rest of the water with the higher chemical concentrations. But during this period, we do not expect any seepage to occur.

Chemistry of the seepage water is at all times very similar to the initial dilute pore waters. The calcium/bicarbonate ratio is much less than unity, at times when seepage can occur, suggesting that calcium chloride waters are unlikely to evolve, based on that water entering the drifts.

Large variability in the initial pore water composition is predominantly responsible for the spread in the final concentrations. So, basically, the initial variability in pore waters is the uncertainty and not the model make it diverge in the different chemistries for the rock.

Sorry it took so long.

DUQUETTE: Thank you. I want to take the Chairman's prerogative and make the first comment, and that is all of your models are based on absence of a container inside, or even a drip shield inside the drift. I think that the corrosion people, myself included, are more concerned about what happens in potential crevices, whether they be manufactured or naturally occurring because of precipitates, where the local chloride concentrations and pH can be quite different than what you calculate based on equilibrium with pore water and the absence of the containers being present. So, I think one has to be very careful in not confusing the pH and chloride that the corrosion people are concerned with, which is at the container level, versus the pH and chloride concentrations in the water itself.

BODVARSSON: Very correct, and I agree with you. The only thing I said is that the seepage water entering the drifts will have a pH of 7 to 9, and whereas when you calculate it going to the waste package that Mark is going to talk about and Joe is going to talk about, they will show you probably different pHs than what I talked about coming into the drifts.

DUQUETTE: Right. If you have a crevice in salt water, just conventional sea water, where the pH is at about 8, the pH inside the crevice, if it's in contact with many passive metals, can get down as low as .5 in the crevice itself, which is independent of the pH of the surrounding environment. So, we have to be very careful in looking at those pHs.

BODVARSSON: And your point is well taken. I'm only talking about the pH of the water entering the drifts.

DUQUETTE: I understand that. Paul, and then Ron.

CRAIG: Yeah, that was fascinating, Bo, and it reminds me of the paper by Roseboom and Winograd back before I had ever heard of Yucca Mountain.

BODVARSSON: 1981.

CRAIG: Yes, it's before I ever heard of Yucca Mountain. Exactly so. And, basically, what you seem to be coming up with is that their model was a pretty darned good model, and when you examined it in detail, it comes out about the same, although they did allow for some very fast flow, which they thought would drain out through cracks in the rock. But, you're not even seeing that. And then the heat only makes things better. So, it sounds, if I understand what you're saying, if you were to take bare fuel rods, or practically anything, it doesn't matter what you put in there, nothing is going to come out for several thousand years. And then when you go beyond the several thousand years, what kind of mechanisms finally begin to transport material? Am I expressing my conclusions correctly?

BODVARSSON: Yes. See, my opinion, and I guess I have to say to be honest with you, the question is my opinion and not necessarily that of the project. My opinion is exactly like that, Paul, is that you will have no water entering the drifts for thousands of years, in my view, in almost all emplacement drifts (a). And (b), not only will you dry out the rock around the drifts, but more importantly, too, you'll dry it below the drift. So, in order to get things out, which was your question, you have to diffuse because you can't get it by liquid flux. And if you diffuse it into that region, as I call the shadows zone region, which is dry, it takes thousands and thousands of years, and maybe even 10,000 years, to go a few meters. So, I think we have, even though we have taken credit for a lot of what is happening there, I think the real system will be even more robust than what we are taking credit for.

Did that answer your question?

CRAIG: Yes.

LATANISION: Latanision, Board.

I, too, found that fascinating. I thank you for the full presentation. It was very helpful. One question that remains in my mind, however, is where does the issue of deliquescence enter into the equation from your perspective?

BODVARSSON: It enters into it through Mark's presentation. (Laughter.)

LATANISION: Wait a minute. That's not fair. We can put it off until Mark speaks. I mean, it seems to me that that's a subtlety in this comprehensive presentation of yours, and it still needs to be included.

BODVARSSON: Absolutely, and I think Mark will handle it very well.

LATANISION: Thank you.

PARIZEK: Parizek, Board.

Bo, it's always interesting, the presentation, and with the confidence that you give us. Is there any more work to do? Margaret has a problem with funding, and there's limits on dollars. Are you done with this? Or what are your priorities? Because, obviously, there's work to be done here. I mean, we feel so comfortable having heard your presentation, that you should just be done, give back your money?

BODVARSSON: I'm pretty much done, and I'm going to give all my money back. (Laughter.)

PARIZEK: Be honest, this is your chance.

BODVARSSON: Yeah, I don't know what to do tomorrow. To answer your question--

CHU: Here's your big chance, Bo.

BODVARSSON: Okay. Here's my big chance. My big chance. What I think is important in my view, and that is in the local areas, too, I, we, have a lot of confidence in our results. We have a lot of confidence in our models. We rely a lot on our models and our confidence. We rely a lot on the very robust waste package, for example. The thing that Paul asked about in terms of the natural system can be strengthened to the extent I think we can put waste, without any waste package, into the ground and have a great confidence without relying on it, and I think that would be very, very important to do.

Without going into further details, the important thing is to make sure we are all comfortable with the risk we are taking, and we all have, I'm sure Margaret, she has to look from the top pictures and be comfortable, I have to be comfortable as a manager and a scientist, and you have to be comfortable as a Board member. And our degree of confidence may vary. I am very confident in what I have presented, and some of these are stronger than other areas, and I would like to think that we will strengthen those as we go along, and we will debate what is the right thing to do. And I think I'm very pleased that Margaret relies so strongly in the science and technology program. So, I think through those, we can build our case more and more, and all of us having a lot of confidence.

PARIZEK: I mean, there's obviously work, you still have priorities of things you would do, given the opportunity to do that.

BODVARSSON: Yes.

PARIZEK: I didn't want him to give all his money back. I seems like there's work to be done.

And then the temperature, you brought us about 2500 years, was what temperature now? Was that the high temperature design, lower temperature design? Because you're saying really, from your point of view, keep it dry longer, and that's to everybody's advantage.

BODVARSSON: Yes.

PARIZEK: We won't know what the metals do right now, but keep it dry, and we've got 2500 years before you get water in it, if water wanted to get in there.

BODVARSSON: Yeah. This is the basic design that we have had all along, which I think you would call the hot design, where you actually have boiling in the rock. And that gives you really the additional thermal barrier that I talked about.

PARIZEK: Yeah. But on Page 24, you show this little tiny little space, well, the 81 meter spacing, and this little tiny shedding. Why can't you then put drifts closer together. Priscilla was asking you about that. In other words, you don't need much space. Look at that space, all that wasted rock that you could put repository in. So, the footprint is littler. What's the answer there? Is it heating up down below, or heating up above?

BODVARSSON: No, I--my answer is the following. We all have a different level of confidence and a different level of the way we want this to work. For example, a big issue with the NRC, for example, is drainage between pillars.

PARIZEK: Yeah, but that's like a woman with blue hair, and you could get women in the mountain.

BODVARSSON: A woman with blue hair?

PARIZEK: Well, I have. But I'm saying you could get more in there. You could get more women in there.

BODVARSSON: Yes, I could get more women in there.

NELSON: Excuse me, Richard. (Laughter.)

PARIZEK: But just in terms of design itself, and so on, but a chance to talk about this 81 meter spacing.

BODVARSSON: Yeah. It's an issue, and I'm sure Nancy and Margaret and others are considering it. But, you know, you have a lot of, or it seems like you have a lot of confidence that you have plenty of drainage, the thermal is not going to kill you right now, and NRC might be confident that it is so. I don't know. That's up to them.

PARIZEK: I'm sure there are other reasons to get into the design question. We didn't hear anything about I guess it's the mountain scale behavior, that's the Chlorine 36. When you calibrated models with it in, I mean, that Chlorine 36 doesn't exist as a question anymore.

BODVARSSON: Yeah.

PARIZEK: Will that cause you trouble? And I guess it's another day's discussion. I mean, that's not part of this presentation.

BODVARSSON: Well, to answer that question, I might have Mark tomorrow, Mark is going to talk a little bit about it tomorrow. To me, the Chlorine 36 has never been a big issue for the site scale model.

PARIZEK: I thought you calibrated with it in.

BODVARSSON: We calibrated with it in, but it's only like 1 per cent of the total mass is affected by the fast flow paths. If they are in the model, I would not spend time or money to change the model, if there's no Chlorine 36, because it just doesn't have its effect. So, therefore, I think the impact on our large scale model is very small.

PARIZEK: So, it doesn't adversely affect it.

BODVARSSON: It doesn't adversely affect it.

PARIZEK: Some while back, you talked about in two weeks, the silica plugged up the crack in your laboratory experiment.

BODVARSSON: Yes.

PARIZEK: But, we haven't heard any more about that. Have you ever resolved that problem, what happened to the silica and why it sealed the crack so quickly in the lab? And just to be consistent with the observations you've been showing us, it's one of the graphs there.

BODVARSSON: You have too good a memory. (Laughter.) This is absolutely correct. We did a lab experiment, for those of you who didn't know, we were concerned with what happens when you actually precipitate silica or calcite, and the concern obviously is that if you (a) if you seal it up, because silica gets deposited with the boiling front, because that's where you concentrate pore waters, and so we did an experiment with the lab and it sealed up in two weeks totally in the area. We see that also in the 3-D model results that I showed, THC, that it's still an issue that the project must address, because if you seal it up and then you have a hole in it and you focus water, that can have adverse effects. So, honestly speaking, I still think that's an issue.

PARIZEK: So, let me ask, because a gentleman's daughter from Westinghouse competition years ago ran an experiment like that and showed us silica reprecipitation is an issue. I think I saw him earlier.

BODVARSSON: I cannot say that it's not still an issue.

PARIZEK: One last point. Define the blind prediction. I understood it, I think, but blind prediction, you were right on. Now, that's based on the theory, you have a solution, or just to make sure we all understand blind prediction.

BODVARSSON: Blind prediction is simply that what we often do, we always like to predict tests, and that's what this project has been very good at, we want to predict to see how good our model is. Often, our prediction is not so good that it comes back not the way we thought it would be, and then we calibrate, we change our models and parameters until they actually fit the data. That's what is called calibration. It so happened that with drift scale tests, and we are very pleased with it, and that's why it gives us confidence in the models, is that we actually predicted and did not have to calibrate, basically the temperatures rose as we expected. The partial pressure of CO_2 fell and then rose and the pH in the water did what we expected. And that gives you a lot more confidence, than if you have to turn knobs.

PARIZEK: But that was based on analytical theory. So, you has a reason.

BODVARSSON: That was all based on we put so many things in there, we put in the mineralogy we measured, we put in the air permeability we measured, we put in the pore water chemistry we measured. Everything was put in that model, and then we do blind predictions.

PARIZEK: And I do apologize to Priscilla for the blue haired women.

DUQUETTE: For the record, there are people with blue hair.

We're running a little bit over. I'm going to allow three more very brief comments from the Board. Priscilla first, Dan next, and Mike last, and before I do that, I was asked to make an announcement that a luncheon buffet is offered in Jeffries, plus the regular menu.

NELSON: Nelson, Board.

What about rock bolts, steel sets in terms of changing the permeability over the crown rock, changing the chemistry of the water, introducing new cations? Whose responsibility is that part of the evolution? I mean, you could say it's the natural system, but you could also say it's not. Is that Mark?

BODVARSSON: That's Mark.

NELSON: Okay, Mark, I'll ask you that this afternoon.

BODVARSSON: I think to answer your question a little bit, Priscilla, we have been concerned that perhaps the rock bolts are going to increase seepage around them, because they generate a pathway, if you will, because you drill up there. All our studies indicate that's not the case, because there's such a small volume of water that can access it anyway.

With respect to the chemistry affected by rock bolts, I think that's more in the in-drift environment. I don't know if Mark is going to handle it, but maybe you can address it in yours. I'm strictly rock in this presentation.

BULLEN: Bullen, Board.

Could we go to Slide 50 just real quick? Your second point on Slide 50 basically made the comment that for a relatively short period of time, you'd have the effect of the chloride in the drift, which I think is your second one, elevated concentrations of aqueous species.

BODVARSSON: Yeah.

BULLEN: Predicted for short periods of time. Go back to Slide 45 now. I just had a question about that elevation there, and the time frame of it. Could you explain to me what the 1080 and 1520 mean? And it looks to me like the heat Rev 1 that you have sort of takes about 500 years or so to come back down to ambient. So, is 500 years a short period of time?

BODVARSSON: This is the values of chloride that are not plotted on there. They were way up there in the beginning.

BULLEN: All right.

BODVARSSON: In all honesty, my recollection of this is that it drops practically instantly down to a few hundred milligrams per liter, because the amount of chloride is just tiny. So, this occurs, most of this occurs in--

BULLEN: In one PA time step?

BODVARSSON: Yeah, like tens of years.

BULLEN: Okay. So, the time frame of 500 years I'm looking at is sort of an artifact in the calculation?

BODVARSSON: I think it is, because what we can do for you, and I've asked Doug Shirley and my people to do this, is to calculate the volume of minerals that are on the drift surfaces, and they did it, and it's a very, very small amount. You just can't get aggressive brines for long periods of time. You get small amounts.

BULLEN: Okay, thank you.

CORRADINI: One last question. So, I want you to, either one of our cartoon images there, so the way you describe the story, is it independent of the power generated in the drift between that purple area? I was told the lower end of that purple area is 1 kilowatt--

BODVARSSON: Yes.

CORRADINI: Just let me say it out loud, because I might have it wrong, so you will correct me if I'm wrong. So, the bottom part of that purple band is 1 kilowatt per canister on average?

BODVARSSON: Right.

CORRADINI: And the top one is 12 kilowatts per canister on average?

BODVARSSON: Yes.

CORRADINI: And the story you just presented sounds to me in the first thousand years, independent of that power.

BODVARSSON: Yes.

CORRADINI: Okay.

BODVARSSON: It's independent of that power, and let me just clarify it a little bit. If you have an average power of, say, five, and then you have the 12 kilowatts, and then you have the 1, so the average is 5, this story is dependent on the average. But the average of the waste packages that show a fairly large temperature variability in the drift, do not go very far into the rock. So, that's why everything is averaged that way. So, the story is pretty much the same for rock distribution of the waste packages.

CORRADINI: Can you repeat that one more time? That's actually important, and I didn't catch it. So, say it again. Do you mind?

BODVARSSON: I'm saying that if you have assemblages of waste packages with various thermal output, they have an average thermal output of X, the distribution of the waste packages are not going to matter a heck of a lot. It's the average of X heat output that the rock sees, because the rock doesn't see a lot of variability within the drift, because it's averaged so much out within the drift, because the heat transfer of the waste packages to the drift wall is by radiation, and if you have a hot one here and a cold one here, it still radiates in all directions. Is that clear?

CORRADINI: I've got it.

DUQUETTE: That concludes this morning's session. Before actually concluding, I'd like to thank the speakers in the technical part for really very nice and complete and well prepared presentations, and the Board thanks you very much for that.

We'll convene exactly at 1:30. So, we'll look forward to seeing all of you then.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

CRAIG: Okay. We are now beginning the second part of the Technical Session. I'm Paul Craig and I'm chairing this session. We have what turns out to be about four hours to talk about a bunch of different things; three talks in four hours. So, we do have time.

We're going to begin with Mark Peters from Los Alamos who is going to talk about the Character of the In-Drift Environment, then Joe Farmer of Livermore, Materials Performance, and then we'll take a break, and then after that, Bill Boyle comes back to talk about the Technical Program Summary. Then, we're going to have a lot of time for general discussion, and at 5:30, I will give up the Chair to Mike and we'll have public comment. If you want to make public comments, you should register with Linda Coultry or Linda Hiatt out at the table in the back of the room. And, you are, as always, welcome to submit your comments for the record.

We're going to begin with Mark Peters. Mark, have you ever actually lived at Los Alamos? No. When I first knew him, he was in Las Vegas and now he's living in Washington. Okay, he gets around. So, Mark, I'll give you a warning when you've got about 10 minutes left, and as with this morning, but with slightly more enforcement, please, hold your questions until the end of Mark's talk unless you can't hold yourself and you have a question of clarification.

PETERS: Okay. I'm going to wander, if that's okay.

Let me start by saying I'm going to be picking up from where Bo left off. He walked us through the seepage piece during the thermal period, in particular, dropped the water to the drift wall. So, looking at the poster, he focused, in particular, up in this region here. I'm going to talk about regions maybe more than you've heard up until this point today where you've got an above-temperature region, a transition region, and then a below-boiling temperature region. So, if I mix those in, I'm talking about those three regions. But, again, I'm going to evolve the chemistry from seepage at the drift wall and evolve it in the drift, and simplistically speaking, hand that off to Joe who will then talk about corrosion of the base metal and also about the welded zone along the lines of what Ron was asking about.

You know, I don't want to belabor what was already said this morning by Bill and Bo, but this is really a mechanism for us to put together our understanding or our basis of the system for this piece, these set of features, events, and processes; the coupled processes, the in-drift environment, and localized corrosion on the metals. So, again, protection by the rock in general terms, protection by the alloy. There's this transition zone where it's very important. These may very well overlap. Bill kind of alluded to that, but it's very important to understand chemistry throughout the system, but particularly in this region here.

I also want to say that I am not the modeler. I think everybody knows that. I'm speaking for a wealth of people who have done a lot of very good work at Bechtel SAIC in Las Vegas, at Livermore, and at Berkeley, and I'll be presenting the results and our basis. I've reviewed it extensively so I can sit up here and speak with confidence, but I don't want to take credit for the good work. But, if it's a bad presentation, blame me. Okay?

So, next slide, please? I'm going to walk through real quick what I've already said basically. The purpose of the presentation, talk a little bit about the three temperature regions. I'm also going to want to emphasize as we're going through, don't forget about the X axis here. How long are we spending in the different temperature regions? That's an important component to this. Up here, you know, you're above-boiling for a significant chunk of time. You're really below-boiling for a significant chunk of the 10,000 year period. You're in this transition zone near boiling really for a very short period of the total compliance period. That's an important point that I think needs to be brought out, as well. So, I'm going to talk some more about the three temperature regions, talk about the evolution of the in-drift chemical environment, bring up chemical divide theory, the ternaries that Bo and Bill have shown will show back up. We'll talk a little bit more about how the water is represented in that idealized ternary, talk about the in-drift water chemistry model, our basis, our validation of that model, then talk about deliquescence. Bo handed that off to me. So, we're going to discuss deliquescence. Then, wrap up with emphasis on my part of the picture, but also hammering home some of the points that Bo made and also pointing forward to some of the points that Joe is going to make and finally just conclude.

So, again, I'm presenting the in-drift chemical environment modeling approach that's being developed for the License Application and I'll talk about the basis, the validation basis, for that model. It's going to be broken up into talking about it in two components. Let's keep it at two components. First, I'm going to talk about the seepage aspect. Bo's brought the water to the drift wall. That's potential seepage into the drift. It's got a very dilute chemistry, a fairly narrow range of pHs. That is possibly water that could drip in. That water will be evolved in the in-drift environment through evaporation as a function of temperature and relative humidity and other parameters. That's the first part. The other aspect of the system is deliquescence that could occur in evaporative salts that have gathered on the metal surfaces, or for that matter, deliquescence on the dust that's got on the waste package surface. That will be the second piece that I'll talk about. And then, we'll wrap up.

So, to summarize, meet up front, and some of this again is what Bo's already discussed, the chemical evolution of the seepage waters controlled by temperature and relative humidity in all three regions of the stages of evolution; the dry-out, the orange piece, the transition, this greenish-gray piece, and the low-temperature or blue piece of the graphic that we've got over here on my left.

Bo's already also talked about the second bullet, the variability and uncertainty--oh, excuse me, he was talking about the variability and uncertainty in the pore waters, capturing the variability and uncertainty that you get when you bring the water to the drift wall. This is also getting at my summary where I'm talking about the variability and uncertainty in the predicted composition of evolved seepage waters is accommodated in our approach for the model. I'll hope to convince you of that as I go through.

The concentrated waters that could support localized corrosion have been shown to be relatively rare, and even if they did occur, they'd be very small quantities.

Experimental evidence, as well as model calculations, on the influence of deliquescence of the dust or salts on the metals has been shown not to initiate localized corrosion. I will touch on that and Joe will then pick up and run with that from terms of how it fits into the metal degradation story or non-degradation of the metal story, however you want to put it.

The probable evolved seepage waters and deliquescence brines are benign. Calcium chloride brines are not expected.

And, finally, the drift environment is expected to be an "open" system.

So, again, just to--I really want to hammer home tying back to the graphic or the poster here, the dry-out region, again in the orange area, were above-boiling, were in dry-out. I use the word "shields" in quotes, but the capillary barrier, as well as the thermal barrier protecting the drift from any significant water seepage. Bo stated we expect no seepage during that dry-out period. Deliquescent brines can occur in the absence of seepage brines during that period.

Moving down-temperature into this transition region in here again, a short amount of time of the compliance period, but just the same, we do go through that period. The onset of seepage may occur. The chemical evolution of the potential seepage is of interest because of evaporative concentration that may produce brines and that's going to be the lion's share of what I'm going to talk about next when we talk about the evolution of potential seepage waters.

We are doing testing in addition to the models that I'll talk about. We're doing a lot of testing looking at representative waters and how they evolve in terms of evaporative concentration. And, the geochemical model has been developed to represent the water compositions in the drifts that would expect and we capture the variability and uncertainty in the processes.

Then, moving down to the lower temperature region, the blue region, where we're well-below boiling and really makes up the significant chunk of the compliance period, the seepage water chemistry evolution again is a function of the amount and chemistry of the incoming water, the temperature, and RH in the drift. There is an effect from committed materials. What I mean by committed materials, there was a question earlier about effect of rock bolts. Committed materials reads rock bolts, steel sets, grout, although there isn't grout in the drifts in our current design basis, but those are the sorts of things that we're referring to. And, finally, deliquescence can still occur, but remember we're at a very low temperature here and Joe will talk a lot more about temperature thresholds for localized corrosion and all that sort of stuff.

Just to hammer home that I'm breaking this up into really talking about it in two pieces, you've got--this is just a cartoon or a conceptual picture of the drift, waste package drip shield showing seepage water that could enter the drift and also deliquescence films that could be produced on the metal surfaces. So, these can again occur at a range of temperatures. Seepage, we expect to occur at and below-boiling. But, we've got a model which in general speak is termed the in-drift precipitous salts model. It's the model that defines and is validated that defines the evolution of the in-drift chemical environment.

I'm going to repeat some things, I think for emphasis that Bo's already touched on, but in terms of the boundary--I'll call it the boundary conditions--where we start from when we enter the drift. What sort of available data and measurements do we have? I mean, Bo talked about we've got 8km of tunnel in the exploratory studies facility. We've got pore water measurements from the proposed repository horizon levels, five of which Bo has taken and evolved through the THC coupled process model. We've got observations that Bo talked extensively about from our heater tests, in particular the drift scale test, and of the predictions and the measurements and the confidence that we have a valid model in that area. I would want to reemphasize that fact that we've been able to collect a lot of water from the drift scale test, in particular, and I think we've gained a lot of confidence in our understanding of the chemistry aspects of the system by being able to collect that water compared to predictions. And, finally, really in the in-drift environment, it's focused on laboratory studies where we're taking pore waters, representative waters, and putting them through evaporation experiments and also doing some experiments with deliquescence.

So, let's talk about the model for the in-drift environment. There's a handoff from models that Bo discussed that come through the THC time series, the calculations from the unsaturated zone THC model with TOUGHREACT. We take those water chemistries and through a series of--on here, it's called abstractions, but they're binning exercises and I'll talk more about what we do as we take that TOUGHREACT output. We're able to put those into a set of representative water chemistries that capture the uncertainty and variability that we'd expect to come in a seepage. Again here, I'm focused on seepage for now. Then, moving to deliquescence, we do an experimental evaluation of deliquescence. Then, also, an important point that's going to be brought up towards the very end is we also in our base is developing an understanding of the gas phase mixing within the drifts. That's real important when you start talking about being able--could we sustain, say, acid gas type environments in a drift? I'll get to that, as well, at the very end.

