UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

# FALL 2003 BOARD MEETING

September 16, 2003

Longstreet Inn
Amargosa Valley, Nevada 89020

NWTRB BOARD MEMBERS PRESENT

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

NWTRB STAFF

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

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P R O C E E D I N G S

8:10 a.m.

CORRADINI: Good morning. It's my pleasure to welcome you to the U.S. Nuclear Waste Technical Review Board meeting. 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 try to meet once each year in Washington to make it convenient for our federal government and congressional decision makers to attend, but most of our meetings are in Nevada to provide the citizens here with an opportunity to observe and question the material that is being presented. Some of our Nevada meetings are also in towns close to Yucca Mountain, such as today's meeting here in Amargosa Valley.

As many of you know, the Board was created in 1987 on amendments to the Nuclear Waste Policy Act. Congress established the Board as an independent federal agency to evaluate the technical and scientific validity of the activities of the Secretary of Energy related to disposal of commercial spent nuclear fuel and defense high-level 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 Sciences, and the Board is, by law, as well as by design, a multi-disciplinary group composed of eleven members with a range of expertise.

Let me introduce the Board members again to you today.  We do this every time, but as the audience and the locale changes, we want to make sure everybody knows all the members.  As I introduce them, I would like them to raise their hands to be identified.  Let me remind you that we all serve in a part-time capacity.  We all have other jobs.  In my case, I am Chair of the Department of Engineering Physics at the at the UW Madison, in Madison, Wisconsin.  My area of expertise is nuclear and industrial safety, with emphasis on subjects involving multi-phase flow and heat transfer.

Mark Abkowitz is 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 area of transportation, risk management, and risk assessment.

Can Bullen is 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 Distinguished Professor of Geology and Geophysics and 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 Professor of Ecology and former Dean of the Nicholas School of Environment at Duke University. His area of expertise includes biology, ecology, and ecosystem management. Norm chairs the Board's Panel on the Waste Management System.

Paul Craig is Professor Emeritus of Engineering at the University of California at Davis, and a 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 recently retired from his position as professor at MIT to pursue a senior position with an engineering and scientific consulting firm, Exponent. Ron retains a position as Emeritus Professor at MIT. His areas of expertise include materials processing and corrosion of metals and other materials in different aqueous environments. 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, Dick 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, let's turn to our meeting agenda today. First this morning, we will 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 will hear updates on two technical subjects we have reviewed previously: first, the performance of engineered barrier systems and, second, continuing efforts to determine whether the presence of

Chlorine-36 at the repository horizon has implications for the performance of natural barriers of a Yucca Mountain repository.

After lunch, we will move into one of the central purposes of today's meeting. You might remember, if you were with us last time, that at our last Board meeting, the DOE gave us a rather detailed presentation on seepage, waste package environment, and waste package corrosion--man made system. Today, the DOE will give us an analogous presentation on the natural barriers of the proposed repository. We expect to hear the DOE's views on how natural barriers are projected to contribute to waste isolation, resulting in a multi-barrier repository system.

Tomorrow, we plan to begin the morning a little earlier than usual. We'll start at 7:15. Board members, I think all know, but to remind them, will be available here for coffee and conversation with members of the public. The formal meeting resumes at 8:00 a.m. with welcoming remarks by Henry Neth, Chairman of the Nye County Board of Commissioners. Technical presentations tomorrow include performance confirmation, igneous studies, and transportation plans and activities.

And, so, let me mention a few important business items and logistics before we begin. First, the Board values public participation, so we have given the public a variety of ways to comment during the meeting. We have set aside time for public comment before lunch today, and at the end of the sessions today and tomorrow. So, three different times. 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.

Could you guys raise your hands so everybody knows? So, in the very back by the exit sign.

Most of you who have attended our meetings know that we try to accommodate everyone, but as you can see, as usual, 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 in the back, and we'll ask the questions if time permits.

So, now, I will offer the usual disclaimers. If you've been here--I'm a short-timer, three meetings--but for the record, we all know this, but I want to make sure we're all clear, so that everybody is clear about the conduct of our meetings 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 Board members speak quite frankly and openly about their interests and opinions. But I have to emphasize that when we speak, they're our own opinions, and we're speaking on behalf of ourselves and not on behalf of the Board. When we are articulating a Board position, we'll be sure to let you know, and usually Board positions are stated in letters and reports, and 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 off, since I don't think there's very much reception anyway.

And then, finally, for the speakers, I've been told by our staff over here to speak into the microphone. Get close and personal. All right?

So, let me start, first of all, the meeting by introducing Dr. Margaret Chu. She is Director of the Office of Civilian Radioactive Waste Management. Before her confirmation as Direct in March of 2002, Dr. Chu had over 20 years of experience at Sandia National Laboratories, ranging from research and development to program management. Dr. Chu will update us on the Program developments within the Office of Civilian Radioactive Waste Management.

Margaret?

CHU: Thank you. I really appreciate this opportunity to provide an update of DOE's Civilian Radioactive Waste management program. Before I start, let me first tell you how pleased I am to be here in Amargosa Valley. It's just absolutely gorgeous out there. I wish we were out there instead of here.

Let me give you a brief update on the organizational aspect of our office. Early this year, in January, I told the Board that a reorganization we have put in place to create an organization that is responsive to the needs of the next phase of our program, namely licensing, construction, and operation.

As a result of that reorganization, John Arthur, who you all know, came on board as the Deputy of the Repository Development in Las Vegas. Since the first week of August, we finally have a Headquarter Deputy, Deputy Director of the Office of Strategy and Program Development. We have selected Ted Garrish for that position. Ted is a lawyer by training and has had a long distinguished career in both the government, specifically mostly in DOE, and the private sector.