So, moving into the chemical divide theory, this is something that, I think, the Board has seen before. Greg Gdowski has shown it before in the past. It's out of Drever's textbook. But, it shows how natural waters through the influence of gypsum, calcite precipitation, gypsum precipitation, as well as magnesium silicate precipitates can evolve geochemically as a function of evaporation. I only want to bring it up because you can see--I mean, it's a somewhat idealized diagram, but it shows how one could through calcite precipitation and gypsum precipitation, for that matter, evolve to a chloride brine and it all depends on the relative concentrations of carbonate to calcium to sulfate in the system. It's a somewhat simplistic diagram, but important to bring in the context as I move into the evaporative concentration calculations.

A similar depiction of what Bill showed earlier, chemical divide theory. Here, we're representing in this again the same ternary diagram; calcium bicarbonate, sulfate. One question earlier, it's an equivalence. That's an important--that might have been clarified offline, but it is an equivalence. So, you've got calcium again by carbonate sulfate. It's an idealized ternary. I think that's probably obvious, but I do want to state that. We've got a multiple component system and we're projecting it onto a three component system, first of all. And, also, these boundaries are ideal boundaries meaning if there's any non-ideality in the system bringing activities that aren't equal to one, in other words, that will cause these boundaries to shift. That's maybe a subtle point, but it's important to remember particularly when we start talking real water compositions up here. But, we felt that it was a good way to walk through the system how it might evolve with the proper caveats.

Something you've already seen, Bill showed this and Bo also showed it. So, I won't dwell on it. But, this shows the same ternary, same divides, idealized boundaries with the different water chemistries. The pore waters that Bo started with are a subset of these five dark, I guess, that would be bluish-purple circles.

So, how does the in-drift water chemistry fit into the picture? We started with a coupled THC model. We take the output from the THC model which represents potential seepage. So, you've got a series of--you know, actually, move to the next slide, if you would. Let's talk through it here. This is kind of a wiring or flow diagram of how the models fit together. Bo has focused, in particular, on just these first two boxes; taking the ambient pore waters and evolving them through TOUGHREACT and bringing a set of waters in the system.

The result selection involves looking at all the date points and all the grid blocks and taking only those waters that make sense in terms of potential seepage. There's waters that are below the drift. Those aren't going to seep. So, it really focuses on narrowing in on the waters within the system that are likely to seep. So, that's the first set of criteria that we follow through.

Then, we also look at the time evolution of the waters. In that process which here is called generically a binning process, we were able to take the output from TOUGHREACT and represent that as what's going to, as you're going to see in the next slide, 11 Bins that represent the variability and uncertainty of waters that could enter the drift as seepage.

So, a lot of what I've just said, you get 11 types of water compositions. They can be evolved through many states. Importantly, the types of brines are determined by the source location if the initial brine, i.e. crown waters. And, I think, Bo clarified what we mean by crown. We can also extract the frequency of occurrence for each type of water and I'll get to a table that really is the take-home point from this part of the presentation. Then, I'm going to talk a little bit more about the binning process as it applies to starting water chemistries in the next couple slides.

A complicated diagram. I don't want to dwell on it. What I'm plotting here is molality versus time in years for several different species, also pH and Bin. I'm plotting molality of a chemical species on the left and pH and Bin are plotted on the right. What is this showing? This is showing one of the starting pore waters and showing how it evolves through the THC model. So, really, Bo could have spoke to this piece, but my part comes in because it shows how the different water chemistries as a function of time breakout according to the Bins, the 11 Bins that we come up with when we do the binning exercise to look at potential incoming seepage. So, a couple of important points. The pH, this particular one, here's that--you know, it's pretty constant, though really, I think, the nice job of pointing it out, this particular water chemistry stays pretty constant right around 8. But, as time goes on, the different aspects through the different aspects of the evolution, you can see how the data points break up across the 11 Bins. You can see these data points end up in Bin 3, whereas these up here end up in Bin 11. Again, there's a table here where I'll talk about all 11 Bins and talk about where they start and where they end in terms of their evolution.

Next slide is just another example for a different sorting pore water again showing how we trace through the THC model. You can see 11 seems to show up a lot and I'll get to the frequency of occurrence again in that table that I'm about to get to.

There's the table. So, we've taken the THC output, we've put it into 11 Bins that represent the variability and uncertainty of the potential seepage that might come in; so, Bins 1 through 11. The second column represents the time-integrated relative frequency for crown waters. Let me point out a real important point. These are for crown waters. These are waters that occur at the wall just inside the rock, as Bo put it, that first grid block, and it could potentially seep.

CORRADINI: A question of clarification. So, I think I understand it, but I want to go back. So, you predict what you think is at the wall. You then trace the chemical species and then you break them down into 11 representative groups, and by the prediction of how much they appear, you get essentially the frequency or the probability of seeing Bin 11, Bin 10, blah, blah, blah?

PETERS: Yes.

CORRADINI: Is that approximately right?

PETERS: Yes.

CORRADINI: Okay. So then, you are assuming that this will or could go into the drift?

PETERS: Right. I'm not saying it will drip.

CORRADINI: But, you're saying if it did, this is the composition?

PETERS: Right.

CORRADINI: Okay.

PETERS: So, that's an important link back to Bo's because Bo--you know, it won't necessarily drip, but if it did, this is the frequency of water--the frequency broken up and these numbers add up to 100 percent of the 11 Bins. Now, if you take those 11 Bins and you plot them--and you think about where they start their life in terms of chloride, nitrate, sulfate, bicarbonate waters, this column here is showing where they start their life. This RH is their final deliquescence point where they evolve to when you evaporate the complete dryness. And, if you evaporate the dryness, this is where they end up. So, an important point. Two of the Bins could evolve to a calcium chloride end point, but they don't occur in the crown. I think that's an important take-home point.

There's three Bins that make up the dominant water that we would expect that could seep into the drift. Those are Bins 4, 9, and 11 and you can see they evolve to chloride-nitrate, or carbonate chloride brines. An important point, nitrate, and we'll get to nitrate/chloride ratios in those bins, in particular, as this plays out, and then Joe is really going to talk a lot about the influence of nitrate on localized corrosion.

BULLEN: Bullen, Board. Just before you leave that one, a clarification question.

PETERS: Yeah?

BULLEN: The first two Bins are essentially the most aggressive waters, the calcium chloride.

PETERS: Right.

BULLEN: Why do you never see them?

PETERS: Because they don't occur in the crown.

BULLEN: Do they occur elsewhere?

PETERS: If you look at waters down along the side, if I did this thing for a combined crown and something that might leak in from the side, these numbers would be less than one percent. So, they occur, but they don't occur as seepage. It can't occur as seepage.

BULLEN: Okay. And then, I guess, the question is what are my error bars on this prediction of they don't occur in the crown, plus or minus what?

PETERS: Error bars on the end of--I mean, it's a simple fact, they don't occur in any of the grid blocks that we would expect to seep throughout the time.

FARMER: All predicted waters are accounted for in the

11 Bins.

PETERS: Yeah.

BULLEN: So, there's a median and--

CRAIG: Joe, use your microphone. You've got to use a microphone.

FARMER: Sorry. All predicted waters are accounted for in the 11 Bins. There's a median, a maximum, and a minimum. And, if you look at a particular Bin, it encompasses many water compositions, but if you go from Bin 1 to 11, those 11 Bins account for all the predicted water compositions and there are quite a few of them.

BULLEN: Thanks.

PETERS: So back to the ternary diagrams. Here is the 11 Bins plotted up on the idealized ternary. Shown in green, italicized, are Bins 4, 9, and 11 which make up the majority in terms of relative frequency of occurrence. Also plotted on here in yellow circles are the solutions that are used for corrosion testing. Joe's going to talk more about that. I won't dwell on that, the point being that the kind of solutions that we're using for corrosion testing are really--they're bounding our problem. That's my take-home point from this.

Back up one second, please. What I'm going to now talk about is take these 11 Bins and then evolve them through evaporative concentration. How do they evolve, what do they produce? The table already gave you the answer, but we're going to show a few specific examples.

What about the model validation approach before I get to showing you a couple of examples of evaporative concentration calculations? The improvement in the in-drift model from ESR and the LA has been the incorporation of a Pitzer model for activity composition relations, in particular, that does a nice job of evaluating these more concentrated solutions. It's an improvement that probably means something to the geochemist types in the audience. But, it's a significant improvement going from a (inaudible) to a Pitzer type formulation. To demonstrate and to build confidence in our model, we validated it against some of your own laboratory data, as well as looking at published results and I'm going to walk through a couple examples of that in the next couple slides.

First, a set of calculations done by Garrels and Mackenzie. This is the evolution for the evaporation of Sierra Spring water. Plotted on the left is again the Garrels and Mackenzie calculation done with a different approach; on the right is our EQ3/6 simulations for the same starting water composition. So, it's concentration of various species also showing how pH tracks versus concentration factor as you evaporate the water. We dug into the details. It does do a nice job of showing that independent calculation techniques do give a very similar answer which builds confidence in our model.

Another example shows predicted solubility. Here, I'm looking at sodium chloride solubility as plotted in the CRC Handbook versus our predictions, a one to one line, as well as under prediction by a factor of 10, and over prediction by a factor of 10. It then it shows the actual data from the CRC Handbook showing that our model does respect that data, as well.

Next, some experiments that have been done by DOE for Yucca Mountain. These particular set of experiments were done by Nina Rosenberg and coworkers at Livermore. They took a representative Yucca Mountain pore water and it evaporated. These particular experiments were done at 85 degrees Celsius. They concentrated it. Two sets of data are shown here; one for a concentration factor of 1, one for a concentration factor of 1,243, again showing under prediction by a factor of 10, an over prediction by a factor of 10, and our EQ3/6 simulation of those experiments.

So, moving now to Bins 4, 9, and 11, the three that I pointed out were the most common in terms of occurrence in the crown, potential seepage. I'm going to show three plots, one for each Bin where I'm plotting either pH or molality versus relative humidity showing how different species track and also how pH varies as you evaporate these waters towards dryness in the model. Also shown on here are blue curves that show the relative amount of water remaining just as you go towards dryness in the three Bins. Also shown on the bottom is a reference back to the temperature-time curve, in particular the time, and at approximately at what times we expect the kind of relative humidities that are shown on the plot. A lot of information on these. A couple of take-home points; the pH in this particular, you can see, stays right around 7.5 to 8 and the relative amount of water, as one would expect, decreases until you get to complete dryness.

Next one, Bin 9, again one of the other common Bins. Here, pH is actually evolved as you evaporate up to the range of 10. Bo really emphasized and made some good points about what the pH is of the waters in the rock. It really fits within a fairly narrow range, 7 to 8. If you look at the 11 Bins as they evaporate in the drift as we evaporate potential seepage, the pH range is more on the order of 4 to 10.

CORRADINI: Again, a point of clarification?

PETERS: Uh-huh.

CORRADINI: I read this from right to left. Is that incorrect? Because your time marks are going from right to left.

PETERS: Right.

CORRADINI: Is that correct?

PETERS: Yeah. Yes, that's probably the right way to think about it in terms of this picture. Yeah.

CORRADINI: Okay.

CRAIG: How do you read the relative amount of water remaining throughout this?

PETERS: It's not really plotted on a scale. It's just trying to give a sense for relative change in the amount of water.

CRAIG: And, the amount of water increases with time if you read from right to left?

PETERS: Yeah, I'm sorry, that's confusing. We're reading it in this direction.

CRAIG: I think that's what--

PETERS: As you take a water and evaporate it down, you start with this much water, and when you get towards dryness, you've got a factor of 2 less.

CRAIG: Okay. But then--

FARMER: Excuse me. The terminal point corresponds, I think, to the relative humidity of the deliquescence point. So, you begin at 100 percent RH with a very dilute solution, and then as you evaporate it down, you approach the deliquescence point and the terminal point would correspond to the critical RH where you would have deliquescence.

CORRADINI: Can I try one more time? I caused this. So, I look at the red bar, the yellow bar, the blue bar. I interpret that to mean that the molalities--forget the water line--the molalities are actually going from right to left because you're essentially drying out and you're becoming more higher relative humidity. Is that incorrect? I'm just trying to understand your red, yellow, and--

PETERS: I understand what you're saying. If I had a drop of water at 2000 years, I'm basically ambient, right? So, I'd be here. If I had to draw off the water at the drift wall that I then introduced into the drift at, say, approximately 500 years, this is the conditions that it would be at. It would evaporate to that point. Clearer?

CORRADINI: Yeah.

PETERS: Joe, you want to take a cut at it?

CORRADINI: Yeah. Yeah, I think this shows the--I think to try to map time over on this curve is probably not the best thing to do though. There are scenarios where that would be appropriate. I think what this is showing you is that for a particular Bin water, in this particular case Bin 9, you can start out with a very dilute Bin 9 water which has an equilibrium RH close to 100 percent. Then, as you take that Bin 9 water that, frankly, is a fairly dilute solution, at least compared to what you eventually get to is you evaporate the dryness, and you start evaporating that Bin 9 water--let's say hypothetically on the surface of the waste package--this shows the concentration evolution that you get on the surface of the waste package as you evolve a Bin 9 type of water. So, as we go through these types of calculated or predicted evolutions with the EQ3/6 code, we see how the 11 characteristic Bin waters evolve on the surface of the waste package, as Mark said, as we evaporate them to dryness. So, you're going from a very dilute aqueous phase to something that approaches a deliquescent brine to something that at the terminal point is almost a solid. So, this shows the evolution of that solution. I think these calculations, I believe, we done actually in response to some of the earlier requests by the Board.

CERLING: Cerling, Board. So, what do the 2000 year point, 1000 year point, and 500 year point--

PETERS: It was attempting to try to tie--perhaps, Joe's correct, but it was attempting to try to tie the expected RH conditions in terms of time because--

FARMER: That's correct.

PETERS: That's what were trying to do was tie this back to the time axis here because the RH at early times is low in the drift; hence, that is--

FARMER: Yeah, the time points on the X axis were not used in any way in the prediction. Basically, during some of the preparatory work for this presentation, people were curious and--you know, there are certain RHs that occur in the repository at certain periods in time. For example, if you give me a certain point in time, I can look up on this curve that we show you on the poster the temperature and the corresponding relative humidity. So, there were questions during the dry runs pertaining to, well, when would you have a deliquescence or a particular type of solution occur on the waste package? So, those bars at the bottom correspond to--

CRAIG: Okay. Continue on, Mark. I'm reminded by Scott to ask people to speak into the microphone and identify yourselves, please.

PETERS: Okay, next one. I won't dwell on this. This is another one of horsetail plots, as we like to call them, for this time, Bin 11, showing the pH again up around 10.

Let's talk now about chloride-nitrate ratios. Again, we're still focused on the crown seepage waters, the potential waters that could seep. This particular calculation happens to be for one temperature and one Pco_2. I should have mentioned back earlier that we've done this at three temperatures and three Pco_2s, 40, 70, and 100 degrees C, and Pco_2s at 10^{-2}, 10^{-3}, and 10^{-4} in terms of partial pressure of CO_2. We've also done uncertainty analyses down to 10^{-5} in terms of partial pressure of CO_2. But, this shows nitrate/chloride ratio. That's a function of RH which is again--I'll call it a proxy--for evaporative concentration. That's my words. But, it shows how those Bins evolve in terms of nitrate/chloride ratio. This is particularly pertinent to what Joe's going to talk about in terms of the presence of nitrate and how that affects localized corrosion.

So, back to the ternary and again the three Bins that are the most frequent in terms of relative frequency occurrence; Bins 4, 9, and 11 showing how they evolve. Bin 4 shown in green, Bin 11 in the blue, and Bin 9 in the red showing where they start. This is really a graphical representation of what you saw on the table, where they start and how they evolve in terms of this idealized ternary. Why does Bin 4 start out in this idealized system in what looks like a calcium chloride brine field and evolves across the divide? That seems to violate thermal dynamics. It's because it's an idealized ternary. There's actually a slide in the backup that shows the evolution in terms of what's precipitating as you evaporate. It shows that there's a lot of other components driving why you're seeing that across that divide in this idealized system. So, we've taken potential seepage, developed the evaporation of that potential seepage and that results in a set of look-up tables as a function of RH temperature and all the species that then the model can then--TSPA can then go pull off of a look-up table and go in and say this is the environment as a function of time and temperature.

So, now, moving to deliquescence, this is a somewhat busy diagram just to try to help talk a bit about deliquescence. We've talked about the evaporation process. You get water dripping in, evaporate and precipitate salts. There's also dust potentially gathered on the surface of the metals. You evaporate that towards dryness. As you evaporate it towards dryness, you get again a set of precipitates gathered on the surface that then, as a function of RH, can lead to deliquescence or generation of liquid films on the surface of the metals. The different deliquescent minerals will produce aqueous or deliquescent films depending upon their particular properties and the relative humidity conditions in the drift. Chlorides will have a different deliquescence point in terms of relative humidity than a nitrate, for example. Then, you go through the deliquescence process. So, you can actually produce liquid films on the surface of the metal at actually fairly high temperatures and relatively low RHs depending upon what the composition of the phase is on the surface.

So, now, let's talk a little bit about deliquescence. This is first leading off with a simplified system, sodium chloride-sodium nitrate at a specific temperature, 90 Celsius, showing a phase diagram as calculated by EQ3/6 and also some measurements that we've done in the laboratory. This is leading off with trying to show that we are, in fact--we're validating our model, we have confidence in our deliquescence model, and we're doing similar types of calculations for other systems to gain confidence in our models.

Next, I talked about the fact that as you evaporate, you can build up deposits on the surface. That's an important component of deliquescence. That's accounted for within the deliquescence model in the in-drift chemistry model. What about dust? Dust from the surface can produce deliquescence. We've already said that. I've talked in past meetings about dust analyses that we've done, the U.S. Geological Survey has done. They've taken over 50 samples. They've done analyses of those dusts. Those dust compositions have been grouped into four likely categories in terms of their deliquescence behavior and then we modeled that with EQ3/6 to look at what the deliquescence RH values are for those four representative dust compositions. That's shown, I believe, in the next slide.

A graphical mistake on my part, these red squares should be up here on this line. I'm not sure what exactly happened. But, what this is showing is relative humidity this time on the Y versus temperature for the four representative dust Bins and how they evolve in terms of their deliquescence point as a function of temperature and relative humidity.

Next slide. So, we've accounted for the dust aspects of the problem in terms of deliquescence. Now, let's talk a little bit about just deliquescence, in general, and particularly, the introduction of an evaporative salts on the surface. This is the piece that I'm going to touch on and Joe is going to also carry forward as he talks more about the metal degradation piece of the story. The objective here is to characterize the films that may form on the package, detect if there's any associated corrosion, and then measure how much corrosion. We've done these experiments. These are Livermore experiments that have been done in a thermogravimetric analyzer where you look at weight change. You fix the relative humidity and temperature. In most of the experiments, we've applied actually a deposit of a chloride or a nitrate and seen how that evolved through the function of temperature and then we look at the samples after we've exposed them for a period of weeks.

Next slide, just a nice picture of the apparatus. I'm not going to dwell on this one. I think Joe will probably touch on it in more detail.

Next slide shows the results at three different temperatures. Joe's got a slide that, I think, focuses mainly on the 150 degrees C data. What you're seeing here is the results of one experiment where we've deposited calcium chloride on the surface of Alloy 22 at fairly low relative humidities and its weight change is a function of time. These experiments have since been run out on the order of weeks, but it's again showing weight change for three different temperatures. The 100 degrees C data shows that the calcium chloride is stable for the entire duration of the test. You go to the higher temperature data, 125 degrees C, you can see that the solution evolves slowly and you slowly form insoluble precipitates, whereas with the 150 C data, you form insoluble precipitates and then evolve acid gas which produces the weight change. When you look at the surface of the samples--and I'm not going to focus on this again; Joe is going to talk more about this--we see nor detect Alloy 22 degradation in the presence of these deliquescent brines. We have characterized what precipitates out as you heat the metal surface with the deposited calcium chloride on the surface and those properties have been analyzed in order to understand how they evolve as a function of temperature.

So, a quick slide on what sorts of things we saw on the surface of that metal. SEM analyses were done that indicated that precipitates contained some substantial amounts of calcium, chloride, and oxygen. There was a wet chemical analyses done and Raman spectroscopy done, as well, and the precipitates were in all likelihood a mixed calcium-hydroxide-chloride phase. But, this is the kind of characterization that we're doing on all the deliquescence experiments to understand what sorts of things evolve when you introduce, for example, a calcium chloride coating on the surface.

So, we've talked about potential incoming seepage waters, how they evolve by evaporation, deliquescence on evaporative minerals, deliquescence on dust, and what about condensate? You can see Bo alluded to the fact that we've seen evidence of condensation in the cross-drift. Perhaps out in this part of the evolution in the blue area down here when we're not even in the cooler area of the temperature-time evolution, you could see scenarios where you could get condensation on the metal surfaces of basically distilled water, but if there's dust on the surface, that could evolve to a brine. So, we've done a series--again, using the same dust analyses that I talked about when we were talking about deliquescence, we've done a series of calculations starting with those four--excuse me, starting with a representative set of dusts and evolving those through EQ3/6 and looked at how those evolved in terms of brines when you interact the condensate with the dust.

The next table, the next slide, shows the results of those brines. A similar kind of format to what I showed with the seepage water. Here, you've got six representative Bins, the relative frequency of occurrence of the Bins, let's see, where it starts in terms of its composition, and where it evolves to at its end point with its deliquescence point shown here in this column here, the end point RH. You see they evolve and they evolve to nitrate or nitrate-chloride or bicarbonate brines. These dust leachates again are all part of the story that goes into the evolution of the in-drift chemical environment that results in a set of look-up tables that's used by the total system model to sample composition of the in-drift environment as a function of time.

What about committed drift materials? I have one slide on that just to point out that, yes, there are committed drift materials. They're important to consider in the story. The majority of the materials are metals, no surprise. There's a lot of different elements that could affect the chemical environment. These are really not anticipated--they're not anticipated to significantly change the conclusions. We've done EQ3/6 simulations looking at the effect of some of these materials on the in-drift chemical environment and the waters are not expected to change significantly in terms of ionic strength. Also, given that the environment is an open system, there will be sufficient oxygen to oxidize the committed materials and I'll talk more about the open system aspect as I move to the next phase of the presentation.

So, let's talk a minute now--go ahead and go to the next one. Let's talk for a minute about the open system aspect of the drift or of Yucca Mountain first. Bo and I, for that matter, in previous meetings have talked extensively about observations in the cross-drift behind closed bulkheads. Bo has also presented results of comparing radon measurements versus what he would expect to see in terms of radon in the tunnel. We look at barometric pressure observations from surface-based boreholes from years back, as well as ongoing measurements of barometric pressure variations in the drift scale tests and other tests and you also do air permeability tests. All those indicate that it's a well-connected fracture network. These are my words, the mountain breathes. All those in-situ observations are consistent with the UZ flow model. And, finally, and I'm going to talk more about this, we are doing some modeling and also looking at the natural convection tests that were done at Atlas, and when you look at a closed drift segment in terms of what would go on inside a drift, you know, I've already been hammering home that the UZ is an open system. When you look at gas phase flow within the drift itself, you get significant mixing on fairly short time scales within the drift. And, I've actually got an animation that hopefully will hammer that home.

Next slide, just a slide that talks about the piece on radon and also air flow in the drifts. This bottom graphic here shows the radon measurements versus predictions and also the barometric pressure data. It's also important to point out all boreholes have intra-borehole gas flow.

We've just got a wealth of evidence that there's barometric pumping within the system and it's an open system at Yucca Mountain.

So, what about within the drift? We've developed a model to analyze heat and mass transfer. It's been developed at full scale to look at repository scale effects or drift scale effects within the repository. The simulation that I'm going to show you is actually going to be a quarter scale simulation that was used to do some test design calculations. But, it's using a CFD code FLUENT. It's a representative 14 waste package segment modeled over 70 meters. When you look at the preliminary results of those calculations, there's a significant component of axial transport and it's several orders of magnitude higher than molecular diffusion. You get significant mixing expected along the drift.

Can we try to run that movie? Don't start it yet, Denise, okay?

CORRADINI: What are the boundary conditions on the ends?

PETERS: Closed.

CORRADINI: So, hermetically sealed?

PETERS: Yeah. And, the wall is going to be closed, too. I didn't run the simulation. So, I know you're going to ask me a lot of very detailed, very good questions about how it was done and I'll have to to some extent probably say we can get you the answer because the PIs aren't here. But, what you've got here is you've got a representative--this is again a quarter scale calculation. These are each individual packages--let's call them packages--along the drift. And, what we're going to do here, the drip shields are in the model. This is similar to a calculation that's being done for the real repository scale. This is just a representative animation just to give you a feel for the kind of mixing that we're expecting. It's going to march through time and this is in seconds. So, it's going to introduce particles at this end and you're going to see them evolve along the drift and how they evolve as air flow as you mix in the drift. It's going to run all the way out to 600 seconds or 10 minutes. The particles are going to change color as they get older. Okay? So, you can run it to like--I think, we talked 595 or so. We're going to introduce particles here, you can see as they actually transport along the drift. You get effects from the cooler packages versus the--the hotter packages versus the cooler packages. There is just pulses of particles being added periodically. I believe, it's very two seconds at this end--every two minutes, pardon me, two seconds, I think. There's also interesting effects. If you look at a picture down the drift, you can see the effects of the drip shield. You get flow under the drip shield and flow over the drip shield.

PARIZEK: Can you point out the level to which it might occur?

PETERS: Where the--

PARIZEK: The drip shield top is right here.

PETERS: Right about there.

PARIZEK: Okay. No wonder it breathes, it's alive.

PETERS: And, this is for a close drift. So, you're getting actual transport just due to the temperature gradient within the drift.

NELSON: Nelson, Board. When you say closed drift, that means there's no--

SPEAKER: Mass loss.

PETERS: No mass loss.

NELSON: So, no air is entering through the rock?

PETERS: Right. For this simulation.

CRAIG: Do you have any idea what the oxygen content will be when it's above-boiling? Have you looked at that?

PETERS: What's the air mass fraction? Bo, are you here? What's the air mass fraction in the drift above-boiling for the repository? I don't recall.

BODVARSSON: (Inaudible).

PETERS: Yeah, I just don't--

BODVARSSON: (Inaudible).

PETERS: Yeah.

CRAIG: But, the mountain is breathing. So, oxygen could be coming in from the outside.   You're non--not in this--

PETERS: Not in this simulation.

CRAIG: Not in this simulation, but in real life.

PETERS: You can freeze it. Go ahead and freeze it, please?

What I want to focus on is the fact that you're introducing particles at this end and you can see--let's take, for example, these fairly old particles. They're spread throughout this segment of the drift. My point is is that you get significant mixing just within the drift not even taking into account the fact that the rock--you're also getting significant mixing from the unsaturated zone itself. That's my point. I think that's important when you start talking about can you sustain environments where you, say, distill a pore water down to where you generate very, very acidic acids. Can you sustain those kind of environments locally within the drift? I would argue this is a piece of the argument that says no way, even if you could generate them.

PARIZEK: Would that have happened if those were uniform temperature packages?

CRAIG: Speak into the microphone.

PARIZEK: The question I asked, whether or not it would have happened if they were uniform temperature packages?

PETERS: Just by straight diffusion. But, the transport--the diffusion (inaudible) scales, I wouldn't think--it wouldn't occur to this extent because you're getting acts of transport due to the fact that you've got different temperature packages. You'd get some diffusion, but it wouldn't be over that (inaudible) scale would be the way I'd answer it.

So, let's try the next slide. So, let's go back to the system evolution, the three regions. Just to reemphasize, you're got the orange area, the drift will be dry, you're above-boiling. There's little possibility that you'll get any seepage. The surface of the metals are expected to be dry. That's not to say that there couldn't be deliquescence, but no seepage. You've got an open system. Convection is turbulent inside the drift. You've got an open environment. Development of concentrated distillation of inorganic acids is very unlikely. Although deliquescence is possible, it's not expected to produce localized corrosion and the in-drift chemical environment is expected to be benign.

Next slide, moving into the transition region here right around the boiling point, the rock above the drift is cooling through the boiling point, moisture returns, seepage may enter the drift. You still get a capillary effect that diverts seepage. It's still likely to be much less than percolation flux. Bo when through that extensively. The relative humidity starts to go up and begins to approach 100 percent at the drift wall. You still have this same open environment inside the drift. Again, concentrated distillation of inorganic acids is considered very unlikely. Deliquescence can occur, but it not expected to produce localized corrosion. And, the in-drift chemical environment is expected to be benign.

Moving to low temperature, I won't dwell on this, a lot of the same observations. The bottom line is we expect the in-drift chemical environment to be benign.

Next slide--that's it.

CRAIG: Okay. Questions? Okay. Ron?

LATANISION: Latanision, Board. When you make the comment in your summary that deliquescence is likely to occur, but unlikely to lead to corrosion, are you speaking of unwelded sections of metal or are you thinking of all of the possible configurations that might be present in the package?

PETERS: I'm speaking at it in terms--Joe might answer this differently. But, I'm speaking at it in terms of what sorts of brines--what sorts of phases and brines one might produce at the surface and they tend to be nitrate. You don't tend to get chloride type deliquescence brines.

LATANISION: No, I understand that, but I think there is an indication that welds and thermally treated sections behave differently than--

PETERS: Right. And, I'm going to do just what Bo did to me. He's going to talk about the materials degradation piece. But, seriously, Ron, he's the guy to talk about that. I'm not the guy to talk to about it.

LATANISION: Okay, fair enough. Yeah.

BULLEN: Bullen, Board. Could we go to Slide 31, please?

PETERS: Yeah.

BULLEN: I was interested in the 55 samples taken by the USGS. Do you think that this is a representative sampling of all the dusts that are possible in the repository? I mean, how all encompassing is that?

PETERS: It's data that we have available. What it is is it's dust--and Zell will steer me where I veer off. It's taken from tunnel walls. So, it includes components introduced construction, water use associated with construction, and also dust introduced through the ventilation system. We're using the same kinds of materials, you know, conveyor belts. So, I would say, you know, I don't think we're doing anything dramatically different as we construct the--it's something we would be doing in the repository. So, I have no reason to say that it wouldn't be representative.

BULLEN: Okay. Bullen, Board, a follow on that. Would you expect a dust composition to change affected by the thermal pulse?

PETERS: Good question. I'd have to look at the--I mean, it's coming from rock dust primarily. The stuff that's coming from rock dust, I wouldn't expect to because it's well-within its stability range.

BULLEN: Bullen, Board. But, if I have evaporated and concentrated salts somewhere and now I have potentially calcium chloride and now I have moving air or convective--

PETERS: Right.

BULLEN: Can I move dust that might not be the same 55 samples that were taken here and--essentially, since you showed us the nice animation, you could entrain that dust and move it just about anywhere.

PETERS: Right.

BULLEN: I would assume, right?

PETERS: So, you're thinking of a scenario where you have dust sitting on the surface somewhere at a given temperature, you move it somewhere else where it's out of its stability range, and it changes face?

BULLEN: It could be that or it could be made of dust that's not the same 55 samples that you have here. That would be a more aggressive dust species because I've concentrated it in some other form and then I move it.

FARMER: Actually, in regard to the deliquescence measurements, we have tried--we have done deliquescence measurements with mixed salts, but we've also done single component salts. If you'll think back about the ternary, we've actually been doing deliquescence measurements at the apex of that triangle which is the worst case scenario. At least, worst case in that we have a divalent cation that can undergo hydrolysis reactions.

PETERS: The other answer I would do is I would go dig into the output that Woolery got when he did the deliquescence or the dust leachate calculations as a function of temperature because if there is any--I would bet that we've bracketed the potential phases that could occur, but I would need to defer to Woolery on that.

BULLEN: Bullen, Board. Well, my thought process is that you mentioned that you couldn't make Bins 1 and 2 water, but I could still make the salts from Bins 1 and 2 water on the side or on the bottom of the drift and I can move them?

PETERS: Well, that gets back--I'm struggling with how you do that. You're saying they--how do they--they come into the drift?

BULLEN: Well, actually, can I take the waters from Bins 1 and 2 and concentrate them to make dust that would include calcium chloride?

PETERS: And, that's what Joe's point is is those are part of the program. We're looking at those worst case deliquescence brines in our program. He's been--

FARMER: One thing we're finding--

CRAIG: Joe, could you identify yourself when you speak just to make life a little easier for the readers, please?

FARMER: Yes, sir, my apologies. My name is Joe Farmer. I'm from Livermore Lab. So, one thing about the calcium chloride dust that might evolve, we have looking at deliquescence and corrosion underneath those deliquescent films. One general observation we make is that corrosion underneath a calcium chloride deliquescent film at very high temperature, 150 degrees Centigrade, close to the deliquescent point, is not the same as aqueous phase corrosion below the boiling point of a much more dilute calcium chloride solution. And, in fact, the more dilute solution can be more aggressive. So, I think we also have to be careful not to overly generalize, you know, what aqueous solution is. We talk about deliquescent brines as if they're your standard aqueous electrolyte, but there are differences in terms of transport, kinetics, and many other things that go on in these solutions. Much more complicated than that.

BULLEN: Bullen, Board. One last question and that deals with drift degradation. I mean, there's another opportunity for movement of dust and that's if things fall from the ceiling. I'm assuming that everything doesn't stay in place forever. So, have you considered that during your evaluation of the deposition of dust, I guess, is the question?

PETERS: In terms of the kind on of compositions that would be introduced?

BULLEN: Yeah?

PETERS: That's rock dust.

BULLEN: Bullen, Board. It's rock dust if it's not modified by the thermal pulse, right?

PETERS: Oh, we already--I thought we went through that, but to me, that's rock dust.

CRAIG: Dave?

DUQUETTE: Yeah. Duquette, Board. Could you go to Slide 26, please? This was one that there was a lot of questions about early-on. And, I'm just a dumb metallurgist and need to understand the chemistry a little bit.

PETERS: Okay.

DUQUETTE: Could I treat this almost as a symmetry type situation forgetting for the time being the bars on the bottom where initially you're starting out with a dilute solution, and as you get evaporation because of increasing temperature when you're first emplacing all of the things, you're increasing the concentration of the salts?

PETERS: Right.

DUQUETTE: Okay. And then, as a function of time after some long period of time, water comes back into the vault and dilutes the salts again, is that correct?

FARMER: I think that's a perfect interpretation.

PETERS: Yes.

DUQUETTE: Okay. I want to understand. Now, if you go to the next slide which is Slide 27, what that would imply is that as--I mean, I think of nitrate as being an inhibitor for localized corrosion in many cases. So, what that means is after long periods of time, the nitrate-chloride concentration would decrease. So, your inhibitor to aggressive ion situation would decrease as a function of time, is that also correct? Going from right to left now.

PETERS: Yeah, but I'm going to--we're going from right to left, right.

DUQUETTE: Yeah, yeah, because--

PETERS: You clarify as I go along if you need to. I'm still going to want to try to tie this--this is relatively high temperature here.

DUQUETTE: Correct.

PETERS: You're at low RH, relatively low RH. So, you're--

DUQUETTE: Correct. And, as I started to seep water back into the--and I start to dilute the salts again, I'm going to flip the balance between nitrate and chloride to be a little bit aggressive. In my case, we're going to make the solution more aggressive.

PETERS: Right, but I'm also going to be moving down here where I'm not as concerned about localized corrosion. That's the key point here.

DUQUETTE: No, no, I understand that, but the fact of the matter is that we're making the assumption that your time base models are correct and that you're not off by an order of magnitude.

FARMER: I think that you're right on the right path and Mark is, I think, leading us in the right direction for the answer. It turns out that the highest temperature where we're above the critical temperature for localized corrosion, let's say, crevice corrosion, we actually have an abundance of nitrate inhibitor. So, as you'll see in the next presentation, we have enough nitrate inhibitor so I think that it protects our waste package quite well.

As we lower the temperature in the repository and we start having a lower nitrate to chloride ratio, less inhibitor per a number of aggressive ions, we are transitioning into the temperature regime of below which we can have localized attack even in the absence of inhibitor; let's say, pure calcium chloride.

DUQUETTE: Thank you. That's where I was heading with that argument. Thanks.

CORRADINI: So, to go back to your--I don't know which slide it is, but to go back to how you link up with what Bo and the previous speakers--Corradini, I'm sorry.

PETERS: The great flow chart?

CORRADINI: Yeah. What is the handoff? I should have asked it at the point. Is it a time handoff, is it a temperature handoff? What exactly is the connection between them? What is the logic in the analysis?

PETERS: Yeah, let me get back to the slide. It will be easier.

CORRADINI: Okay. Sorry, I don't know the number.

PETERS: 10 or 11?

CORRADINI: It's the one with your boxes.

PETERS: Evolution--let's go back, Denise, scroll back.

CORRADINI: It's 14.

PETERS: Okay, thank you. Bo talked about up to here.

CORRADINI: Right.

PETERS: He's got a THC model for all the grid blocks as a function of time, water compositions within those grid blocks as a function of time.

CORRADINI: Uh-huh.

PETERS: Those results are looked at in terms of their spatial--spatially, let's use that word, and when I talk about crown waters, I focus in on just the waters that could potentially seep.

CORRADINI: And, what is done so that that--that gets to my question. So, you've explained very well the chemistry of what could get in and what could have deliquescence. How do they get in? What is the assumption right now in the analysis of how they get in? Why I ask, you keep on--

PETERS: It's tied back to the seepage model in terms of volume and amount and all that.

CORRADINI: So, some fraction given the total infiltration or flow path?

PETERS: Right, right.

CORRADINI: Some fraction of that goes in?

PETERS: Right.

CORRADINI: Okay.

PETERS: And so, we look at it spatially first because there could be grid blocks way over here that don't map for our purposes.

CORRADINI: So, again for analysis just so I can think through the story, at what point is seepage disallowed in this thinking process? All the way through--I'm going to use your purple band over there--all the way through 1,000 years because it's above 100 C?

PETERS: We will allow--I'm evaluating potential waters even if Bo says they won't seep.

CORRADINI: Right. Right. So, I'm asking, the connection there is when are they allowed to seep from an analysis standpoint in the current thinking?

PETERS: That's probably more his answer. When are they allowed to seep?

BODVARSSON: Hello, Bo Bodvarsson. The framework we are putting together here gives all the components to total system performance assessment for their evaluation. Our results indicate that there will be no seepage during the thermal period when there is boiling at the crown of the drift.

CORRADINI: Okay.

BODVARSSON: So, assuming that that is probably going to be used in TSPA perhaps with some modification to be a little bit more conservative in some cases because we have to discuss which to use in the total system performance assessment, but I expect it to be some version that has almost zero probability for seepage during the thermal period.

CORRADINI: So then, my question--don't go anywhere because I still don't understand the connection. I want to understand the story of how the water goes. Does that exactly occur for what's underneath so that is if there is no water to get there, there's no water to transport the radioactivity away. Correct? Is that a consistent assumption in the current analysis? Because the thing that was said in a sentence by you that struck me was if I can't get the water there, I have no mechanism to diffuse the radioactivity away from the package even if it's failed.

Have I misunderstood?

PETERS: No, that's what Bo said.

CORRADINI: That's what I thought I heard you say.

PETERS: In his presentation?

CORRADINI: Yeah. Do I have that? I want to make sure I'm clear.

BODVARSSON: No, you are absolutely clear on the following. What I say is this. If there's no seepage into the drift and there's no water coming into the drift and if the waste package fails for some reason, then the only way to transport the waste is way of diffusion because the diffusion does not require advection to happen. Diffusion can happen through the (inaudible) and into the rock. It is an extremely slow process that takes thousands and thousands and thousands of years. So, in a sense, you are absolutely right. If there's no seepage coming in, the waste is probably going to sit there for a long, long, long time.

CORRADINI: Okay.

BODVARSSON: Does that--

CORRADINI: I think I've got it. Thank you.

CRAIG: Okay. Dave Diodato?

DIODATO: Defer to Dr. Nelson first and then I'll go after.

CRAIG: Oh, excuse me. Oh, go ahead, Dave. We'll make Priscilla wait.

NELSON: Go ahead.

DIODATO: Oh, okay, thanks.

CRAIG: You were in line first.

DIODATO: Diodato, Staff, I'm sorry. Mark, on your Slide 32, you show the dusts that were analyzed and, I guess, my understanding is the same as Dr. Duquette's in terms of the nitrate being a corrosion inhibitor, right?

PETERS: Yes.

DIODATO: So, it's pretty fortuitous that you either have sodium-nitrate or potassium-nitrate or calcium nitrate in every one of these samples from inside the mountain. And, what I'm wondering is would that also be the case from exogenous dusts that were saved from Forty Mile Wash or somewhere else on the mountain? Do all dusts have nitrate compounds in them? Is that the case?

PETERS: The dusts that we--I don't know. Zell, we haven't analyzed Forty Mile Wash dust. We haven't gone out and analyzed Forty Mile Wash dust? Yeah, come on up.

DIODATO: Geologically, I mean, you might be able to intuit something.

PETERS: Yeah, but that could be theoretically airborne--part of the airborne component that comes in is the answer. I guess, I'm a little hung up on what you said by fortuitously it all ends up being--

DIODATO: Well, because it's corrosion--it's there and--

PETERS: And, that's a good thing.

DIODATO: Yeah, it's fortuitous.

PETERS: I don't know if it's fortuitous. That's what the data tells us, I guess. Okay.

PETERMAN: Zell Peterman, USGS. That's the other part of the equation which we don't have a good handle on yet is the ambient atmospheric dust at Yucca Mountain. And, I think the thinking is that in the long-term, you know, after the repository is loaded and hundreds of years down the road, it's going to be that atmospheric dust that may get into the repository. Now, here are dust collectors in Nevada. There's one over by Red Rocks and the composition has been used there, but it's probably dominated by carbonates in the (inaudible).

And, just one other comment, Scott Tyler at DRI has looked at soil compositions and he's got a paper out and the desert soils are amazingly high in nitrate. So, I would expect that dust from Forty Mile Wash or any other soil in the vicinity of Yucca Mountain to be similar to that. His work is over in Frenchman Flat, but it's just remarkably high in nitrate.

DIODATO: Okay, thanks.

CRAIG: Priscilla and then Carl.

NELSON: Nelson, Board. Three fast questions. What kind of bolts are you using?

PETERS: I'm not sure exactly. I think, they're split sets.

NELSON: Okay.

PETERS: No grout.

NELSON: Okay. Have you done any tests of the rock on thermal cycling, just the rock like to see whether it spalls and deteriorates?

PETERS: You're familiar with the plate--we've done the plate loading tests, three of them, in the ESF here in the last year or so where we drove the rock to failure if that's--

NELSON: No, I--

PETERS: And, one of those was done at elevated temperatures.

NELSON: No, I just mean like an index test to identify whether the lithophysal rock deteriorates on thermal and humidity cycling?

PETERS: At a drift scale, have we done that kind of test? We've done laboratory experiments looking at, you know, strength as a function of strain rate. You're familiar with all that stuff that the Board's been--

NELSON: Well, I'm not thinking about slaking, per se, but some sense of rock material deterioration on thermal cycling.

PETERS: Well, we've got empirical observations from the drift scale test.

NELSON: Which really haven't been through thermal--see, I'm just wondering about--

PETERS: They've simply taken out--

NELSON: --and whether a thermal cycle causes deterioration in the rock.

PETERS: Well, we've done--I mean, I don't know what else to say. We've done lab experiments at elevated temperatures, cycled varying strain rates. We've got empirical observations from the drift scale test on what a drift does as you heat it up and now cool it.

NELSON: Well, the drift scale test is not in the lithophysal rock. I'm just looking for--I mean, if you put it under stress and heat, then you're going to have a combination of drivers.

PETERS: Right.

NELSON: I'm just wondering about thermal cycle? When you take a rock, heat it up, dry it out, re-saturate it, does it deteriorate the rock? Just curious.

PETERS: Go ahead, Bill?

BOYLE: Bill Boyle, DOE. You're testing my memory. I think we have insights into this as a result of preparing specimens for testing. The rocks at Yucca Mountain are partially saturated with some varying degree of saturation which introduces uncertainty in the results. What effect did the saturation have on whatever property it is we were attempting to measure. So, in many of our testing programs for years, we decided to test it to end-member states, 100 percent saturation and 0 percent saturation. And, the method used commonly to get the 0 percent saturation was heating. So, we would heat the rocks to drive out the water until we got them bone dry, if you will, and then we would test them. And so, I know that we--and then, they would be allowed to cool and then we would go test them for whatever property we were interesting in, whether it was thermal conductivity or whatever. Off the top of my head, I'm not aware of any gross difference between the results of specimens that went through this thermal cycle versus those that didn't. There's an obvious difference between saturation and non-saturation, though.

NELSON: Nelson, Board. I remember asking this question about two of three years ago. Just simply, does the rock deteriorate on thermal cycling? Not associated with stress, just thermal cycling and re-saturation. And, it seems to me that this is a question that could be of interest because that's the mechanism that may indeed make dust whether you have a stress situation on the outside or not. It would seem that you could evaluate this fairly easily.

PETERS: Okay.

NELSON: So, that's why I ask it. Okay. Finally, tunnels are really interesting environments because people work in them and live in them while they're under construction. I'm wondering when you took the dust samples if you looked for any evidence of Sally's bugs?

PETERS: They measure again a carbon in the dust. We've got--they didn't do a microbial analysis in terms of what kind of microbes occurred in the dust, I think, is the direct answer to your question.

NELSON: Are there any thoughts or plans to see if there are microbes in the dust?

PETERS: As of right now--go ahead, Zell? Zell has raised his hand. As he's walking up, as you know, we've got bugs growing in the cross-drift, and that, we've characterized extensively. That was growing on, what I'll call, introduced materials.

NELSON: Yes.

PETERMAN: Zell Peterman, USGS, again. We sent three samples to AECL of Pinawah to the biologist up there and she cultured them and they grew very nice molds and some sort of penicillin type, the same sort of thing that's growing in the cross-drift. So, yeah, there are bacteria in the dust.

NELSON: Nelson, Board. Is there any possibility of having such microbial activity involved in any of the corrosion?

PETERS: Yes is the answer. As soon as we observed what ended up being penicillin in the cross-drift, the person who we had come down and take the samples and to the characterization was Joann Horn who is the PI for the MIC work at Livermore. So, that's an ongoing program and she's brought that into her thinking as she thinks about MIC.

NELSON: And, that will eventually be brought into the story that's told about--

PETERS: That's another part--yeah, I mean, if you wrote--you couldn't theoretically write the coupled processes as where's the M and where's the B, where's the mechanical and where's the biological.

NELSON: No, I just--because so many people with engineering training will first seek to understand things physically and then chemically if they have to. The last thing is biologically.

CRAIG: Okay. The last question is Carl Di Bella.

DI BELLA: Thank you. This is again about Slide 32, Mark, your dust--I should have--

PETERS: May I shouldn't have put that one in there, huh?

DI BELLA: I hadn't seen it before. First of all, just let me remark in preference to my question. I'm sort of surprised by these extremely, to me, low-looking deliquescence points at higher temperatures for things like sodium-chloride, sodium-nitrate mixtures. And, I assume that you calculated this by a model and don't really have any experimental confirmation for the high temperature results. But, my question really has to do with how you model these because my recollection from looking at Zell's paper given at the Highlands Waste meeting was that the heated elemental analysis on the dust, he didn't do a salt analysis on the dust and I hope he comes up and confirms or says it wrong. So, it would seem to me for your modeling, you would have to sort of assume that is dissolved in water and then evaporated to dryness and see what sort of chemical divide kind of results you get and what sort of deliquescence comes out of that. But, that's not exactly what's going to happen. If you get deliquescence, it's going to depend on the salts that are there already. They're already dry in the dust.

PETERS: Right.

DI BELLA: And, the things that deliquesce first are going to be your lowest deliquescence point salts which are going to be magnesium-chlorides if there are magnesium-chlorides present. I know magnesium there, I know chlorine is there. I don't know if it's there as magnesium-chloride. So are early calcium chloride or mixtures of the same. So, can you answer my quest--well, my question is this. Did Zell analyze the salts or did he just analyze the elemental composition of the dust?

PETERS: You don't need to walk up. You just analyzed the elemental compensation of the dust?

PETERMAN: We did both.

PETERS: Okay. I'll repeat what he says.

PETERMAN: (Inaudible).

PETERS: Okay. He looked at both the insoluble and soluble fraction, but in terms of the phase--I think, in a way, Carl, I know what you're saying. You're saying the phases that occur in the dust and how that goes through the deliquescence process. Yet, you answer the question the way it was modeled as best I can tell. You take the composition, it's got these components, and you put it through EQ3/6. You don't assume certain phases to start with. You put that compositional space through EQ3/6 and it evolves to that.

DI BELLA: Right.

PETERS: So, is it mechanistically the same as what you'd expect? No. Is it telling you the same chemistry? I don't see why not.

DI BELLA: If you have a dry salt, what's going to deliquesce first is going to be the lowest deliquescent point material as you come down from any thermal peak.

PETERS: That's a good point. And, there may be details within the model that I'm just not prepared to speak to.

CRAIG: Okay. Time to hold that one until coffee break or something or later on. Mark, thank you very, very much.

Joe Farmer has appeared so often here that I'm only going to say welcome, Joe, and I'll give you a bell when you've got 10 minutes to go. How many total minutes? Total minutes is 50, I think. Yeah, 50.

FARMER: Well, during this part of the presentation, I'm going to concentrate on telling you about the--

CRAIG: The microphone is not working, Joe.

SPEAKER: Maybe it's not turned on, Joe.

FARMER: Can you hear me now?

SPEAKER: No.

CRAIG: Okay, it took a while to activate. Okay.

FARMER: At any rate, in this part of the presentation, I would like to tell you about what we know in regard to materials performance. And, more importantly, and unlike perhaps some of our earlier presentations to you, I would like to cast what we know about materials performance in regard to our integrated strategy. And, as Bill told you this morning and then followed up by both Bo and Mark's presentations, we have worked very hard since the last Board presentation to try to pull together an integrated story. And, this is very crucial for the materials strategy because, obviously, as we discussed at the last Board meeting, given a sufficiently aggressive environment, you can destroy virtually any engineered material. So, it's very important that we have a realistic idea of what our environments are.

As Mark and Bo have both told you, we can subdivide the operation of the repository into three general temperature regimes. There is the dry-out region which is a relatively high temperature region and generically I would--based on Bo's presentation, I would characterize the dry-out region as that region line above the boiling point in the repository. And, we have very specific sets of mechanisms that are operable in regard to materials degradation in the dry-out region. We heat-up through the dry-out region and then we cool down through the dry-out region. During the initial phase of operation of the repository, we have two mechanisms drying out the walls of the drifts. We have the ventilation which will dry the drift walls even without the presence of heat. We have radioactive decay. The radioactive decay will tend to further dry-out the drift wall. Once we reach a peak temperature and the waste packages being to cool, we eventually pass through the deliquescence point. And, now, as you see from Mark's presentation, as we pass through that deliquescence point, we can use those evaporative concentration curves and use RH as a look-up parameter and now go in and actually assess the local environments on the waste package surface. So, we pass first through the deliquescence region and that deliquescence actually occurs in the dry-out region of operation.

Eventually, we reach a point where we cool below the boiling point. As we saw from Bo's presentation, as we cool below the boiling point, we have the possibility of seepage brine entering the drifts. Now, we don't believe that's a very large probability or that there's going to be much seepage, but it is possible. So, at this particular point, we have to start taking into account actual aqueous phase corrosion. And, I will differentiate that from the deliquescence type corrosion because, frankly, the performance of the material that we observe in deliquescent brine versus aqueous electrolyte is very different.

We continue to cool the waste package surface, and at this particular point--and as we present the poster, we haven't really committed. We're not telling you if the threshold temperature for localized corrosion overlaps with the dry-out or if it's in the transition zone or perhaps slightly below. But, at some point, we eventually pass through the threshold temperature for localized corrosion, crevice corrosion most probably in regard to the performance of Alloy 22. And, below that threshold temperature, frankly, it doesn't matter what the water chemistry is because at that particular point in time, if we establish the critical temperature for localized corrosion in a worst case brine, such as calcium chloride, we realize that at any temperature below that threshold, the waste package material will protect us against that water chemistries of that nature or perhaps less aggressive. So, at that point, we become relatively insensitive to water chemistry. So, as we look at collecting data, modeling the waste package, and assessing the performance of the overall system, we try to cast it in regard to these three temperature regions of operation; dry-out, the transition zone, and this blue region on the poster where we actually have protection by Alloy 22 even in worst case conditions.

So, Denise, if I could have the next slide? This, frankly, summarizes the general attributes of the repository as we understand it. We believe now that the waste package is protected by different mechanisms in the three temperature regimes as I just discussed in regard to the poster. I think most importantly, as we look at this very carefully, I think we also realize that the dry-out region provides an additional barrier for the waste and additional protection for the waste package material. So, frankly, as we look at this, we actually see a very beneficial effect of having this high temperature zone of operation. Recalling some of the historical rationale behind having the high temperature mode of operation, this was one of the initial views by the thermohydrologists who promoted the high temperature mode. We also believe that the project's overall strategy is consistent with conceptual models being developed by other experts in the field. In particular, a presentation recently given by Professor Payer from Case Western received, I think, very high marks at the AC&W meeting.

So, in the next slide, I'd like to borrow from Joe's presentation. Joe represents a different metallurgical conditions by these ellipses, and frankly, my chart is not quite as good as Joe's. So, I will show you Joe's next. But, I think it's a very nice convention. He, in essence, gauges the closeness of an environment to cause corrosion by the closeness of these ellipses. This ellipse represents a range of metallurgical conditions that might allow a material to be susceptible to growths of attack and the environment ellipses represent a range of environmental conditions that might actually cause corrosion of waste package material. And, the closeness of these two ellipses, in essence, is a gauge of how close you are to having attack. So, as we try to apply this general graphical convention to the strategy that we've laid out to the poster, we believe that during the ventilation and initial heat-up, we have essentially dry conditions and there's very little corrosion of the waste package material.

As we enter the very hot regions of dry-out, we believe that we've pushed the conditions that cause corrosion further away from the metallurgical condition for corrosion and have any less chance for attack. The primary regions that we're worried about are as we pass during cool-down below 150 degrees Centigrade which corresponds to the deliquescence point of calcium chloride or perhaps the boiling point because here we can have the existence of aqueous phases on the waste package surface and the possibility for a localized attack.

Next slide, please, Denise? So, in this particular slide, I just wanted for accuracy to show you Professor Payer's original rendition. I may have altered the interpretation slightly, but this is certainly what we have gotten out of his graphical representation, and frankly, it was very helpful for us in terms of trying to integrate our strategy over the last few months.

Next slide? As we've said at previous meetings and some points are worth making again and again, we have actually picked a very, very good material. I think as I speak to you today, I would like to point out that our materials selection has, indeed, been reviewed by many international panels. And, through all these reviews, people have recommended, of course, that we collect more data, perhaps that we do measurements in a different way, but no one has really recommended a better material. And, frankly, the reason for this is we've picked an incredibly robust engineering material.

Next slide? As we have formulated our models, we've tried to formulate predictive models that could use parameters that are easily measured in the laboratory. It's very easy to formulate a theoretical model where the parameters are so difficult to measure that the model is virtually worthless.

So, as we formulated our model for localized corrosion, we decided to quantify the propensity for localized attack in terms of the Delta E value. This is the potential difference between the open circuit corrosion potential, that equilibrium potential that the metal surface tends to reside at unperturbed, and the potential where the passive film breaks down.

As we look at different types of materials that we might use for construction of a waste package and, in particular, looking at the 300 Series stainless steels and the nickel-based alloys that we've considered in this program, a general rule of thumb is we observed that as we increased the overall concentration of chrome, molybdenum, and tungsten in these alloys, we tend to push the threshold potential which can be quantified either as the breakdown potential of the passive film or the potential at which that depassivated surface repassivates. We tend to push that Delta E value to higher and higher levels. And, of course, I also make the point that there are materials out there that have a higher threshold potential for breaking down the passive film, for example, the titanium alloys such as we're using for construction of the drip shield. Frankly, the reason that the titanium material wasn't used for construction of a waste package is this opens up a whole new can of worms. The nickel-based alloys are not nearly as susceptible to hydrogen embrittlement as is the titanium-based alloys. It turns out that if you get into the literature, you find that there's a lot more known and published about a mechanism such as stress corrosion cracking than there is o fa hydrogen embrittlement of titanium alloys. And, from some of our early measurements, we also understood that there was a lot of hydrogen absorption in titanium crevices. So, we felt that overall the selection of these nickel-based alloys with lots of chrome, molybdenum, and tungsten to push the breakdown potential of the high values was a prudent choice.

Next slide? Here are some real data collected a while back, but still quite relevant. This shows the breakdown of the passive film of 316L, one of our earlier candidates for the waste package in simulated saturated water. And, as you'll see later in this presentation, this SSW electrolyte at the boiling point is, in fact, a realistic environment to do testing in. So, you can see that in the particular case of this material, you have breakdown of the passive film, pit initiation, very open to the open circuit corrosion potential. So, clearly, with a material like this, a little bit of gamma radiolysis and other effects could push you over the edge, so to speak. So, this is why we didn't choose the 316.

Next slide? If we take Alloy 22 and this very concentrated brine solution at 120 degrees Centigrade which turns out to be the boiling point of this near saturation salts solution, we see that we have to push the potential to a very high level and, actually, we never break down the passive film. We begin to evolve oxygen on the surface of the material, but we never break down the passive film because we have, as you can see, this negative going hysteresis loop as we reverse the potential scan. So, in this realistic repository water, SSW, we have a very good resistance to localized attack.

Yes?

CORRADINI: Can you walk us through the arrows for the uninitiated?

FARMER: Sure. Normally, in these potentiodynamic tests, one begins at the open circuit corrosion potential and the test that we have done historically, we normally let a sample sit in the solution for perhaps 24 hours. Then, we initiate a potential scan that scans are sufficiently slow so that most people practicing the art believe that at any particular potential level, you've achieved equilibrium. Normally, you're operating at perhaps tenths of a millivolt per second, very slow scan rates. And so, one of these curves takes, frankly, quite a long time to generate.

As you ramp the potential in the anodic direction, you eventually reach a level where a number of things can happen. One thing that can happen, of course, is that you can break down the passive film and getting anodic dissolution of the underlying metal surface. And, of course, that's a very undesirable effect. Another thing that can occur, as in this particular case, is one can start to evolve oxygen on this anode surface which is what you're observing here and then you eventually reach a very high level, here 1.2 volts. This is just about the thermodynamic viscous of water and you can reverse the potential scan. If I have broken down the passive film during this forward going potential scan, I would observe a positive hysteresis loop because I will be passing much more current on the way down than I observed on the way up. However, is there no breakdown at the passive film, I see what is generally a negative going hysteresis loop as we observe here.

And, I might also point out, we don't just rely on these potentiodynamic curves because, as many of the corrosion sciences will tell you, many people regard reading polarization data alone very much like reading tea leaves. So, we certainly don't do this. Every curve that we collect is collected by, in this, traceable standards. We gave these all catalogued. They're in the project's database, and going with this, we also have archival samples, for example. If you come to Livermore, we can actually show you DEA 033. We can also show you all the macro photographs to go with that, SEM, and other data to compliment this.

So, the point being Alloy 22, frankly, is a very good material selection.

Next slide? Now, today, I'm going to concentrate on telling you about the measurements of corrosion breakdown and repassivation potential as we use those measurements to understand these three temperature regimes and the resistance of the waste package degradation especially in the transition region and the cool-down region. But, as I discuss these measurements with you, I think it's important to realize that this is not the whole story. For example, just a few minutes ago, we were discussing microbial growth and the impacts of microbial growth. We've had a microbial corrosion program at Livermore for almost a decade now and, frankly, it's probably one of the two best microbial corrosion programs in the United States, the Army having the other one. So, we're very proud of the work that Joann Horn is doing and we have, in fact, assessed the corrosion performance of Alloy 22 in many of our waste package materials in a variety of microbial solutions. We normally run these against sterile standards to make sure that we have a good basis of comparison. But, today, I'm going to concentrate on sharing with you some of our measurements of corrosion breakdown and repassivation as it pertains to the expected waste package surface environment.

Next slide, please? As we've told you before, we have a number of tests systems at the project's corrosion laboratories in Livermore. We have long-term corrosion test tanks. These have processed now over 15,000 samples, all of this traceable. Contrary to what's been said in some past meetings, these environments as you saw from Mark's presentation are, in fact, relevant. These are all brines that form in the bicarbonate region of the ternary. So, we would characterize these as being representative brines. We also quantify stress corrosion cracking and use banks of traceable potentiostats that do these measurements of breakdown potential.

Next slide, please? As I walk you through my presentation, first, I would like to discuss with you the formation of deliquescent brines and the types of degradation of the waste package material that we've seen in these deliquescent brines.

Next slide, please? As Mark showed you, as we do the deliquescent studies either looking at the process of deliquescence from different types of salt deposits or as we look at the degradation of the waste package materials underneath those deliquescent brines, we use the environmental thermogravimetric analyzer built by Greg Gdowski at Livermore. And, Greg has done a very nice job of building this apparatus. It enables us to be sensitive to weight changes as small as tenths of micrograms. So, this is a very high sensitivity TGA even by TGA standards. And, it allows us to operate up to temperatures of 150 degrees Centigrade which is particularly important in assessing the deliquescence of the calcium chloride brines.

Next slide? Here are some data taken from the TGA. We've shown you other data like this in the past, but you see the initial absorption of water. The initial absorption of water in the deliquescence process is followed by the thermal disproportionation of that deliquescent brine with the liberation of, in this particular case, hydrogen-chloride and eventually we reach a nice stable regime and the laser has died. But, we eventually reach this nice stable region where we have no further deliquescence and also no additional corrosion or attack of the waste package surface. So, we have very good quantification on the processes that occur during the deliquescence.

Next slide, please? Here, you see panels that the header reads temperatures above 110 degrees Centigrade. We're actually tying this into the dry-out region of the poster. But, the temperature at which these data were collected, it was 150 degrees Centigrade, just about the maximum temperature that you would be concerned about with the calcium chloride salt. Greg intentionally coated the samples with dry calcium chloride salt. We exposed these to different humidities until we get the formation of a deliquescence brine. And, in essence, the take-away point from this particular slide is that with the lesser materials, such as Alloy 825 and Alloy 825 was one of our early candidates, we do, in fact, see a localized attack in these deliquescent type solutions. However, if you look at Alloy 6--I shouldn't say--I started to say the trade name. But, if you look at Alloy 22, you see that it has very, very good corrosion resistance underneath these deliquescent brines. These white spots that you see are actually the calcium-hydroxy-chloride that Mark mentioned to you before. We've determined that through a number of analyses including a Raman and elemental analysis.

Next slide? And, here are some SEM analyses, just a repeat of one that Mark showed you showing that we have actually gone in and imaged these white deposits to make sure that they are white deposits and not corrosion products and that is, indeed, the case.

Next slide, please?

LATANISION: Joe, what was the--

FARMER: Sure, Ron?

LATANISION: Latanision, Board. Just out of curiosity, what was the time--the length of duration of the tests in the previous--

FARMER: Most of Greg's experiments range from several weeks, typically three weeks to several months. That might be a couple or three months. And, in this particular panel, I would have to look back at the database to be certain, but I think that was something on the order of a month that I just showed you.

So, again, I think the take-away point in regard to the deliquescent brine story and corrosion of a waste package is that if you're up in this dry-out region, you can have the formation of deliquescent brines, but from studies like this, we know that those deliquescent brines are not going to have any significant impact on the corrosion of the waste package. So, I think that's the important point that we're trying to make with the deliquescent brine studies.

LATANISION: Latanision, Board.

FARMER: Yes?

LATANISION: May I ask my question again about whether welds make a difference in this discussion?

FARMER: Welds probably do make a slight difference. We are looking at age samples. You've been there to Livermore, I know, and we have the long-term thermal aging facility. So, we can simulate welds by doing extreme thermal aging or we can actually measure welds directly. And, studies like that have been done. I think my recollection is that the results are similar, but what I will do is I will get some of the weld data for you. I should have brought that, but didn't. I will get that data to you.

Next slide? So, now, I'd like to move from the topic of deliquescent brine formation that's relevant to the dry-out region of the poster and now I'd like to tell you a little bit about the types of processes that occur as we cool the waste package down to the boiling point with the possibility of brine seepage into the drifts. So, as we have the possibility of brine seepage into the drifts, it's important for us to understand the aqueous surface environment that could occur on the waste package surface. This gets into a lot of the calculations using EQ3/6 and the EBS surface chemistry model that Mark shared with you. So, I would say unlike perhaps a year ago, we now have a model that allows us to assess with some degree of certainty what these waste package surface environments are likely to be.

Next slide, please? This is showing you again something that you saw in Mark's presentation, but the primary point of repeating this is I just want to make the point that these are observed waters. Some of them fall in the three phase fields; calcium chloride type brines, sulfate type brines, and bicarbonate type brines. But, the important point, a brine that starts anywhere in this ternary will eventually evolve to one of the corners. And, if you'll notice the yellow datapoints, our test solutions, are very close to the corners in this triangular chart. So, our test waters that we've used over the years to evaluate the various waste package materials are, in fact, bounding. So, this is, as Mark said before, the take-away point.

Next slide, please? This shows probabilities of occurrence of different types of evolved brines in the repository. And, of course, over the last year, the project has spent a considerable effort evaluating the calcium chloride type scenario. Now, it's very important to understand this because, clearly, 1 percent of our waste packages failing is still unacceptable to us. You know, we're striving for very high standards. But, the important point to note is the two brines that would be closest to a pure calcium chloride have almost no probability for occurring. This third calcium chloride type brine has a very significant nitrate level in it. In some of the slides that will follow, you'll see that we have enough nitrate in these solutions so that we don't expect the corrosion--the localized corrosion to be particularly problematic, perhaps with the exception of the welds and we're taking steps to make a better look at that.

Next slide, please? We have actually gone in and we've calculated for various of these water compositions, both the chloride-nitrate ratio, and as you'll see in the following slide, the nitrate-chloride ratio. Bins 1, 2, 3, and 4 are the four Bin water compositions that fall in that calcium chloride regime meaning that if you apply this sort of very simplistic theory that you would expect those perhaps to evolve to a pure calcium chloride type brine. Now, the important thing about looking at Bins 1, 2, 3, and 4 and as you look at the nitrate-chloride ratio, you see, in essence, there's quite a lot of nitrate there.

Next slide, please? And, as Professor Duquette points out, it's much more appropriate to actually look at the ratio of the nitrate ion to the chloride because it's a measure of how much inhibitor you have present. So, as we do this, we see that most of these are clustered around a nitrate-chloride ratio of about 0.1. We're very happy to inform you that most of the tests that the project has done are also centered around this red line; the point being that we are testing in representative test waters. We're testing in solutions that have chloride-nitrate ratios or nitrate-chloride ratios, if you prefer, that are representative of these Bin waters and also waters that we observe at the mountain. These are some pore waters for the Paintbrush. I have some slides in the backup that--I put these on here, more or less, as a reference point, but if you plot the waters for the Topopah, you see some more results.

Representative waters, Bin waters, they all have this similar chloride-nitrate ratio.

Next slide? So now having some knowledge of what our surface environments are realistically--and again getting back to the last meeting--there is a difference between what's possible and what's plausible. But, looking at plausible waters and measuring these potentials, I think we can start to have an accurate idea of how well the waste package can perform in some of these scenarios. And, again, these measurements pertain to the types of processes that can begin to occur in this transition region as we cool the waste package temperature down into this blue region. The performance is pretty much insensitive to any water chemistry because it's good in all.

Next slide? So, the objective of these cyclic polarization studies, potentiodynamic tests, are to quantify the threshold for localized corrosion in aqueous solutions that are representative of bounding--and I emphasize bounding--deliquescent brines and evaporated seepage waters. And, these are the very types of waters that Mark just discussed with you. So, frankly, thanks a lot to the folks at BSC and some of the other labs. I do think we're starting to get a very good hold on what these environments really are. We've tested at near-saturation, 18 molar chloride, and we have again tested primarily at two nitrate-chloride ratios, though we have some data at other levels. But, the two that we worked primarily are 0 and 0.1, pure calcium chloride and calcium chloride with this level of nitrate inhibitor. And, we've made these measurements at temperatures as high as 160 degrees Centigrade. We've used two types of samples. The bare waste package surface in uncreviced regions would be best represented by the standard types of discs that are used in ASTM measurements of cyclic polarization, whereas we use a special multiple crevice assembly sample to mimic the effects of crevice corrosion that one might have at contact points; for example, where the waste package is sitting on the pallet.

Next slide? This shows one of our thermostatic potentiostats that is used to collect this polarization data and again we've linked our model to these potential measurements. It's a very good measurement and, frankly, a very good basis for a model. You can see that the reference electrodes are water cooled to make sure that they are giving us accurate measurements of potential if these heat up, if one runs into a problem as some investigators at other places have found. And, we also have a condenser on the head of our kettle where we do these corrosion tests. And, this condenser is particularly important because any volatilized or disproportionated mineral gas that might come off would be recondensed and fall back into our pot where we're doing the corrosion test.

Next slide? This shows the multiple crevice assembly sample that's been used to determine many of our repassivation potential. The beauty of the multiple crevice assembly sample is that this scalloped crevice forming washer actually forms many, many crevices around this lollipop shaped sample. So, by forming these multiple crevices, we actually form these occluded geometries that can give rise to the types of hydrolysis reactions and the lowering of pH alluded to by the panel a few moments ago. So, the measurements of breakdown potential and repassivation potential that we're measuring in many cases are, in fact, representative of crevice surfaces.

Next slide, cyclic polarization. There are multiple ways for determining the breakdown potential or the threshold potential for localized attack. If you look through the scientific literature, you find that different sciences, different investigators, different institutes use different methodologies. So, in striving to please as many people as we possibly can, we've decided to actually evaluate our data by all three methodologies. For example, Method A, we measure the actual potential where we start to see breakdown of the passive film, at least, in those cases where this potential can be identified. And, in this particular case, we look at--as we start seeing these anodic excursions in current density, we pick off the potential where the current density has risen to about 20 microamp per square centimeter. And, from our experience, we know that that's clearly in a regime where if we have had a breakdown of the passive film, that is a fairly good measure of that breakdown potential. Method B and Method C are both methods for determining the repassivation potential. And, it's a little bit of a chicken and an egg story. Until you have breakdown of the passive film, it's physically impossible to measure repassivation because first you have to have breakdown and then you're going to have repassivation.

If you have had breakdown of the passive film, you have two methodologies for determining the repassivation potential for the surface. One is what is referred to as an ER1 method and another is an ERP method. ER1, in essence, makes the fairly good assumption that the passive current density is around 1 microamp per square centimeter and this is very similar to what the NRC's Southwest Research Center does except in their particular case they make the assumption that this threshold is about 2 microamps per square centimeter. So, we draw a horizontal line across our polarization curve about 1 microamp per square centimeter and we see--we pick off the potential at which the negative going hysteresis loop intersects that threshold and that then gives us the repassivation potential. A second methodology which, frankly, probably has a better founding in physics is actually the intersection point between the forward going scan and the negative going hysteresis loop. And, by picking off this crossover point, we actually pick off a point as we scan the potential of the surface and the negative direction. We pick off a potential that corresponds to a current density that we know is characteristic of a current density for a passivated surface. So, this is a fairly good indication during the negative going scan that this is, in fact, a repassivated surface. But, again, for our data, we use all three.

Next slide, please? As I move into the summary of the data of all the calcium chloride and calcium chloride with nitrate data, I would like to show you actually some of the polarization curves because, quite frankly, I think this gives you a much better feel especially for those of you who are experts as to how these surfaces actually behave in these calcium chloride solutions.

So here, as you see, 45 degrees Centigrade, a very concentrated 10 molar chloride solution, a multiple crevice assembly sample. So it is, in fact, crevice. We begin at the corrosion potential. We scan the potential in the anodic region. We go all the way up to 1.2 volts and this is the point when we began to electrolyze our solution. So, the reason we don't go higher is we're not limited by the material, we're limited by the electrolyte. So, at this particular point, we have potential reversal. And, one thing I'm finding about getting older is my hands were steadier a few years back. But, I'm not nervous; I'm just having poor motor control. But, at any rate, as you reverse the potential scan and go in the negative direction, you can see that the hysteresis loop here is actually a little bit below what it was in the forward going scan. If we had breakdown of the passive film, clearly, we'd have a positive going hysteresis loop. That's not the case here. So, at 45 degrees C, there is no breakdown of the passive film. Now, this has some complications for the person gathering the data because, for example, if your boss tells you to go out and measure the repassivation potential at 45 degrees Centigrade or you're fired, well, you better start looking for a job because there is no repassivation potential here. So, a very, very good measurement.

Next slide, please? Okay. As we go up in temperature, again we've got 10 molar chloride, a multiple crevice assembly sample, 90 degree Centigrade. We see that we do in this case have breakdown of the passive film. We start at the open circuit corrosion potential, we scan in the anodic direction, and we do, in fact, get breakdown of the passive film at this particular point. So, we can apply Method A and determine the breakdown potential here. Then, we continue to scan up to some reversal potential then you can here during the negative point scan we do, in fact, have this characteristic positive hysteresis loop that is indicative of a depassivated surface. Now, at this particular point, we have crossover between the hysteresis loop and the forward going potential scan. So, at that point, we have a current density on the surface that corresponds to the current density of a--if we can go back a couple slides--I thought I was doing better with the presentation than this. Back a few slides, Denise, thanks. One more. This is good, thanks. Up one. Slide 29, there we go. So, we see that at this particular point, we have a current density that corresponds to a fully passivated surface. There are two methods again for determining the repassivation potential. One are these constant thresholds where we would look at the intersection of the hysteresis loop at about this 1 microamp per square centimeter level, or if you prefer, the NRC value of 2 microamp per square centimeter or the crossover point. The point being you can see that both of those potentials are quite close. The reason they're close is because people who accept those thresholds and various standards have a wealth of experience to draw upon.

Next slide, please? Here, additional data. This is at a very high temperature, 120 degrees Centigrade.

Clearly, we're up in this upper region of the transition region described to you by Mark. 10 molar chloride, very high chloride level, you can still see that there's a very good margin between the corrosion potential and the repassivation potential. So, we have no localized attack here.

Next slide, please? And, I might also point out those slides that I just showed you are with no nitrate inhibitor present. As we've discussed earlier today, the presence of nitrate is a very important feature. So, if we look at the 24 hour corrosion potential and these repassivation potentials determined by the crossover method or Method C, I believe, we see that we have a pretty good gap between the two. Our Delta E value is significant. And, just based on these corrosion values and these repassivation potentials, we would expect to localized attack. What I have done is I have drawn a green box--and I'll show you on a subsequent slide. We now have open circuit corrosion data for these samples after one and a half years and similar electrolytes. So, indeed, in this particular case, you do get some ennoblement, but it doesn't go any higher than this after the one and a half years, as you'll see in the next slide. But, this green box does represent the open circuit corrosion potential and the type of nobility that can develop with these samples after one and a half years. So, if I were going to do an analysis here, I would look at the crossover between this red curve and this curve to determine at what point I might start having localized corrosion of the Alloy 22 in this pure calcium chloride environment. Here, you see that this corresponds roughly to about 90 degrees Centigrade. Now, there are some other points there, but I think, frankly, given enough data, I would probably do that at or about 90 degrees Centigrade.

Next slide, please? These are actually the open circuit corrosion data collected for one and a half years at 120 degrees C and 10 molar chloride. So, you can see that the open circuit corrosion potential does come up. If there's ennoblement, it comes up and hits a very constant value at around -150 millivolts versus (inaudible) electrode. So, you have a fairly constant corrosion potential there to compare against which, frankly, is good news because it's a very stable value. You know, it isn't continuing to (inaudible) for eternity. Okay?

Next slide? So, now I would like to turn your attention away from the pure calcium chloride environments which, quite frankly, are so harsh, they are unrealistic. I would now like to concentrate on showing you some data that have nitrate inhibitor present at appropriate levels. That is a nitrate to chloride ratio of about 0.1. So, here, you see data for a 5 molar calcium chloride solution with a .1

nitrate to chloride ratio at 90 degrees Centigrade. So, you're kind of right at the lower end of this transition zone. And, you can see in this particular case, there's a very large separation between the open circuit corrosion potential and the repassivation potential for a crevice sample with these severe occluded geometries. So, we have a voltage margin of about 600 millivolts. Clearly, this is enough for protection at 90 degrees Centigrade.

Next slide, please? We have similar measurements, but we're going up in temperature. Here, you can see that we're 130 degrees Centigrade. The calcium chloride solution at this particular point has the consistency of maple syrup. It's about 14 molar chloride again with a nitrate to chloride ratio of about .1 which we believe to be representative. And, you can see that we have a very good margin between the open circuit corrosion potential and the repassivation potential. So, again, I think most experts in the field looking at this data would conclude that this is a pretty good margin against localized attack.

Next slide, please? We've applied Methods A, B, and C to all of our data in all scenarios, but I didn't want to just show you all the charts today given the limitation of time. So, I showed you here basically the breakdown potential determined by the E20 method. That's looking, during the anodic going scan, the intersection of the current excursion with the 20 microamp per square centimeter threshold. And, you can see that based on the breakdown potential which, frankly, is probably the truest measure of where the passive film breaks down, you have quite a large margin between the corrosion potential measured after 24 hours and the breakdown potential. The green box represents the open circuit corrosion potential of an unwelded base sample after about one year, I believe. I think close to 13 months, but about one year. And, here, you can see that very clearly, even looking at a corrosion potential as high as this, you still have margin at the boiling point when we would start seeing seepage come into the repository. Now, if we look at similar measurements for welded samples, we can see that the ennoblement with the welded samples--perhaps because of the precipitation (inaudible) phase--we do get a higher open circuit corrosion potential. So, you might say, well, perhaps there's a possibility of attack in the weld region of slightly below that boiling point. But, we're working on it to get additional data in this particular region. And, one more important point, as we look at this, I think we have to keep in mind that this represents, at best, somewhere between 0 and 1 percent of the possible water compositions. So, we're really--we're way out on the tail of the distribution in terms of water chemistry when we're doing these quantifications.

LATANISION: Latanision, one thing before you move on. In this case, you are plotting the breakdown potential not the repassivation potential. Is that correct?

FARMER: No, actually the previous slide was breakdown. Here, it's repassivation.

LATANISION: Okay. This time you're doing--okay.

FARMER: We have three methods. I showed you one representative curve for each method. The first one I showed you for the pure calcium chloride was Method C where we have a crossover point. We then show you the breakdown potential in Slide 35, the previous chart, and here, we are actually showing you--yeah, see, breakdown potential, the next slide, repassivation.

LATANISION: Joe, just for--again, on curiosity, how different in this case is the repassivation potential and the breakdown potential?

FARMER: They're very close together as you can see from those cyclic polarization curves that I showed you.

LATANISION: In that case, you answered my question. I would say you have a problem with welds.

FARMER: There is concern about the welds and, of course, this is why we're testing with the welds.

LATANISION: And, at very low temperatures.

FARMER: Well, very low temperatures in solutions that have a probability somewhere between 0 and 1 percent.

LATANISION: Well, okay, fair enough.

FARMER: Okay?

LATANISION: Yep.

FARMER: Okay. Next slide, please? And, just a more graphical illustration and I, frankly, show these just to tie back into the last presentation. As we showed you before, I show you one sample where you have the multiple crevice assembly and I think it's very important to point out that we actually push these to the point where we intentionally get crevice corrosion, but this crevice corrosion that you observe here occurs at the reversal potential. This type of attack never occurs close to the open circuit corrosion potential. So, we purposely fail the samples and then we look at the point as we reverse scan these where this crevice corrosion attack stops. That is the repassivation potential. But, again, nitrate has a very beneficial effect and gives you another 300 or 400 millivolts margin in terms of the performance of the material.

Next slide? This is--again, we also have multiple crevice assemblies at this temperature, but these are disc samples. We push it to the point we're actually getting pit initiation in a disc at very high potential, very high temperature, and then we reverse the scan and, of course, we get a repassivation. Again, a very high margin between the open circuit corrosion potential and the repassivation.

Next slide, please? I believe, the science and technology program, if I'm not mistaken, is going to be pursuing some gamma pit experiments as the project did in the '80s where we actually look at gamma radiolysis effects on Alloy 22. Up to this particular point in time, we've simulated the effects of gamma radiolysis on the open circuit corrosion potential by doping the solutions with hydrogen peroxide solution. And, the point here is that at the maximum gamma dose, the greatest excursion in corrosion potential would be somewhere around 250 millivolts. It wouldn't change much more than that because at this particular point, the corrosion potential effect has pretty well been saturated and additional introductions of hydrogen peroxide do not further or increase the corrosion potential very much.

Next slide, please? Now, another nice thing about the dry-out region here, we experience the maximum hydrogen peroxide production at the maximum radiation dose and relaxing the temperature. Only one catch; there's no aqueous phase up here. So, frankly, those excursions that we see in open circuit corrosion potential due to gamma radiolysis would probably not be observed. By the time we have an aqueous phase even beginning to be possible on the waste package surface, the dose is down around .1 rads per hour and those effects that you see on gamma radiolysis are far below what I showed in the previous slides. Clearly, that would be a worst case scenario.

Now, I would like to go--I think, just before I wrap up, go to a couple of the backup slides because I have, I think, another important point to make. Actually, I will tell Denise which slide we should go to. Let me see, if we could go to Slides 47 and 48, I think that would be valuable.

Okay. One of the things that we've been concerned about is this acid gas volatility, the fact that some have postulated that we might have substantial amounts of acid gas inside the repository. So, frankly, encouraged by some of our management, we decided to sort of do the standard back of the envelope calculation to see what the level of significance of these types of scenarios is. I realize, of course, those of you who are going to tell me exactly to two decimal places what the waste package diameter is. I realize that this is a hypothetical waste package. In my waste package, I use pure nickel, not Alloy 22. But, it's pretty close, I think, in terms of the orders of magnitude of numbers.

So, if I could have the next slide? Basically, what I've done is I calculated what the maximum amount of hydrogen chloride is that could come into the repository introduced by ambient seepage. And, of course, the ambient seepage, as you see from Bo's presentation, is far greater than what we actually expect coming into the repository due to the thermal hydrology effects. But, if I look and count and use the--assume all that seepage water comes into the drifts and all of the chloride brought into the drift without seepage water is converted to hydrogen chloride gas and recondensed into HCl on the waste package and all of that HCl reacts with the waste package surface to make a nickel-chloride corrosion product, I see that under the worst, worst, worst case conditions, the maximum impact or the maximum amount of waste package material that could consume would be around 3 to 4 percent. So, this is may way of kind of assessing what the level of significance of this hydrogen chloride gas problem is.

So, at the encouragement of someone else who, frankly, is more experienced than I am, it was said that maybe a graphical illustration is good. And, I didn't make a pie chart for this, but let's say if this our waste package material, I think--I only had two pennies so I have to--the budget is down. But, these are my two pennies. That is kind of the order of magnitude of impact, I believe, this hydrogen chloride gas problem is compared to our waste package material.

So, with that final point, I'll go back to the summary slides which, I think, are--Slide 43 is the last slide. So, if I could back up from there. Back one more, one more. There.

So, just to recap, again we've tried to illustrate for you that we have three logical temperature regimes in the repository. We have a high temperature dry-out zone. As the waste package cools through the deliquescence point, we have the possibility of deliquescent brine formation in this dry-out zone, but from doing corrosion studies there, we find that there's no significant impact of these deliquescent brines on the localized corrosion of the waste package surface. We further cool the waste package down to the boiling point, and as we know from Bo's work, at this particular point we can start having seepage come into the repository. So, we have to begin to worry about aqueous phase water chemistry as Mark has shown you. But, we know from doing a number of studies over the years in the aqueous phase electrolytes and which bicarbonate samples are, in fact, valid, there's not much localized attack. Eventually, we reach a threshold temperature which is probably somewhere between 90 and 100 degrees Centigrade. We've even in these worst case brines--let's say boiling calcium chloride near saturation--we have sufficiently good metallurgy to protect us. So, frankly, the waste package performance down here becomes fairly insensitive to water chemistry. I mean, it's okay in the worst case scenarios and it's certainly going to be okay in the lesser aggressive solutions.

So, without beating this any further, that I think is for all practical purposes our message. And, the last three slides, I've recapped those points.

So, thank you very much.

CRAIG: Thank you very much, Joe.

FARMER: Sure.

CRAIG: Mark, Dave--wait a minute, Mark, Dave, Priscilla, and Ron?

ABKOWITZ: Abkowitz, Board. I'm not an expert in this area which makes me, I think, qualified to ask the couple questions I'm about to ask. I'm interested in returned back to 30,000 feet and I'm going to ask you just two or three framing questions, if I could.

First of all, I might take away from the presentations that have come through with yours kind of closing the argument that the high temperature design that DOE is currently committed to will cause fewer corrosion problems than a low temperature design. Is that correct?

FARMER: Yes, and let me give two answers. Let me give --

ABKOWITZ: I only want one.

FARMER: Okay. Well, let me give my answer as a taxpayer and as a voter. As a taxpayer and a voter, not as a member of this project, as I look at this data, whether I'm a part of the project or not a part of the project, I prefer the high temperature operation because, frankly, it does keep the waste packages dry. If you look at the number of problems that you mitigate with a dry waste package--for example, preventing microbial growth--I think it's a better design.

ABKOWITZ: Okay. I'd like to return to my question. Just a yes or no answer would be adequate.

FARMER: Okay.

ABKOWITZ: With the information presented today, can one reach the conclusion that the high temperature design will cause fewer corrosion problems than the low temperature design?

FARMER: I personally believe that would be the case.

ABKOWITZ: Okay. And then, secondly, how certain are you of that? Are you more than 90 percent certain of that conclusion?

FARMER: I haven't quantified my answer.

ABKOWITZ: Well, what will it take to quantify the uncertainty?

FARMER: Frankly, that's going to be done through the TSPA calculation. What we've done for you today is show you our database. We've shown you the data that we've collected, what we've quantified. The way that the actual performance assessment for the waste package will be quantified and calculated is to take these measurements that we have that are being used by our TSPA group, they're being into probabilistic calculations, and this will then be converted into how confident I am.

ABKOWITZ: All right. Could you speculate on what aspects of the modeling effort you have the least confidence in, and therefore, that's where the uncertainty modeling attention needs to be focused?

FARMER: Well, you know, I'm an electrochemist or electrochemical engineer actually by training. So, I'm prejudiced towards the corrosion processes as are many in this room. I have a colleague next door who is a metallurgist and who is very partial to the precipitation kinetics problems. So, I think it depends, by and large, by who you talk to. Frankly, I'd rather work on corrosion problems. So, I tend to see more problems there.

But, to tell you the straight of it, I think, frankly, we've done a pretty good job of covering the bases. It's a very broad problem. We initiated--for example, let me go back to phase stability. One of the reasons that we shied away from the Hastaloys in our early '80s--I'm sure as Dan probably recalls--we thought that there would probably be phase stability problems. You know, we've initiated work with Larry Caulfed at MIT. He's using a lot of the expertise, you know, with the Caulfed modeling approach and we've calculated phase diagrams and now we have, frankly, a fair degree of confidence in the phase stability of these materials at relatively low temperature. I think we need to collect more corrosion data. I think now that we have started to get handle, a very good handle, on the waste package surface environment--you know, you think about it. The last year because we didn't have perhaps the handle on the surface environment that we should have, we spent a lot of our effort making measurements on an environment that has a probability of 0 to 1 percent. Well, good, you know. This is a good thing to do and I'm glad we did it. But, I think, the thing we need to do now is we need to go back to some of these more realistic environments, the benign environments, and look at what the degradation scenarios are there. We have a lot of historic data there, but I think, you know, frankly, our researchers have gotten better, our techniques have gotten better, and I think now we need to go back and look at measurements in more realistic environments instead of just concentrating, frankly, on some of these things that, frankly, are probably overly weighted in terms of the resources that we're investing.

ABKOWITZ: And then, one final question. Assuming the direction of your conclusions, can we go hotter? Shouldn't we go hotter?

FARMER: Frankly, the limit on the waste package operation, you know, on--I think in terms of chemistry and material science because that's what I do. But, we have a 350 degree Centigrade limit on the waste package and that's determined by the thermal stability of the zircalloy cladding on the fuel. Okay. So, that's the upper limit for the waste package.

Then in the old days when we first started going our phase stability studies, we thought we were bounded to around 300 degrees Centigrade based on some very early sketchy data that we had for phase stability. We thought we were bounded to about 300 degrees Centigrade for the Alloy 22 in terms of a place where you can operate for 10,000 years without precipitating a lot of phi, sigma, and mu phase which has embrittlement problems enhancing susceptibility to localized attack. Now, if you talk to Tammy Sommers and others who, frankly, probably have become some of the world's best experts in the phase stability of these alloys, you find that that limit is probably about 250 degrees Centigrade. So, I think the upper limit of waste package operation is bounded by the phase stability of the material; you know, things that we determine from time, temperature, transformation diagrams. The low temperature limits of operation or, I should say, the temperature limits of operation at lower temperatures where you might have these condensed aqueous phases is a force determined by a susceptibility to localized corrosion and stress corrosion cracking. And, the stress corrosion cracking is a whole different story and we're looking at that and we have--we're doing stress mitigation and we have a whole program of stress corrosion cracking that we haven't even talked to you about much lately.

ABKOWITZ: Thank you.

CRAIG: Okay. Dave Duquette?

DUQUETTE: Duquette, Board. I guess, I'm a metallurgist that does corrosion. So, I guess, I sit in both offices.

FARMER: There you go. Great.

DUQUETTE: Could you go to Slide 31, please?

FARMER: Okay.

DUQUETTE: And, I would just like to correct or, at least, address some terminology. I'm not sure I want to say correct.

FARMER: You betcha. Okay.

DUQUETTE: But, the green solid line at the bottom, you've indicated is the corrosion potential. And, I would argue is the zero current potential which depends very much on how long you've decided to do your cathodic reduction and it depends on surfaces, it depends on surfaces, it depends on a whole bunch of things. You've shown unequivocally, I think, that the corrosion potential which is the free potential that it arrives at is quite a bit noble to that. I would argue that the only valid data on that curve that means anything from an assessment of crevice corrosion is the dashed box versus the red line that intersects it. And, I would also argue that you should not look at the average of the repassivation potential, but the minimum in the repassivation potential for any experiments because if it can happen at that potential, it can happen in the long-term. If I do that, I drop my critical temperature, granted, without nitrate down to about 65 degrees or maybe 70 degrees Celsius and not up at that higher number. So, I'd like us not to be thinking of that lower line as a corrosion potential. It can be an artifact of the experiment as you already indicated because if you let it sit for a year and a half, it comes up to the upper potential.

FARMER: What you point out is absolutely correct. Now, one thing, though, that I would have to point out as a counter-argument is this was measured for a 24-hour exposure, as were these red data points. These red data points were not measured at a year and a half. What we need, quite frankly, and I think this is a place where some additional testing is needed getting back to the earlier question is I think we need to measure these breakdown and repassivation potentials of samples that have been sitting there for quite some time.

DUQUETTE: I fully agree.

FARMER: So, what we need--because, frankly, we've measured a few of these and, you know, those give some rise for optimism because it isn't just this that's just in the anodic direction, you also get shifts of the upper curve. So, I'm in full agreement with you. Frankly, as we look at these curves, we are in positions where--you know, we get to a point where we have to kind of freeze what we know about--I mean, we're never going to know everything about any material or the repository, but we're going to have to build it. And, I think, it's very important that we have the very best design that we possibly can and we've done that.

But, I might also point out that while you have this intersection, this is why we've worked so hard to get these water chemistries that Mark described to you, 0 to 1 percent, and frankly, the pure calcium chloride probably zero. In fact, as we looked and did all that binning process, none of those calculated waters fell in this category. They all had substantially more nitrate present. So, if you look in the curves that follow this, you find much more margin in regard to the resistance to localized attack.

DUQUETTE: Which brings me to the second part of my question. It has to do with the nitrate situation and relates back to what Mark had talked about a little bit earlier. You've assumed that the crevice corrosion initiation and propagation will all occur in the cool-down period, but you haven't set up any situation where you might have crevice conditions or potential crevice corrosion conditions during the heat-up period while it's being put in place. That is that the water that's there gets into the crevice, doesn't cause corrosion then because you heat it up to some extent, may or may not boil out of crevices because of capillary situations, and then you're back in the cool-down period having set up an environment which doesn't have nitrate inside the crevice. And, it would be interesting to do some experiments where you purposely wet the surface with no nitrate and then did your experiments in a solution that contained nitrate to see if the nitrate was able to protest the inside of the crevice even though it's on the outside.

FARMER: That's a very good point. But, one thing--and I have to apologize because, frankly, there's a lot of historic data. You know, we have 17 years of data and it's hard to put it all in an hour, though we try. But, we did do some experiments, probably I'm thinking maybe it was five or six years ago, where we actually built crevice cells and we put microsensors in these cells and measured the pH in these crevices. We looked at, for example, the types of crevice corrosion attack that you would get and the pH suppression you would get in these crevices without nitrate inhibitor or without bicarbonate as a buffer and, you know, we would see predictable things occur. You would polarize a sample up to a particular point in time and then you'd start seeing the lowering of pH in the crevice even before the passive film would break down and then eventually failure of the passive film. We would do similar experiments with realistic waters which at that particular time we were looking at things like SCW, you know, simulated concentrated water which is a very, very concentrated brine that has all the ions that you would encounter at Yucca Mountain. But, we would look at some of these expected waters, and frankly, you could polarize the sample with those crevices to incredibly high voltages and you would not see these hydrolysis reactions occurring in those occluded geometries. And, this is, I think, consistent with some other published work in the field. We did numerical simulations of that and drew similar results.

So, I think, as we've looked over the years inside these local environments using microsensors, we've seen that there's a very big difference in the types of hydrolysis you get in these occluded geometries with and without nitrate, with and without buffered ion.

DUQUETTE: Finally, there are, of course, a variety of ways to look at crevice corrosion resistance. One of those is the one most of us accept which is the one you've used here.

FARMER: Okay.

DUQUETTE: There's another school of thought that says the size of the hysteresis loop is a measure of the crevice corrosion resistance of materials. As a corrosion scientist, wouldn't you be more comfortable if you had a material that had no hysteresis loop, at all?

FARMER: Oh, absolutely. You don't want the passive film to breakdown, at all. But, frankly, one of the problems that we ran into early-on a few years back is we were trying to assess what voltage to use as the breakdown potential. As you know, in many of these standards--for example, as we measure these repassivation potentials, it's sensitive to the technique that I use. If I pick a different reversal potential, I change the repassivation potential. If I change the scan rate, I change the repassivation potential. So, any time, in my mind as a scientist, if I start having a measured parameter that is a function of how I run the test, that's a cue to me that something isn't exactly as it should be in the testing methodology. But, even so, this is the standard methodology that we use in the field and I'm sure we all know this.

DUQUETTE: Duquette, Board. I can't disagree with you, at all.

FARMER: Yeah. Yeah. And, frankly, early-on, we were thinking the most rigorous way to do this is actually potentiostatic step methods where you keep stepping the potential to the point where you actually do see the surface depassivate. That's probably the most rigorous way of doing it, but it's also the hardest, the most time-consuming, and for those reasons, the method that many people shy away from.

CRAIG: Okay. I have Priscilla and Ron and Dan. And, make your questions brief, please.

NELSON: Yes, sir. Nelson, Board. I'm certainly not an expert in this area, but I found two questions. One, I think, was nearly the same as Dave's second one which deals with the expectation that water is present before this chart starts.

FARMER: Oh, good point, yes.

NELSON: And, to what extent is that something that should be considered because I actually think there will be water present during the heat-up.

FARMER: Actually, I'm going to let Bo answer that question if he doesn't mind. He got Mark and I; so, we'll get him back now.

NELSON: Yes, the question--

FARMER: I've been looking for the ideal opportunity to pass it to him.

NELSON: Nelson, Board. The question doesn't deal with will there be water, but assuming that there is water.

FARMER: Okay, you bet.

NELSON: Now, talk to me.

FARMER: Okay. So, you assume there is water?

NELSON: Yes.

FARMER: Well, if you have water during the heat-up phase, clearly, I mean, you have--our criteria for whether or not you can have aqueous phase corrosion is do you have an aqueous phase? So, if you have an aqueous phase, we would argue, yes, you can have an aqueous phase corrosion.

NELSON: Nelson, Board. Can that be important in the overall expected performance assessment?

FARMER: Well, I actually read through some of the AMRs on thermal hydrology--not that I understood them, but I read them, you know, like the dutiful student--and what I did get out of reading them is it seems to me that as we go above the boiling point, you know, water kind of starts to leave the--I mean, the drift walls dry-out. In fact, before we start hearing up, if you pass ventilation through the tunnels, the surface of--the walls of our drifts right now at Yucca Mountain are dry.

NELSON: No, wait. No, I'm not asking you to explain the thermal hydrology. I'm just asking you to say if water is there during the heat-up, all right, up until the time of--

FARMER: Hypothetically, if water is there?

NELSON: Yes, if it's there?

FARMER: Hypothetically, if I had a--

NELSON: Can it be important to your assessment of the life of the package?

FARMER: I would say yes, but--and let me caveat this. The beauty of the chemistry model that Mark outlined for you is on the old days, you know, we had to do very painstaking tests, as you have illustrated here, where we would actually go out and experimentally try to simulate these evaporative concentration events. With the modeling that we've now done that is bolstered by having done these experiments, we can now simulate the water compositions, the equilibrium water compositions we see on the way up and on the way down. By doing these binning type processes and knowing that, well, all of my waters are going to be represented somewhere on those 11 Bins, and if I evaporate one of those Bin waters, I'm going to have some water that is representative of what I see in the repository. It allows me to tie realistic conditions back to test environments. When I look at the types of waters that I expect on the way up or on the way down, I'm not thinking--based on what I've seen with these results, I'm not thinking there's going to be much calcium chloride there. Certainly, not a saturated boiling calcium chloride with no nitrate. No, I don't think that's very realistic. And, when I put realistic amounts of nitrate in there in an open system with all the other realistic constraints on the system--

NELSON: Including the microbes?

FARMER: Including the microbes.

CRAIG: Okay. I've got to break in here. We've got two more quick questions. Ron?

LATANISION: I'd like to follow up on some of the-- Latanision, Board. I always forget that. I'd like to follow up on a question that Dave Duquette was asking.

FARMER: Okay, sure.

LATANISION: And, if we could turn to Slide 7?

FARMER: Okay. Sure, Slide 7. Okay.

LATANISION: This is a schematic, admittedly, but I think it's very instructive to just walk through this quickly.

FARMER: Okay, sure.

LATANISION: As you point out, if you exceed the breakdown potential, a crevice will become activated.

FARMER: Yes.

LATANISION: And, as you drive the potential in a reverse direction, you'll reach a point at which the crevice will become repassivated or protected.

FARMER: The outside film will reform.

LATANISION: Right. Now, that is, conceptually, something that I think is very clear in the literature and people would agree with. But, the important issue here is from the point of view of determining whether or not a given metal or a given alloy is susceptible in service is where the corrosion potential lies relative to those, what I would describe as, anodic kinetics that are shown in the hysteresis loops.

FARMER: I think that's true and that's what we try to capture with the Delta E value there.

LATANISION: Well, on that basis, Joe, I would say that all three of the metals you've shown here are resistent at that corrosion potential to crevice corrosion.

FARMER: Yes, exactly right.

LATANISION: Right. Now, on the other hand, if you allow the crevice potential to rise as you had shown in Slide--let's now go to Slide 37--36, sorry.

FARMER: Okay.

LATANISION: It's a companion to the one that Dave looked at. What you're now showing in this slide by virtue of the change in the dashed green box that you have described as being typical of the base metal--

FARMER: Right.

LATANISION: --you've shown a considerable ennoblement, a couple of hundred millivolts and that is approaching the repassivation or breakdown potential which, as you pointed out, were relatively close. The point being that I think there's very much less reason to expect the breakdown potential to change. I think there's far more reason to expect the ennoblement in terms of the open circuit--

FARMER: The open circuit corrosion potential.

LATANISION: Right.

FARMER: You're probably correct. There is some change though in terms of the passive film properties. As the sample sits around for an hour, defects are less prevalent.

LATANISION: Right. But, I think historically if you look into the literature, the anodic polarization curve, the shape, is relatively fixed. What is important is the location of the corrosion potential.

FARMER: Right, the green box.

LATANISION: That's right.

FARMER: Okay.

LATANISION: And, that's always a function of the intersection between the anodic kinetic curve which would be represented by the three different material curves that were shown on Slide 7 and the cathodic kinetics.

FARMER: Yes.

LATANISION: And, if you take the position, which I think is legitimate here, that the cathodic kinetics are somehow increasing the corrosion potential in a noble direction, not an unusual phenomenon in corrosion engineering systems.

FARMER: Okay.

LATANISION: That you're approaching the critical potential or breakdown potential, and therefore, at a relatively low temperature you've got a susceptible material.

That's the first point.

The second point is that welding--this is why I asked at the time you showed this--welding looks to me as though it creates another degree of susceptibility beyond that which would be typical of ennoblement because your potentials are even higher.

FARMER: That's correct.

LATANISION: So, I'm very troubled by this, I must say.

FARMER: Let me--

LATANISION: Let me just finish.

FARMER: Sure.

LATANISION: And, I know that you point out that you're dealing here with a relatively low frequency of low probability environment. But, I'll just say that, you know, the demographics of solution chemistry are such that the species that are in high frequency or high population are not necessarily the ones that you're concerned about.

FARMER: Right.

LATANISION: I mean, a few parts per million of chlorides in a steam generator will create havoc with a nuclear power plant.

FARMER: And, with due respect, Ron, and back to a question that Mark had raised earlier, how confident are you? I think the first thing to realize is, frankly, we have data. It's very noisy in this band. The centroid is kind of around the center of this box and I think the--I, frankly, at this point couldn't tell you if it's one sigma or two sigma, but this represents the band of data that we see.

LATANISION: Yeah.

FARMER: One thing that you get from the probabilistic risk assessment that we do in the project, for one thing, you know, we can't assume that all of our environments are this because they aren't. I mean, that would be unrealistic.

LATANISION: Of course. Right.

FARMER: We also can't assume in a probabilistic risk assessment that all open circuit corrosion potentials are here or here because that's wrong. So, what we do is we assume the center of the distribution and we look at the width of it and we do a probabilistic risk assessment. And, things that occur at a probability of less than 10^{-4} are not such a problem for us. Now, if we came in here today and we told you absolutely nothing about our knowledge of the waste package surface environment and left you at the end of today with the belief that this might be 100 percent probability, you know, that's a problem. But, the fact that I--

LATANISION: I guess, I'm missing a point though. I mean, even if it were 1 percent probability, but it were the causative agent and it were present on a 1 percent frequency, I'd be concerned about that. I mean, it's like looking under--you know, looking under a lamp pole for something you've lost even though you didn't lose it near the lamp pole. I mean, from my point of view, if this--if, and it's a big "if", Joe, I admit that. I don't know that this is the causative environment, but if you take the position that it might be, then this data would tell me that if you're operating at temperatures--and let's take a midpoint in terms of the corrosion potential given the dashed boxes, I would say that in the case of the base metal, you know, you're at maybe 110 degrees Centigrade, and the case of welded base metal, you're somewhere closer to 100 as your threshold. And, those are well-below the operating temperatures in terms of the high temperature--

CRAIG: Okay. I have to apologize. We have to take a break. Dan Bullen, you get the first question after Bill Boyles' talk.

FARMER: Well, let me make one final--

CRAIG: Okay.

FARMER: --if I can, frankly. If Denise could go to Slide 51 and 52? You know, these actually represent realistic waters. Simulated acidic water, you know, people have said they don't, but, frankly, probabilistically this is a very realistic environment. We have a very large margin between the open circuit corrosion potential and what might be the breakdown potential, but, frankly, probably more a limit on the electrolyte. And, frankly, no negative going--

I'm sorry, no positive hysteresis here. So, this is a realistic environment. And, here, you have two conditions. You have a severely aged sample which would be the very worst type of metallurgy that you'd see with any welding occurring and you have a base metal. And, in both cases, you have fairly good margin and water that we would expect at relatively high probability.

And, if I could have the next slide? This is the other category of expected water. Again, we have a good margin between the open circuit corrosion potential and, you know, realizing, of course, that we can have ennoblement here, as well. Frankly, there isn't much ennoblement in this particular electrolyte, but there is some with the SAW, as you may recall from some earlier meetings. But, again, we have a fairly good margin between this anodic oxidation peak and the open circuit corrosion potential. We have a 600 millivolt margin and the ennoblement we're talking about is a couple, 300 millivolts.

CRAIG: Okay. At this point--

FARMER: And, these are expected waters.

CRAIG: At this point, we're taking our break. We've got 15 minutes. You're all due--

SPEAKER: Whether you like it or not.

CRAIG: Whether you like it or not. You don't have to take the full 15 minutes.

FARMER: Thank you very much.

CRAIG: Thanks a lot, Joe. And, we're all due back at 4:25.

(Whereupon, a brief recess was taken.)

CRAIG: Folks, can we sit down, please? It's time to get started.

(Pause.)

CRAIG: Okay. Bill is ready to go. And, we're now into the last session which is Bill Boyle talking about the Technical Program Summary and Discussion. Bill, you've got 20 minutes and I'll ring the bell after 15.

BOYLE: Okay, thank you. As the title indicates, I'm going to try and summarize the technical program that we've been working on most of the day. The last four slides in Bo's presentations, I think they deal with observations and summary and conclusions and presents our understanding of what will happen with the water, both under ambient conditions and also during heat-up into the orange region. And, not just what happens with the water movement, but also with respect to water chemistry. I think based upon his entire presentation summarized in those last four slides before the backup (inaudible), he gives a good understanding. We have a confident understanding of what happens with the water and the chemistry of the water. We then switched over to Mark Peters' talk using as a starting point the water chemistries that Bo had described.

Now, Professor Duquette, he mentioned that with respect to corrosion, those water chemistries as calculated by LBL may be not particularly germane or relevant to crevice corrosion, but we're interested in those starting water chemistries for other reasons beyond corrosion. We need to know what effects there might be on the rock itself. Once we have those chemistries, we might as well use them.

Mark in his talk talked about not only what happens to those seepage water chemistries, but also deliquescent brines and also what happens if water vapor condenses and interacts with the dust. And, in his talk, the last three slides before any backups deal with each one of the color coded regions and what we expect will happen when we're in each of those regions.

Using the knowledge of chemistry in the tan region that Mark described, we then had Joe Farmer's talk on the blue region and what will happen with corrosion of the engineered system. The last three slides of Joe's presentation before the backup materials, there's one slide each again for the orange, tan, and blue regions and what we expect to happen when we're in those temperature regimes.

And, I think, in particular, all three presenters and their materials, the last four slides of Bo's and the last three of Mark's and Joe's, very capably summarize what we expect to happen and all the prior material provided the basis for that expectation. I'm mainly here because we're coming back after a break and I'm just reminding people of what they've heard earlier in the day.

But, like Professor Abkowitz, I'm also not necessarily an expert in these studies and I think it's very --I would submit most people in this room, people are lucky to be expert in perhaps one thing, not all these many things; corrosion, metallurgy, thermal hydrology, geochemistry. So, I think, it's useful at times to take more general higher level perspective like Professor Abkowitz did. So, during the course of the day, I've tried to capture some of the concerns and I'll try and bring them up now during this 20 minutes. But, if I only address them from a high level point of view, we have the rest of the time and we can bring them up during discussion again.

So, one of the first concerns that came up was Dr. Nelson wondered whether or not the capillary effect that people observe in soils, if you will, porous medium, was applicable to fractured rocks. Well, as Bo had said during his presentation, the capillary effect is related to the surface tension of water. And, whether people realize it or not, I think they actually have a lot of experience with the surface tension of water. Usually, in a chemistry class with beakers, people are aware of the--of the water and its surface tension is pulling the water up the side of the beaker even though there is no porosity in the glass itself. It's not really a porosity effect, per se, but it's the surface tension of the water.

It also comes up with respect to biology class and thin microscope glass plates. If you take two plates of glass and put a drop of water between them, the two plates will stick together. I don't know if people remember that from biology class, but it's the surface tension of the water, and in some ways, that's very applicable to fractures, if you will, very small aperture fractures. It's hard to get the water out.

But, Dr. Nelson had mentioned what about open fractures? Well, I think, people's windshields in a rainstorm or shortly thereafter also give indications of what the surface tension of water can do. The water will bead up even without the second part of the fracture being present. You know, there is just the open surface of the glass. Water will bead up and won't necessarily move under the effect of gravity.

So, I think, you know, without going into a lot of details that we fully expect the capillary effect to work in fractured rock just as we do expect it to work in the porous medium.

Let's see, Professor Corradini brought up a concern about geophysics also during Bo's talk. And, Bo, like any good earth scientist who is not a geophysicist, expressed skepticism, if you will, or acknowledged that there was a degree of uncertainty about the results. What I'd like to emphasize about those geophysical results is we had more than one geophysical method operating in the drift scale test. We not only had the ERT, we had the neutron logs and we also had the ground penetrating radar. All three techniques are fundamentally different from each other and all three told essentially the same story which gives earth scientists more comfort when it comes to geophysics that if more than one method is used and it's telling the same story, perhaps the story is believable, particularly when it was coupled with our physical observations that we would see water in boreholes when we expected to see it and, as the heating front went by, we couldn't get water out of the boreholes anymore. So, I think our understanding of where the boiling front is, although it's, in part, based upon geophysics, is a legitimate understanding.

I think it was during Mark's talk there was a concern about--I think, Dr. Bullen brought it up--do we have the right collection of dust that we're analyzing? And then, I also believe that Dr. Diodato brought this up, as well. I don't believe that there's any reason to suspect that we don't have a representative dust sample. The dust collected by the USGS shows that it predominately produced by the surrounding rocks, but there are other things in there that perhaps aren't present in the rock. But, although there's many advantages to living in the desert southwest, one of the disadvantages is that on days like today, windy here in Washington, if we have such wind conditions in the southwest, we also have blowing dust storms. Clark County, Nevada has been an EPA, non-attainment area for blowing dust. One of the lakes mentioned in Mark Peters' talk, Page 10, Owens Valley, California, it has such a significant blowing dust problem that the southern California water users now have to keep more water in Owens Lake to keep the blowing dust down and also provide a rock armor to keep the blowing dust down. The general point being is there's a lot of blowing dust in the desert southwest. So, I'm not surprised, at all, that we have nitrates or almost anything else under the sun present in the dust which Zell Peterman also mentioned that we do see it in the Forty Mile Wash area.

Let's see, Dr. Nelson also brought up what about the purple region right here in the heat-up? Well, from a scientific point of view, if we understand what's going on where the purple region crosses the tan region over here, we can use that same scientific knowledge to examine the purple region and the tan region over here which is of much more limited duration. As I believe was already mentioned, when we finally do go into that heat-up phase, it will be after a prolonged period of ventilation during which a lot of the rock will have dried out. Every day of heating that goes on, the relative humidity gets lower and more and more water moves away. It's a continually improving condition, if you will, during the heat-up in contrast to the cool-down where the possibility of water is actually increasing as every day goes by.

BULLEN: Bullen, Board. Just a quick question there, Bill. When you talk about the heat-up phase, that's immediately post-closure, right?

BOYLE: Right.

BULLEN: So, the heat-up is occurring with no ventilation--I mean, the ventilation has occurred for 50 years, but if I start mobilizing water due to the heat, it's mobilized, what, from farther in the rock? Is that the point you're trying to make?

BOYLE: Well, the point I'm trying to make is there's less water to be mobilized to begin with, you know. In the drift scale test, we had dried out a thin skin or rock, if you will, and we ended up with water in the drift scale test. After 50, 100, 200 years of ventilation, you know, I'm sure Bo could tell us how far into the rock mass the drying front has gone, but it will be just that much less water available to move into the drift. And, every day, the temperature goes up, the relative humidity goes down in case people are concerned about relative humidity effect. And, every day that the temperature goes up, it's trying to drive most of the water away, although some does come back into the drift.

CORRADINI: Since he stopped you--Corradini, Board--may I ask you a question now?

BOYLE: Sure.

CORRADINI: I want to know one more time since we're having a private discussion about it, the purple band, what is the loading that gave us the lower part of the purple band versus the upper because I thought I understood and then I tried to explain it to a colleague and I was told I didn't understand. So, do you mind one more time?

BOYLE: I'll try once again and we'll see if I get it right. That in the analyses that led to the plotting of those temperatures, we had a line load, if you will, that over the length of an entire drift, I believe that what it averaged to was 1.45 kilowatts per meter over a kilometer or a kilometer and a half length. That line load was produced by waste packages with many different heat outputs, all the way from very hot ones with younger spent fuel down to the defense glass which is really not putting out much, at all. But, as Bo had brought up in his talk, nature is trying to get rid of all the temperature gradients it can either through radiation, conduction, convection. So, even though the waste packages themselves in this whole drift have greatly different heat outputs, anywhere from 12kw down to 1, they do appear as if they average out at 1.45, but nevertheless, they still are putting out different amounts of heat, but they are trying to average out through these radiation, convection, conduction. And so, if we actually put a thermometer on the coolest waste package in that long drift, we'd get something here, and if we put it on the hottest waste package in that long drift in the center of the repository we'd get the higher temperature.

CORRADINI: So, now, the question, I guess, Dan was asking or just to push it one step further for Dan, so that in the heat-up phase, I would expect what you're doing is driving away the water in two directions, right? You're driving away into the gaseous phase and back into the rock simply because you have a--they physics of it, at least as I would understand it, is you have a pressure front and the pressure is driving the concentration both ways because that's what the heating is doing to this.

BOYLE: Right.

CORRADINI: So, the fact you say relative humidity is falling is simply a fortuitous thing because the temperature is rising.

BOYLE: Right.

CORRADINI: In actuality, the concentration of steam is growing in the gaseous phase?

BOYLE: Right. That can be--right. Or even for all I know, that the water content, you know, molds of water may be constant, may be falling, may be rising, but the decrease in relative humidity probably is largely a temperature effect. But, if relative humidity, in and of itself, is a thing that concerns you, it is falling.

Now, all right, just a few more points, I hope. The discussion at the end just before the break and whether or not--what is the probability of occurrence of some of these deleterious effects with respect to corrosion? 0 to 1 percent is a figure that Joe Farmer used, I believe, or you used 1 percent. Whatever the right number is, we have to take it into account into our total system performance assessment. We have to not only for corrosion, but for everything. It's a probabilistic analysis. Things occur with a frequency that's according to what it should be and the results come out at the end. We do allow for corrosion to take place in the model and I'm not a corrosion expert, but I trust our scientists and engineers who are that they have correctly incorporated, you know, what will happen in a probabilistic sense. So, just because there's a low probability of something bad happening doesn't mean we should be, you know, afraid of it or irrationally concerned about it or--not that I'm implying that anybody is because I'll use examples that are much lower probability that we have to factor into account. On an annual basis, the lowest probability we have to concern ourselves with is something that--one in 100 million per year. And, volcanism, for example, is something down in that range of probability, and yet, we do account for it in our analyses. So, low probability events are provided for in the model appropriately.

Now, Professor Abkowitz asked if there were fewer corrosion problems hotter, and I think the question can be generalized to are there fewer problems hotter? And, I thought this was the route that Joe Farmer was going to go down when he mentioned he was a taxpayer. I believe we've provided you documentation in the past which indicates, I think, for generally pretty clear reasons that colder repositories tend to be more expensive than hotter ones. They usually involve more construction and/or years of operation, and therefore, are more expensive.

But, I believe that there's also fewer--I'll just call it health problems, if you will, hotter rather than cooler. And, I recommend our final EIS to those that haven't read it, particularly Section 4.1.7 which looks at the health effects of higher temperature operating mode versus lower temperature operating mode. What it turns out is because the lower temperature operating mode is achieved through much longer operations, there are more health effects associated with it including things like all those extra hundred years of operation, people will drive out to the site and there will be more car wrecks and things like that. So, the cooler repository not only being more expensive, also is documented in our final EIS, does tend to have more health effects than higher temperature operating mode. But, as Joe Farmer started alluding to, there probably is an upper limit to, you know, the benefits of our repository. For example, back to our liability assessment design which was certainly hotter, but the isotherms between the drifts coalesced raising a significant uncertainty. So, hotter is better under some circumstances. Our HTOM is probably better than our LTOM which you can see in FEIS, if you will.

And then, Professor Abkowitz, also one last point. He asked where was the greatest uncertainty? From my own personal point of view, in the preparations leading up to this meeting, most of the discussion dealt with the tan area. This can get down to mainly understanding a natural system problem, what happens with the water? This can largely get down to a metallurgy corrosion problem, just one scientific discipline, if you will. One scientific discipline. This requires the marriage of the two and that's--I'm not saying it's necessarily the most certain, but it's certainly generated the most discussion.

So, that's my summary.

CRAIG: Okay. Thank you.

BOYLE: Do you want me to stand here and answer the questions or do you want me to go back and--

CRAIG: Well, I'm not quite sure who the questions are going to be to, probably to everybody. So, why don't we set you up as a panel over there. Let's see, as we get on with this, Dan Bullen has the first--

BULLEN: Hour. I get the first hour.

CRAIG: What?

BULLEN: I get the first hour, don't I?

CRAIG: Absolutely.

BULLEN: I have two questions.

CRAIG: We're in a situation where we have a fair amount of time. So, actually, you don't have to be quite as brief as you would normally be.

But, Ron, if you want to continue on your metals discussion and then Carl Di Bella also had a metals discussion. So, I've got three and, let's see, I went that direction, I'll go this direction; Mike and then Dave. Anybody else? And, Norm. Dan, first?

BULLEN: Bullen, Board. Actually, Bill, since you brought the point up, I guess I'll have to follow up on this. I have a couple of issues that I want to raise, but you mentioned that the tan area raised the greatest amount of questions and had the greatest uncertainty. In the high temperature operating mode, I have to pass through that area twice. So, wouldn't it be a simpler design and perhaps a safer design if I never when through that operating mode? And, I guess, that leads to the question of is the system safer if you never go beyond the blue region? And, if so, how, and if not, why?

BOYLE: Okay. Boyle, DOE. Well, this gets back to my discussion of the Environmental Impact Statement, but let me clarify. I think it said that tan area wasn't necessarily the most uncertain, but I think it led to the most discussion. I think because it's the marriage of two different scientific disciplines, you know, the understanding of the natural system and corrosion. But, essentially, your question is if that tan region, staying out of it, never getting up to it, staying cooler than that, we avoid corrosion altogether, if you will, well, that's essentially what the low temperature operating mode did do and it was analyzed in the EIS. The high temperature operating mode results are shown. That's the purple band right there. So, it did go through the tan zone and out the other side. As I was trying to get across in the discussion of Section 4.1.7 in the EIS, you will see that particularly because of the extended operations associated with the low temperature operating mode as analyzed in the EIS, there are more health effects. I'll let you decide how important they are. Everybody can look at the tables, it goes on for page after page, and reach their own conclusion. But, what's particularly interesting is with respect to the high temperature operating mode, the biggest concern seems to be that its performance is perhaps more uncertain. We did examine this in the supplemental science and performance analysis. And, out in the periods of hundreds of thousands of years, you first start to see perhaps less good performance out of the high temperature operating mode relative to the low temperature operating mode.

But, ultimately, whether or not the cooler is safer than the hotter, I could portray it as a choice of the following. If we go cooler, it's with almost near certainty that we will suffer ill effects, you know, the auto wrecks because of the extended duration of operation, and there will be--there's more radiation doses for various reasons. Those will occur with almost absolute certainty and they will occur to our children, our grandchildren, our great grandchildren, and we will choose that and spend more money to achieve it in order to avoid potential cancer deaths 600,000 years from now. I don't know when I pose the choice that way that many people would actually take the low temperature operating mode.

BULLEN: Bullen, Board--

FARMER: Could I make a comment to you on that? A couple of points regarding low temperature. You know, we were discussing the possibility of microbial growth and the impacts on corrosion. That, frankly, from a corrosion or materials science point of view is harder to quantify than corrosion and just inorganic electrolytes. And, we realize from earlier assessments that we did that, you know, the threshold relative humidity for microbial growth is somewhere between 40 and 60 percent RH. So, in terms of minimizing the impacts of microbial corrosion, I would--my preference would be to operate under conditions where we can maintain the relative humidity as low as possible for as long as possible because I think that, once you get into looking at things such as mutation of microbes over a 10,000 year period, it seems to me that is a more difficult problem than the one we're currently faced with.

BULLEN: Bullen, Board. To follow up on that, Joe, the issue there is that if I have a LTOM, I've ventilated for that 300 years. So, I don't have the RHs there.

FARMER: And, to my second point, when you think about the ventilation, I was actually curious--my father turns out to be a civil engineer and so I posed the question to him. But, I don't think are any engineered systems that have ventilation systems that have been operation continuously for the time period that you're referring to.

BULLEN: Bullen, Board. You're exactly right. I agree with that. But--

PETERS: Can I say one other thing, too?

BULLEN: Oh, Mark, you can say whatever you like.

PETERS: Okay. Mark Peters, BSC. One thing I would add to what Bill said, do we have the basis and can we demonstrate safety long-term, post-closure, operating hot, as we like to put it? Yes is the answer. We met the standard. So, Bill focused a lot on the pre-closure aspects of it, but we're still protective of the public health and safety long-term, too.

BULLEN: Bullen, Board.

PETERS: I know you're aware of that, but I want that on the record.

BULLEN: The other issue is can you demonstrate safety cold? And, the answer is also yes, right?

PETERS: We showed that in the SSPA, but what we're here doing is demonstrating the basis for our design and that is what we're terming hot for the purposes of this discussion.

BULLEN: Bullen, Board. I would like to point out that the EIS and the SSPA were both completed with WOPDATE (phonetic) models that were not temperature dependent for corrosion? Is that not correct?

BOYLE: My recollection for the SSPA, Supplement Science & Performance Analysis, is that we did have a temperature dependence for corrosion in there that caused some lively discussion, if you will. So, we removed it. So, the calculations exist both ways. We had temperature dependence and we also removed it or changed it to see what the effect was.

BULLEN: Bullen, Board. But, the EIS and the LTOM/HTOM results that you present are not temperature dependent in WOPDATE?

BOYLE: Do you know the answer--

FARMER: Early-on, there was a concern about the temperature dependence of the corrosion rates. I think the origin of the temperature independence, if you will, went back to the fact that when the actual data coming out of the long-term corrosion test facility was analyzed, there was no indication of temperature dependence there. That isn't, quite frankly, a reflection on the data. It's a reflection of the fact that you have competing processes. As you go up in temperature, you tend to decrease oxygen solubility. I mean, they're competing effects. So, there was, as you recall, many, many meetings like this where that was debated. In the final analysis, I think there were rational and justifiable reasons put forth for that appearance of temperature independence. I think it was more, frankly, a trading off of effects than the temperature independence.

BULLEN: Okay.

BOYLE: And, I'll follow up on that. You know, Mark indicates that the short answer to your question is yes. But, my recollection is is when we had the temperature dependence, the original SSPA results, in some ways they were comparable to the final SSPA results or the EIS results in that when we included the temperature dependence, the performance of HTOM and LTOM, both improved. You know, because we spend most of our time cold and the temperature dependence really had improved performance cold, but on those million year plots, hot and cold both spent much of their time ambient. It shifted everything in terms of those dose plots out to the right, but the HTOM and LTOM, themselves, still looked the same.

BULLEN: Bullen, Board. Just one last question and that deals again with uncertainty. And, I guess, it's an opinion of the entire panel and I'll ask Bo to pipe up, too, because do you feel that certainty in performance is greater for a high temperature operating mode or greater for a low temperature operating mode? And, I have my own bias and opinion and you probably already know what they are, but can you explain to me why do you think the certainty of performance for a high temperature mode would be greater or lesser? And, any of the four.

BODVARSSON: Well, the way I look at the hot versus cold, I look at it two ways. The hot to me is more uncertain in terms of processes because, of course, when you introduce boiling, you're going to introduce thermal hydrological, and more importantly, dissolution and deposition of the waste packages. On the other hand, the benefits you get from the hot, to me, far outweigh the uncertainties of the hot because of the boiling phenomena that we discussed in my part of the talk. There is no question in my mind from a lot of geothermal experience that I have spent 25 years studying in various parts of the world that boiling reduces water contents, it causes dry-outs, it causes heat type effect that we see, it causes chemical dissolution and precipitation effects that we see, and all of these things that seem to be very beneficial to performance. So, in my mind, even though the uncertainty of the hot are somewhat larger, the advantages far outweigh the uncertainties.

BULLEN: Bullen, Board. One last question and then I'm done, I promise. If we don't go hot, do you avoid the formation of the waters that are in Bins 1 and 2?

FARMER: Actually, could I help with--

SPEAKER: Sure.

FARMER: Actually, it turns out that the binning process that Mark referred to--and, again, I'm not an expert, God knows, but I did familiarize myself some with the binning process that I think the geochemists use. As I recall, I think they used the evaporative concentration process using

EQ3/6 as the methodology for binning the waters. So, there were actually two evaporations done during the modeling. There was first synthesized, if you will, or the simulated evaporation that was actually used to determine the trajectory of a particular Bin water on the geochemical divide diagram. And then, depending upon the outcome of that simulation, they would then go back and bin the starting water. So, my belief is that actually I think that the original bin water would still exist, but of course, perhaps how the binning would work out might be determined by the operating mode of the repository because those Bin waters actually came from simulations that probably account for the temperature profile, both spatially and temporally, that we see in them.

CRAIG: Okay. That was Joe Farmer. When you speak up, just give your name briefly for the benefit of the recorder.

FARMER: Again, my apologies.

PETERS: Mark Peters, BSC. That's a good question. Unfortunately, I don't have the plots. What we would need to go to is the evolution of all the pore waters and how they broke into the different Bins and look at what piece of the time history they came out of to understand what temperature they correspond, if you follow me. So, I think, the information is available. I just think I'm at a disadvantage that I just don't have that at my fingertips.

BULLEN: Bullen, Board. I'd just like to see that. I mean, if we never boil the mountain and we don't get to Bins 1 and 2, I'd be interested in seeing it.

PETERS: We can certainly go look at the output to try to get to that answer.

CRAIG: Next is Ron Latanision followed by Carl Di Bella.

LATANISION: Latanision, Board. I just want to return to the conversation we had before the break. I'm looking at this in a totally pragmatic sense and I think we all share the same concern about avoiding circumstances that will lead to radionuclide release. I mean, that obviously is a concern here. And, I suppose from my point of view what I see in the data that's emerging from the project and also data that I think is emerging from the work in San Antonio--and I think we'll hear some of this tomorrow--the only circumstances that I see which will seem to me, at least as a corrosion engineer, that will lead to penetration of the package, the only data that I've seen, is related to localized corrosion and particularly so of welded structures. We're talking about a package that's welded. We're talking about circumstances, at least in terms of the experimental data that's emerging, that has some finite probability environmentally of occurring, and therefore, it concerns me. But, I think the crux of what I'm getting at is not just that we should be distinguishing materials from the point of view of their susceptibility and you can do that by looking at the potential differences, the real question is what is their serviceability and that's a function of not only that Delta E, but it's a question of whether it's the breakdown potential or the repassivation potential, where that lies relative to the corrosion potential. That is the issue. The data that's emerging from what I've seen leads me to draw a question or question, I should say, the serviceability of Alloy 22 under those circumstances.

FARMER: Well, let me just go back, you know, and you'll have to pardon me, Ron, for--I'm kind of replaying part of my answer, but, frankly, I think the answer is legitimate and correct. You know, again, these calcium chloride brines, you know, the project has invested substantial time and effort in investigating a lot of these scenarios that, frankly, they're possible, but certainly the possibility is very improbable. I mean, you know, these environments are not the predominate environment that the waste package will see. I mean, predominately, those waters at Yucca Mountain are bicarbonates and they will evolve that way. So, you're really looking at the tail end of the distribution when you're talking about these calcium chloride waters.

So, granted, you know, we have to be concerned about that situation and we have done our due diligence. We have quantified the corrosion in these worst case scenarios. We also are collecting corrosion data in the more benign scenarios and from my personal opinion one of my concerns is that we've spent so much time on the improbable that we don't have the confidence in the most likely scenarios that we might otherwise have. So, you know, having limited time and resources, I think, personally, I would like to see some more emphasis on the scenarios that we're really anticipating.

LATANISION: But, may I rephrase what you've said, Joe. I mean, I agree with your comment in the sense that it doesn't appear to me that the bicarbonate solutions are likely to be problematic.

FARMER: That's correct.

LATANISION: Right. And, they are the predominant environment.

FARMER: Yes.

LATANISION: But, if there is a finite probability that the presence of an environmental specie that is shown to be troublesome is likely to be present, then I think the--then it addresses the serviceability issue that I raised.

FARMER: Well, there is a serviceability issue and I guess my response would be that I think we are being very responsible in addressing that. My belief is that we have to look at this at--we're trying to assign accurate levels of concern. You know, we're assigning a weighting factor, if you will, and trying to calibrate this appropriately.

LATANISION: Right.

FARMER: But, you know, we also are not being very responsible engineers if we paint such a dire situation that we cannot achieve, you know, the mission set before us because, frankly, these alloys are as good as they get and, you know, you're going to have a hard time building any kind of engineered structure if you only portray the plausible scenarios as the most dire of circumstances.

LATANISION: Let me make sure I'm expressing myself clearly. I don't mean to suggest that there's a dire circumstance. What I'm suggesting is that there's an important diagnostic that you've generated from the project's data.

FARMER: Correct.

LATANISION: And, that diagnostic tells me that there's a changing--for example, a changing corrosion potential that is driving this system in directions that suggest to me that if there's going to be a serviceability issue, that's an issue we have to address. It would suggest that once you recognize the diagnostic that it would be worth investing some time and effort in finding ways of driving that corrosion potential back down. You know, it's a diagnostic.

FARMER: That's a very good point, you know, that you just make. I mean, for example, you know, in our props, most of us who, you know, had boats or--I mean, we all know about zincs on propellers. I mean, if this is the direction that you're headed.

LATANISION: Well, I mean, that's precisely the direction.

FARMER: And, frankly, early-on in one of the pre-viability assessment designs, we had--if you remember those days, we actually had the corrosion allowance material on the outside of the package for that very reason. We wanted a sacrificial material on the outside. And, in those days, we had the corrosion resistent material on the inside so that we would have that sacrificial layer.

LATANISION: Right. Well, let me just close by saying that I see two options really or maybe three. I mean, one is to take the approach we've just been talking about and that is to use the diagnostic and to respond to it by attempting to drive the potential in a direction which is going to provide more serviceability in the language that I've been using. The other, of course, is to consider packages that are not welded and I think that's not in the cards. Or, thirdly, to reduce the operating temperature.

FARMER: There are actually--there's a fourth option, as well, and one that we're investigating. We're considering, frankly, new materials, coatings. Some folks have actually recommended perhaps, you know, coating these welded regions so that you don't expose a potentially susceptible metallurgy to these conditions. So, there are lots of options out there. I mean, I think we have a very good design and I think the problems that we outlined for you today, we did for completeness, not because we believe that these are the most probable circumstances.

LATANISION: Well, I guess, I would be happy to see a lot more detail on those sort of remedial or responsive approaches than I guess I've heard today. That's my only concern. I mean, I think we all share the concern about wanting to make sure that these packages, if they're put into service, are as serviceable as possible.

FARMER: Exactly. And, one final important point, I believe. If we look at the distributions of the waters that we see--let's say, hypothetically, we expect somewhere between 95 and 99 percent to be represented by those waters that we have tested, you know, the yellow data points in the four corners of the triangle. If we do think that these bicarbonate waters are predominant which, frankly, is the case and we look at data coming out of a long-term corrosion test facility--those are, in fact, representative tests-- certainly, at 95 degrees Centigrade over many years, we see no indication of localized corrosions, the initiation of stress corrosion cracking with (inaudible) Bin samples, no pitting. So, I think in the predominant waters that we expect to see at Yucca Mountain, we do have a plethora of data that suggests that while you may have some of these outlier situations that are problematic, certainly this isn't the general case.

BOYLE: Boyle, DOE. I'd like to offer up a fifth way out of this issue, if you will. If there are things that can be done to make the material better and we can find out what they are either through the science and technology program or through our own efforts out at the project, that's wonderful. But, the fifth way out is remembering that these bad conditions occur with some probability. Do the analysis and if they're sufficiently low and yet we allow them to occur, the bad things happen, but the consequences still aren't that bad, we still have an okay system. You know, we do have these bad effects built into the model and they should happen with an appropriate frequency. And, even if they do happen, if the probability is low enough and the consequences associated are low enough, we're still okay. But, if there are things we can do to make it better, then let's do it.

FARMER: And, we're going to work very hard on this weld metallurgy problem. So, I don't want to give you the impression that we aren't working the issue. We're working it as hard as we possibly can.

LATANISION: Bill, I was with you all the way through to the last point. I mean, I agree. If it's a low probability event, that puts some perspective on it. But, if it's a high consequence, I guess I wouldn't feel quite as comfortable as you might.

BOYLE: Well, it's the multiplication of the two, the probability and the consequences, which is what we're really interested in. We just have to do the analyses to see how it turns out. And, our analyses have to have the capability of allowing the bad things to occur if they're believable, but they should occur with the proper frequency. When it gets into this area of corrosion, I have to defer to people like Joe and others that they have built the models correctly to allow these events, however low or high their probability is, that they occur appropriately.

FARMER: And, one thing that's being done right now that we didn't frankly have time to mention very much--

CRAIG: Joe Farmer.

FARMER: I'm Joe Farmer from Livermore for those of you who don't know me by now. But, at any rate, it turns out that the project is spending a lot of effort working with us, like TWI Welding Institute, for example, and I know Ron is well-aware of this. But, we are second guessing everything we do all the time including the weld process that we're looking at. There are new state-of-the-art welding processes out there where the welds are, you know, extremely thin compared to the conventional welding processes. And, we're looking at these because, frankly, the smaller weld zone, the smaller the heat affected zone, the lesser--maybe they're still there, but you minimize perhaps the impact. So, we're looking at a lot of different avenues, I think, as look into the future.

CRAIG: Okay, thank you. That was a good exchange, excellent exchange.

Carl Di Bella followed by Mike?

DI BELLA: Thank you. This will be brief. Could you put back up Slide 36 of Joe Farmer's talk? My question will be brief; I don't know about the answer. This 36, this has to do with the brown bar at the bottom. The brown bar says 0 to 1 percent frequency, but isn't that based on the binning procedure for seepage waters that was explained to us earlier in the day? That only extends up to maybe 110, 120 degrees Centigrade max. Above that, the environments are going to be based on, more than likely, dusts on the waste package and what their composition is. Or do I understand something wrong?

FARMER: No, no, I think you've got it right and actually it's a very good point. At the higher temperatures, let's say, above the boiling point up to the deliquescence point for calcium chloride, let's say 150, 160, kind of--frankly, it's a little bit difficult to quantify the deliquescence point precisely--but let's say 150, 160, between that boiling point and that deliquescence point, you could have deliquescent brine formation. We have looked at the corrosion--or the susceptibility to corrosion in what we believe today to be one--perhaps not the only worse case chloride salt because, frankly, you have calcium chloride and you have magnesium-chloride and both of those are divalent cations which have similar detrimental effects on materials, but we have, in fact, looked at corrosion underneath these deliquescent brines in those high temperature regimes from the boiling point up to the deliquescence point. What we see, generally speaking, in regard to Alloy 22 is we do not see the same types of localized corrosion underneath a deliquescent brine that we see in what is, in fact, an aqueous solution where you have the possibility of convective stirring and all kinds of other things that can happen.

Now, as we go below the boiling point and we get into solutions that I would say are more typical of what most of us think of when we think of aqueous solution, you know, things that are actually liquid, in those scenarios we have, if we're below the boiling point, but above the threshold temperature for localized attack, we could have problems, I believe. And, again, these are observations. You know, we look at the samples and tell you what they say.

DI BELLA: This is the second time you've brought that up today. Would you say a little bit about the apparatus in which you do that kind of observation and some more about your experiments with magnesium-chloride brines?

FARMER: Correct. Again, I'm going to remind you that, as we said before, these are Greg Gdowski's experiments. Greg is the real expert on this, but Greg does an incredibly good job of running these. It's a very unique capability we have. He hangs these samples in the thermogravimetric analyzer. It's an environmental TGA. He's capable of heating the samples, hanging from a quartz microbalance. He's capable of heating those samples up to relatively high temperature. And, we can actually take the TGA above 150 degrees Centigrade, but frankly, Greg doesn't like to do that because he risks damaging the instrument. But, we routinely make measurements at 150 degrees Centigrade, very close to the deliquescence point of the calcium chloride. Under those conditions, he monitors weight change. As we showed you in our data, you can see the absorption of water, you can see the thermal decomposition of the chloride deliquescence brine and you can see that eventually stabilizes, and there's no further weight change of any significance. This is with the resolution at 10 micrograms.

DI BELLA: What you're showing is how an environment is evolving under those conditions, not how corrosion is occurring.

FARMER: No, we're also showing corrosion because in addition to quantifying the deliquescent brine formation--and the nice thing about having the mass change data is you can observe the deliquescence process occurring. We also collect the sample from those exposures after many weeks to many months and we look at the surface. We look at the surface with optical microscopy. As Mark showed you, the project goes in, they use Raman spectroscopy to try to identify crystalline phases that occur on the sample. We do EDS to try to get elemental composition. So, a lot of work goes into looking both at the deposit that forms, the deposit that's put there intentionally, as well as the corrosion that occurs underneath. What we have seen, thus far, you know, and again this--I have frankly absolute confidence in Greg's data and I'm looking at this as an observer, but I have observed him and I've got absolute confidence in what he does. He's not seeing any localized attack of the Alloy 22. Now, we have had Alloy 825 as a candidate material before. That material does undergo localized attack. So, I believe that there's a big difference between these deliquescent brines, in terms of the types of negative impact that can occur, and a true liquid phase electrolyte where you can have convective mixing, the more normal transport processes.

DI BELLA: And, you're run the magnesium-chloride, too, you say?

FARMER: I'm sorry?

DI BELLA: You've run magnesium-chloride, too?

FARMER: There have been some tests done with magnesium chloride, but to be quite frank with you, I don't know the extent to which magnesium chloride has been tested in the TGA. So, I'll have to get back to you on that.

DI BELLA: Thank you.

CORRADINI: Can you go to the next slide since it's up? Corradini. All right. So, I guess, I have two points to--

FARMER: Oh, actually, I'm sorry, one followup. Mark just made a very good point in regard to Carl's question that I had overlooked or forgotten to mention. But, frankly, most of these brines have relatively little magnesium in them. So, the calcium chloride is the more relevant of the two cases, we believe.

CRAIG: A followup question. Carl?

DI BELLA: I can't let that go, I'm sorry. The brine that you're going to get is what deliquesces first. The compound with the lowest deliquescence point is magnesium chloride. Even if it's present in small amounts, it's going to come out first and you're going to have it. Now, you may argue it away for some other reason, but I think you are going to have it.

FARMER: Well, actually, there is--I do not foresee any situation--you know, I'll be the first to admit I can be mistaken. But, sitting here before you today, I can't think of any situation where you're going to have pure magnesium chloride on the waste package surface. I think you're, most likely, going to have mixed salts on the waste package surface. I think those can deliquesce at relatively low relative humidities, but I think they're going to be much more complicated electrolytes than pure calcium chloride or pure magnesium chloride.

PETERS: Carl, and if you go back to the dust deliquescence calculations, it's not clear to me how your line of questioning flanges up with that.

DI BELLA: I'm talking about rewetting as opposed to evaporating concentration. I think the answers are different.

CORRADINI: All right. So, we're on 36. So, I'm looking at the graph and not being a corrosion expert, it looks to me like there are two mechanisms. And, I asked you this privately, Joe, but I guess I want to get at it. From temperature 60 to 90 or 95--it looks like 90 to me--we seem to have a plateau whether it's red or blue, and then at 90 we seem to have a different mechanism occurring. So, my question is twofold. One, I think you agree with that, and two, it goes back to Ron's point which I think is actually very well put is that if you're seeing something and it sounds like between the corrosion shop talk that I see in front of us, that something is occurring there. If you understand the mechanism, you can go back and hopefully improve the performance because the spread--the only place I see spread experimentally is exactly at that point which implies to me a physical mechanism change. So, are you all right with that interpretation of that graph or am I off-base?

FARMER: No, I do not think you're wrong--

CRAIG: You need a--no, Mike, Joe, you've got to--yeah, right there. That's fine.

FARMER: Yeah, I think that's a point well-made and I had to get up actually to see the data. But, you know, there could very well be a mechanism change there.

CORRADINI: Okay. So, that's Point 1. Point 2 is what Ron--I'm taking Ron's point because I think the way he's laid it out in terms of possibilities of action are (a) you may have a problem, what is the problem, try to understand it physically or scientifically, and perhaps fix it; (b) probabilistically, Bill was making the point, if your claim it's 0 to 1 percent so a small amount of water--so let's take 1 percent to be on the high side--so 1 out of 100 of these drips since we're talking seepage get to the point where there is an infected material which is a weld and it fails. What is failure and what does that mean in terms of radioactive transport because I think when I hear you talk about it, I hear that failure is inherently radioactive transport. Is that correct?

FARMER: Yes. I think, frankly, we're looking at the repository and Bill Boyle is probably the most appropriate to give the best answer, but, you know, frankly, we don't look at any of these systems in isolation. You know, we put all these things together and see how they function together and what kind of dose they give at the site boundary and that's the real--

CORRADINI: Right. So, let me just restate my question because I'm leading to my third choice on this. One was to fix it and that's what Ron said. Secondly, I think he said it better, carry out the calculation and see where it leads you. And then, just from a probabilistic standpoint, if you guys are claiming 1 out of 100, the water may look like this and it may seep, then it's 1 out of 100 packages will experience this which means they'll have 1 out of 100 events. My question is what is the result of that event because, if I understand correctly, it's localized corrosion which means at a weld place or something such as that? Does that automatically lead, based on modeling, to radioactivity release?

FARMER: No, it doesn't. A very good point. And, actually, hopefully, I know you're hearing about the science and technology program tomorrow and I think these are the very issues, as I understand it, that the science and technology program are supposed to deal with because, frankly, you know, a lot of us who have been on the project for many, many years look at this very much like an engineering project. We have sort of--maybe we didn't start out this way, but this is how we've evolved. But, I think that with the new science program that's starting, I know that there is a lot of interest in understanding localized corrosion phenomena. In fact, there are meetings that are planned right now. And, I believe that in addition--you know, we have only talked today about the initiation of localized corrosion. People who are far more experienced than I am, you know, are very interested in using--looking at things such as stifling of these localized corrosion phenomena. Just because something starts doesn't mean that it happens, you know, indefinitely.

CORRADINI: Okay. All right.

FARMER: And, those are things that are going to be looked at, I believe.

CORRADINI: All right. And then, my final question or point is--and again you guys tried to provide us a story from the point of it got hot, where did the water go, where did it come back, what's the water look like chemically, what does it do to the waste package? So, my question is now--let's just take my cartoon picture--I fail it locally. What's the transport mechanism at these temperatures to get the radioactivity out? We haven't heard that part of the story.

FARMER: Right. And, Rob Howard actually setting up a mike. Rob can probably shed some light on this.

HOWARD: This is Rob Howard, Performance Assessment, BSC. If we do have localized corrosion and, let's say, we breach a waste package. We characterize what's the size of the opening? Is it a pinhole crack through seven inches of metal or is it a larger opening? We've got to get either water in and water out to have an invection release or it's just going to release radionuclides by diffusion. Bo pointed out that, you know, if we have diffusive releases only which would be likely in the scenario, presuming that it's on a weld on the lid which means it's on the side and not on the top, you're going to get diffusive releases of technetium and iodine, predominately, and maybe Carbon-14. Those kind of releases are orders of magnitude below what you would see as far as a medium standard and transport if you had wholesale dissolution of the waste package.

CRAIG: I wonder if I'm missing something about these Bins 1 through 3. There's an implication going through this conversation that all the other waters are totally benign. Does everybody agree with that?

FARMER: Well, everybody; I'm not everybody.

CRAIG: Do the waters people agree with that?

PETERS: Well, I mean, you--corrosion is the key. But, well, yes, I think, is the answer. I'm speaking for myself, but, yes, they are benign. And, I said 1 and 2 are zero from the crown. I wasn't saying 1, I said zero.

FARMER: Actually, not to cop out on the answer--Joe Farmer again. So, I did say my name once. It turns out that, you know, we've done a lot of testing over the years. I mean, going back to the late '80s, we've tested a variety of materials, these and others. And, all these bicarbonate waters, you know, we've been criticized, frankly, for using J-13. Well, in the early days, that actually was a pretty decent estimate of the types of--at least, gave us the collection of ions that you expect out of the mountain. And, frankly, as we move forward and we've prepared synthetic versions of those waters and seen how those waters can evolve as we evaporatively concentrate them. We see that they do, in fact, move to the corners of the triangle, if you will, so that we are bounding the problem. And, the bicarbonate solutions tend to be very benign.

Frankly, the sulfate brines are even less problematic in my estimate than the bicarbonate brines because, if you remember from the polarization data we've shown you, there's this anodic oxidation peak in the alkaline bicarbonate brines. And, just like you don't want to hit potential where you have pitting, frankly, you want to not pass the potential where you have that anodic oxidation peak.

So, you have actually more margin in the sulfate type brines, I believe, than you do in the bicarbonate.

CRAIG: Terry, I'm going to stay with our list. I've got to put you--let's see, exactly on this subject. Go ahead?

CERLING: Well, yeah, this is actually a follow-on to your question really. And, that is if you take Zell's 55 dust samples and you look at sort of the eutectic water composition that would form during deliquescence, you get--what sort of water composition is that and does it have a fairly restricted composition?

PETERS: For deliquescence, dust deliquescence as opposed to dust leachate, let's see, I can't remember which slide number it was, but there was a table--

SPEAKER: 32.

PETERS: Thank you. Yeah, the one we talked about at length, right?. Okay, that's in mine, Denise. I think what we're about to show is a graph that shows where the four representative--what sorts of compositions they evolve to. And, they're nitrate--I'm pulling from my memory, but they're nitrate chloride type brines.

SPEAKER: 32?

SPEAKER: 32.

SPEAKER: 32, yeah.

PETERS: So, these are the results of the EQ3/6 simulation.

CERLING: I guess, my question would be more if you took Peters'--oh, Cerling, Board--if you took Peters' Slide 30, the back two--sort of, I mean, here, you just have a two component system and you can see that the eutectic point is very well-defined and I was just wondering if you had--if you took Zell Peterman's data to see what sort of eutectic--what would be a eutectic mixture of the first water to form during deliquescence, what that would look like and if it's very variable between all of his different dust compositions.

PETERS: Good question. I'm not going to be able to answer that. We can get you an answer. That's going to be a level of detail that somebody like Tom Woolery is going to have to address. I just don't have that at my fingertips.

CRAIG: Okay. Let's go to Dave Duquette and then Norm.

DUQUETTE: A couple of brief questions. Joe, I'm not surprised that the 95 degree data don't show very much in the way of corrosion. That's basically testing a low temperature operating mode, is it not?

FARMER: Frankly, I wouldn't view that as testing a low temperature operating mode. I would view that as testing at low temperature because, frankly, you get into a low temperature operating mode and there are many things that come into play that we haven't accounted for here today, for example, the microbial growth. No, my personal view is the

MIC is going to be much more as the low temperature.

DUQUETTE: Sure, I understand that. But, it's just that the 95 degree seems to be, as we pointed out already, sort of a threshold where things seem to happen versus some other things that don't.

FARMER: Right, that's correct.

DUQUETTE: Now, if I understood the binning process and perhaps I didn't, but it was based on samples of water actually taken from the repository and in dividing them into various concentration categories. Am I correct on that?

FARMER: I will try to answer it again. Here, you're getting it secondhand. The materials, I understand, either from a firsthand basis or I'm close enough to the problem, I think, to speak as an expert, and this, you're getting my version of what people had told me they did and I believe they did as they said. I think they began with these pore waters, the five representative pore waters, and they used those as inputs to Bo's calculation. And, Bo will have to describe that for you. But, when you get down into the binning process, I think they're using EQ3/6 to actually try to predict how those waters would eventually evaporate and go through this chemical divide theory that Mark described for you. And, based upon where they end up, you know, which corner of the triangle they end up at, they then--and that's an oversimplification because, frankly, we use these triangular diagrams, frankly, just to try to communicate with you today on these, frankly, fairly difficult topics to try to discuss. But, they basically try to predict where those waters will end up if they undergo evaporative concentration. But, they don't stop and use that water composition after that initial evaporation. They go back and capture the concentration they started with, and then based on the outcome of that simulation, they bin that initial starting water. Then, that is then later in the simulation evaporatively concentrated or, at least, simulated using EQ3/6.

DUQUETTE: That's what I thought. And so, when you have 0 to 1 percent of water that's low in nitrate and high in chloride, is there a possibility based on that that 100 percent of the containers will see 0 to 1 percent of that kind of water versus having only 0 to 1 percent of the canisters seeing that kind of water?

FARMER: Exactly. That's an excellent point. If you remember in the bar charts that I showed you and I'll have to look to see what number. I actually went into some of those simulations. Again, I mean, they're not my simulations. I, frankly, was doing very much what you're doing now just trying to understand and quantify what I had because I'm a user of this data. So, I actually went in and looked. You'll see that there's both the initial Bin concentration plotted in those column charts. I think it's Slide 43, 43 and 42. But, if you look at Slides 42 and 43--I'll wait until Denise has a chance to get it--

SPEAKER: For your presentation, have you got the right--

FARMER: Yes, in Farmer's presentation, 42 and 43.

PETERS: Mark Peters, BSC. I'm not sure if this will help or not. What we showed was the crown waters that could potentially seep. It doesn't mean that they all seep. So, it ties back also to what drifts actually see seepage. That's a component of it.

DUQUETTE: No, I under--Duquette, Board. I understand that. I guess--

PETERS: No, I just wanted to clear that up.

DUQUETTE: I guess what I'm trying to sort out is is there a finite possibility that 100 percent of the containers will see this 0 to 1 percent of that particularly bad water, in which case you've got a much different problem than just addressing the fact that you have 0 to 1 percent of that kind of water present?

PETERS: Bo will correct me if I'm getting off track here, but I guess that's what I'm trying to say is it's a very low probability that it occurs in the crown and then all the drifts don't seep seepage anyway. Therefore, 100 percent don't see that kind of water composition.

CHRISTENSEN: Can I interject? This is one of the questions--

CRAIG: Okay. Your question is next. So, it's perfect timing. Go for it.

CHRISTENSEN: Well, this on the same thing. I guess that I had a hard time with--Christensen, Board--with understanding this time integrated relative frequency. Are we looking at probabilities through time or space or both? And, I'm just not sure if at any given time, the probability is 1 percent or if it's 1 percent integrated through time, and if so, what time--it seems like, particularly in this time sequence, this graft here, the actual frequency distribution of a given water at a given time might be different, but I may be misunderstanding that.

PETERS: It's integrating time and space.

CHRISTENSEN: So, that means then that the actual--at a given time that the actual frequency distribution in space might be different. Is that fair?

PETERS: Mark Peters, Los Alamos, shaking his head up and down, yes, that's true.

CHRISTENSEN: Okay. Yeah, I think that's part of the confusion here is knowing that--it seems then it really matters when, what is hitting the packages at what frequency.

SPEAKER: Fully agree.

PETERS: I would also, I guess, point out and Bill's already made the point, I think we're out here doing the risk assessment on the fly here. I mean, this is going to be part of the total system performance assessment that goes forward and I'm personally going to express a little bit of nervousness that we're here doing the risk assessment real time when guys like Rob are going to go back and do the real thing. So, I guess, I would--you know, let's be careful about how far we take this on the record.

CHRISTENSEN: Well, that's why I asked the question. I mean, because you've expressed a lot more certainty about what that meant.

FARMER: Yeah. And, let me--if I could get back to Professor Duquette's question which I think was quite a good one, I actually went in and pulled out the numbers--I have the simulation files or the outputs from these files and I pulled the numbers to make this chart actually from the starting composition for Bin--I can't exactly see the chart from where I sit, but you should see Bins 1 through 4 up there and I'm assuming it's a nitrate chloride ratio. What we did is we went in and we captured the kind of starting nitrate chloride ratio and the end nitrate chloride ratio. Frankly, I was a little bit surprised myself because many times you actually see the nitrate chloride go up due to evaporation. I mean, not always, but in many of these cases. And, I think the simulations are good. It's just that these are very complicated electrolyte systems and sometimes your gut feel or how you think it's going to work out isn't right or your gut is wrong because it's a much more complicated solution than you can guesstimate based on maybe your experience with a simple binary electrolyte or maybe a very simple ternary electrolyte.

CRAIG: Okay. I have three more people on my list at this stage; Leon Reiter, Dave Diodato, and--

CHRISTENSEN: I have one more question if I can.

CRAIG: Oh, sorry, Norm.

CHRISTENSEN: Quickly, Bo, this is for you. It relates to seepage which I think is very important in your argument and it relates to just the one bar graph that you showed that was relating percent seepage against percolation. I'm not sure which number it is, but it's--actually, it's on Slide #8. I wanted to understand that your argument that seepage is relatively unimportant seems to be based really on two pieces of evidence. Part of it is this set of bars where you show the relationship between percolation and percent seepage down to about 8,000 millimeters a year and then your general observation that we don't see seepage. I'm not sure--if you can help me, just how confident are you about the data setup in which these bars are based? I mean, is that really representative of the ESP, in general? Those were fairly confined areas where those studies were done, isn't that correct?

BODVARSSON: The seepage testing has been done in quite a few niches and, first of all, you have several boreholes that have many different packed up intervals where you actually can do seepage tests. The heterogeneity of the formation has been characterized by air permeability tests that shows that they vary greatly just like the whole mountain does in terms of heterogeneity. So, we have done the seepage tests for many locations under very different hydrological property conditions, if you will. In spite of all of that, the results are very, very uniform in terms that the seepage thresholds seems to come through pretty much the same in almost all of these tests and they are basically a thousand millimeters per year in the (inaudible) lithophysal and maybe even 2,000 millimeters per year in the lower lithophysal. That actually has more fractures.

So, that if you look at the statistics of the tests and the number of tests and if you look at the statistics on the heterogeneity, I think you will convince yourself given the fact that the results are so uniform that we have a pretty substantial database to actually make the statements and have a confidence in the seepage threshold.

CRAIG: Okay. Leon?

REITER: Sort of an overlap of some of the other questions. Sorry, Reiter, Staff. Joe, you made the statement and I wrote it down in my notes. It said that what percent of waste package failure is unacceptable?

FARMER: I said I will--and, you know, in due respect, if I said that, I was only--I don't--frankly, I don't think I know what percentage of waste packages are failing because, as Mark pointed out and I think appropriately, you know, we have professionals in risk assessment like Rob Howard and others who know how to do these problems and Rob can maybe address that.

REITER: So, if you said it, you said it. If you did say it, it turns out it's not you're trying to retract it?

FARMER: Well, first of all, I would--if there was a Court reporter, maybe we could check the transcript. I don't really recall saying it that way, but I would like to check and see.

CRAIG: Rob, do you have a number for us?

HOWARD: On what's the number of waste packages that's acceptable?

CRAIG: Yeah?

HOWARD: I'd have to say it depends on the failure mode. Right? If it fails by stress corrosion cracking or localized corrosion, it's a pretty high number if--you know, the worst thing that we've seen in the total assessment performance assessment is a waste package interacting with hot magma. That's bad. But, if it's localized corrosion and it's, you know, a pinhole in every waste package, that's not an issue as far as regulatory compliance goes.

CRAIG: Okay. So, what we mean by failure is complicated?

HOWARD: Yes.

CRAIG: Yeah, good. All right, Dave passes. You're up, Ron Latanision.

LATANISION: Latanision, Board. First, Mark, I agree entirely. I mean, this should not be about risk assessment on the fly and I think your sensitivity is well-placed. But, you know, I will say that in my term as a member of this Board, this has been one of the most instructive conversations that I've been a part of. Frankly, I think the candor both from the point of view of the Board and the project is really, really healthy. So, despite whatever uneasiness we may feel, I think this is really an important and a good conversation.

Now, having said that, Mark, I want to turn to--

PETERS: I thought you were going to ask Bo a question.

LATANISION: I want to turn to the slide which number I can't find that showed the crown, the environments--

PETERS: 18.

LATANISION: And, I'm interested in the degree of confidence that you--when Paul asked the question do we feel confident that all of the other environments are benign or something to that effect, you were very emphatic in your answer to that question and I just want to ask it in the context of what the basis for that is. And, let me just continue by saying that if I look at that table, it's very clear that 4, 9, and 11 are the majority members in terms of their population. And, it happens that 4 has apparently a nitrate presence, as does 9, and 11 has a carbonate presence. And so, in a way, I guess, I wouldn't be too surprised that they're relatively benign. But, if I look at #5, for example, which has a chloride population and no nitrates, I wonder what evidence there is to suggest that #5 which has a 10 percent frequency is also an effectively benign environment?

PETERS: Yeah, I was basing that on my knowledge from hanging out with this guy here about what the material susceptibility is in the different--where our test data is and what the material susceptibility is in the different chemistries. So, that's where I held it up to Joe and said that's more of a corrosion issue.

LATANISION: Latanision, Board. Joe, if you can tell me that, you know, you've done some testing in that environment and you see no evidence of localized corrosion, then I'm satisfied.

FARMER: Yeah, actually, if we could pull up Slide 20 of my presentation, it's exactly like the same table--

SPEAKER: It's like his.

FARMER: Yeah, it's the same table as Mark's except it has an additional column, I believe. Yeah, Slide 20 out of 43. Right, this one.

The first, let me see, one, two, three, four, five --the first five columns are identical to the table that Mark showed in his presentation. In fact, that's where I got the table. What we did is we took the solutions that we've tested in and you see them in that last column. We had our geochemists take those test solutions and bin those for us. They told us what Bin, what representative Bin, those test solutions fall in. So, in the cases of Bins 1, 2, and 3, we found that those that are 5 to 8 molar calcium chloride tests with and without nitrate fell to a greater or lesser extent in those various three Bins. Those are kind of the representative Bins for the calcium chloride. And, frankly, not terrible probable, but that's where those test solutions fall. And, that's where we've been spending a lot of our time lately. We then went back and looked at some of the earlier worst case solutions that we tested and that we thought were worst case at the time, the simulated saturated water, SSW, which is an extremely concentrated brine that has a boiling point of around 120 degrees Centigrade. We tested in that solution and we also tested in the SAW solution that you're well-aware of. Those test solution when they were binned fell in Bins 4 through 7. Then, the others that we've used over the years, SDW, SCW, and BSW, fell in Bins 8 through 11.

So, based upon the types of test results that we've gotten over the years from those various test solutions, this is the basis of probably my comment to Mark. We found, as you saw in some of the charts today, fairly good performance in SSW and SAW. For example, the SAW tests, you know, show a very large potential margin before we get right down to the passive film; probably the largest of any solutions that we've tested in. They simulated saturated water despite our concern--in the early days when we first formulated that, you know, it had a boiling point of 120 degrees Centigrade. We were thinking when you begin testing in a near-saturation environment at 120 degrees Centigrade, you know, maybe you were going to have some problems. But, as you saw in that one polarization curve I showed you today, we tested up to 120 degrees Centigrade without breakdown of the passive film. So, I would say that that certainly isn't as aggressive as those calcium chloride solutions we showed you.

LATANISION: Joe, just to follow that up, though.

FARMER: Sure.

LATANISION: I mean, if I look at those solutions that are clustered in 4 through 7, I mean, three of them have nitrates. Do--

FARMER: Oh, you mean--oh, I'm sorry--

LATANISION: I mean, I'm interested--

FARMER: Oh, 4 through 7, yes.

LATANISION: Right, I'm interested in #5, for example. Do either SSW or SAW--are either of them nitrate free?

FARMER: I have that data. Frankly, I can give you the exact compositions of that electrolyte, but I have to look back at a table to give you a milligrams per liter, whatever units you want.

LATANISION: Okay.

FARMER: And, I have that data on my laptop if I can get my laptop.

CRAIG: Okay. At this point, it's time to call the session to a close. First of all, two observations. No? Time, Richard, sorry. Yeah, well, you'll be faster with your hand next time.

PARIZEK: Not fair. I get the first question tomorrow.

CRAIG: First question tomorrow, okay. I don't know who the chair of it is, but you owe him one.

First of all, thanks to the staff for scheduling all this time for discussion. I really worked out very well and we should probably learn something from that. A great discussion. And, secondly--or, actually, first in importance, I found today's presentations to be the most compelling DOE presentation since I've been on the Board.

They were really wonderful presentations. You guys put a lot of work into it and it shows. It really does show and I want to thank you for it.

PETERS: Thank you very much.

FARMER: Thanks, Paul.

CRAIG: So, at this stage in the game, I think it's time for the public session.

CORRADINI: Okay. Thank you very much, Paul.

We have one speaker, Judy Treichel. There she is, I'm sorry. Judy?

TREICHEL: Is this on?

CORRADINI: Should be on.

TREICHEL: Okay. I know it's not proper to start out with a complaint, but in the interests of time, I think it's a mistake to put all of the public comment at the end of the session. You were in the midst of a great go-around here. You were all jazzed up. You were all part of it. And then, it had to cut off and now go to public comment that goes back to the beginning of the day. So, I feel like an irritation at coming up here at--that's sort of lousy at this late, late hour. But, I'll go ahead because I'm already here.

One time when I came to a Board meeting, my daughter made me a graphic and it was Alice in Wonderland and she had on a virtual reality thing on her eyes and it was because Alice had gone through the looking glass and then she'd actually entered another dimension and that's about how I was feeling because things got so loony and I've seen a part of that here. And, part of it was in talking about one of the real dangers of the cold repository.

Getting off of all of the scientific stuff you're talking about was to go back to the EIS and it would result in traffic accidents. And yet, we, in Nevada, are having a hard time of convincing people that the nationwide transportation of nuclear waste with thousands and thousands of packages going across the country is a danger. It just seems as though some of this stuff gets into the realm of the loony and I wanted to sort of point that out that it's a really hard argument to sell the fact that people want jobs and they'll travel out there to go to those jobs. And, actually, this program was first tried to be sold as a JOBS program.

So, getting away from that, the other thing that bothers me and it has for many, many, many years, for about 10 years, I tried to get people to drop the word "stakeholder". I finally gave up just because I got tired. I never thought it wasn't important. It's very important and now it's coming back again and I'm going to make the pitch again not to use the word "stakeholder". We're always just about this close--and for the record I'm holding my fingers about a half inch apart--to having stakeholder involvement.

The program is more than 20 years old or around that time, 15 to 20 years, and we still haven't had public involvement. Nobody has done a decent job of ever defining what a stakeholder was. I think Ginger King many years ago said it was somebody who had a vested interest and a money interest in this program. And, that may be true, but that certainly leaves out the public. To think that the public gets involved because of something that has to do with cooperative agreements is berzerk. That's a working relationship. Those are people working for the project, cooperating with the project, and that has nothing to do with the public.

So, as we're coming down to things that are as important as other things where there should have been public involvement, don't use the word "stakeholder". It just plain doesn't work. Either the public gets involved in transportation or in licensing or in whatever we've got to go here or they don't. And, I would bet probably that they don't.

Quality assurance hasn't had a big involvement or what it should and it still isn't there and it wasn't there for site recommendation. We had a schedule and we beat that schedule to death and they got a site recommendation and they rammed it through, but it beat out the quality assurance. So, there's always just this sort of chase. And, last week while we were at a technical exchange, we were hearing that the work on time dependency hasn't been done yet. Some of it hasn't even been started yet. The data is very, very preliminary, but the Department of Energy is talking about putting together a top notch license application. It's outpacing the sort of work that should have, I think, been done during site characterization. So, I think the Board needs to be aware of that and keep that in mind when you're discussing these other things.

I would just finish by saying that I've got to disagree seriously with Joe Farmer when he made the statement, "We'll never know everything about the materials or about the repository, but we have to build it." And, he warned people not to dwell on dire possibilities. Well, I would say that if we're ever going to consider the bad possibilities, we'd better do it now. And, I would also like to add that we don't have to build the repository. We never did, we don't now. Certainly, my hope is that we never will.

Thank you.

CORRADINI: Thank you, Judy. I was incorrect and I apologize. There is an additional person with public comment, Kevin Camps from Nuclear Information & Resource Service. Kevin?

CAMPS: Thanks. Thanks for this chance to make a brief comment. I just wanted to say a thank you to the Board. I haven't been able to attend meetings recently going all the way back to before the Yucca Mountain vote. The Board's report that came out before the Yucca Mountain vote was very important. Our organization, Nuclear Information & Resource Service, represents people who live near Yucca Mountain, people who live along the transportation routes to Yucca Mountain, people who live near where the waste is at now, and the public had very few places to turn to in that highly politically-charged lead-up to the vote for objective information, trustworthy information, and this Board was one of the few places where the public could turn for that kind of information. It was very much appreciated. I just wanted to convey a thank you to the Board for that.

One of the things that has really concerned out membership for a long time and I did speak before the Board at a Las Vegas--actually, it was a Carson City Board meeting. The summer of 2000, I think, it was, is the whole transportation issue that time and time again has been shortchanged and deferred into the future. And, I would really encourage the Board to begin to really address that issue as a very important priority because the other agencies involved are finding it very convenient not to do that. I missed this morning's presentation by the Department of Energy representatives, but again it seems like a transportation plan is going to be put forth with very little involvement of the people who live along the transportation routes who would be most affected, most affected by this decision.

One specific example of a subject area that needs a lot of attention is the issue of damaged fuel in the United States. I once heard a presentation by Bill Lake from DOE at a PATRM conference, Packaging and Transportation of Radioactive Materials, in Chicago in 2001 where he said the Department of Energy doesn't have much experience transporting damaged material. It seems to be an issue all over the country. There's damaged fuel at West Valley, New York that could be transported anytime between April 1st and October 1st of this year. That's not the only place. There's damaged fuel all across the country at reactor sites, elsewhere. So, I just really encourage the Board to begin to address such transportation issues as that.

My organization and I know there's other organizations here in D.C., public interest groups, environmental groups, that have similar concerns about the transportation issues, the routing, and I don't want to speak on their behalf, but I'm sure that they would communicate their concerns to you for future meetings, as well as our group would do.

Thank you.

CORRADINI: Thank you.

I now think we have no other public comments. Any last comments by the Board or the Staff?

(No audible response.)

CORRADINI: Okay. I think we stand adjourned. We'll convene again tomorrow at 8:00 o'clock. See you then.

(Whereupon, at 5:59 p.m., the meeting was adjourned.)