You probably remember the main feature of this reorganization is this dual-deputy structure. Now, with Ted based in Washington, D.C. and John Arthur in Las Vegas, the system really provides the leadership, flexibility, and management accountability that supports our mission.

Ted was planning to attend this meeting, but could not because of an international trip this week he was asked to attend with the Secretary. But, he asked me to relay the message he really hopes to meet the Board members sometime in the near future. I just talked to him on the phone this morning.

Now, we also have filled three other very important positions in the organization. All three are senior executive service positions. First, Chris Kouts, probably a lot of you have met, was appointed Director of the Office of Strategy and Systems Analysis, after having served as the Acting Director, and Joe Ziegler has moved from an acting position to the official status as the Director of the Office of License Application and Strategy. John Arthur later on will give you a little more detail on his background.

And, finally, I have selected Gary Lanthrum as our new Transportation Director. I haven't seen Gary this morning. Is Gary here? Okay. There's no hotel room in this hotel, so he had to stay at Pahrump. So, he's supposed to drive in this morning. When he gets in, I will introduce him to everybody.

Now, Gary Lanthrum came from DOE's Albuquerque field office. He was the Director of the National Transportation Program under DOE's Environmental Management Program. So, he has a lot of experience in the transportation area. And, tomorrow, he's going to give everybody an update on what's going on in transportation. But, he's only been here since the first week of August, so he's still trying to ramp up in his knowledge in the Program specific activities.

Now I only have one more position to fill. That's the Director of the Science and Technology International Program, and I'm already in the process of doing that. I hope to fill this last one in the near future, and then I will report back to you when that's done.

Now, at all levels, you can see that throughout the organization, we are aligning positions to the work we need to do, and staffing them with experienced, top-quality personnel.

Another new addition who will be of interest to the Board is Professor Joe Payer, a distinguished scientist from Case Western Reserve University, who has agreed to be my advisor on corrosion science on a part-time basis. Given the importance of corrosion in our technical basis, I am very pleased with this new addition.

Now, for budget, we are continuing to await a determination on the Program's Fiscal Year 2004 appropriation. I am pleased to report that the House side voted for a mark of $765 million, which is $174 million above our request of $591 million. In the Senate side, however, the picture is not as encouraging. Although the full Senate has not voted yet--I heard they're supposed to be voting this week--the committee mark is only $425 million. I hope that the House and the Senate will go to conference very soon so we will know what the budget will be.

After this fiscal year, 03's, $134 million shortfall, it is extremely critical that we secure sufficient funding FY04 to complete the technical work required for the license application and perform other essential work.

Our key goal for our Program remains the same, that is, to begin receiving waste at a licensed Yucca Mountain repository in 2010. To achieve this goal, the program must apply for a license, secure a construction authorization, build the repository and the surface facilities for initial operations, receive a license to operate a repository, and develop a transportation system to ship waste from civilian and defense storage sites to Yucca Mountain. The timeline for all these actions is very, very tight, as you know, but I believe it is achievable, given sufficient funding.

We are working toward our near-term target: production of a high-quality license application in December 2004. This depends on completion of the remaining technical, scientific, and design work, validation of that work through quality assurance, and compilation of the application itself. We plan to submit a license application that meets not only NRC's regulatory requirements, but also our own high expectations for quality. So, the feedback that we receive from the Board will help us ensure that we meet this high technical quality.

Over the past year, we have put in place several management improvements to ensure that our commitment to quality is reflected throughout our actions and products. We have focused on--I have mentioned that a little bit before--improving individual accountability for work, strengthening line management ownership of procedures and compliance throughout our organization, integrating our corrective action programs, measuring our progress systematically, and implementing management processes to manage work and resolve issues. John Arthur will go into a little bit more detail on these.

I have described our ongoing improvements in these areas in a letter to NRC on May 29th of this year. These actions will better position us to be a successful NRC licensee, and to meet mandated requirements for a safely operating repository. Again, you will hear more about it from John Arthur.

Another important step forward in our approach is our approach to addressing Key Technical Issue agreements with NRC. For the past year or so, I have personally encouraged project staff to develop integrated technical stories. I call them stories. You know, the last TRB meeting in May was really a catalyst for our program to start developing these "stories."

In that meeting, you probably remember specifically we presented the relevant processes that are important to the in-drift environment. Since then, we used the "story" concept to pull related KTIs, Key Technical Issues, together and tried to address technical issues in an integrated, comprehensive fashion. We believe this integrated approach will facilitate NRC's review of the license application, and it will also help us communicate more effectively to a variety of stakeholders.

In fact, from our own internal review so far, we have found that these integrated KTIs are of higher quality and they provide clearer explanations. With this approach, we expect to submit a total of more than 200 KTIs by the end of this calendar year. We're really picking up on the KTI issues.

I received the Board's two letters following up from the May meeting, and I expect to provide a response within the next week or so to you. Sorry I didn't do that before this meeting.

I want to thank the Board for giving us many insights and excellent comments through these letters, especially in the seismic and igneous areas. We take these comments very, very seriously, and would like to discuss more with you in the future on these topics.

I understand the Board is also preparing another letter, focusing on our thermal technical basis. I look forward to receiving this letter. I encourage the Board to provide us with specific issues so that we can address them appropriately.

Turning to the agenda for today and tomorrow, I would like to touch on some of the technical topics that other speakers will address.

At today and tomorrow's meeting, we will continue using the approach from our last meeting, that is, we present our technical basis through the integrated "stories" that I mentioned previously. We feel it is very important to address our technical basis at the process level, but I also want to present the effect of these processes on the overall system performance perspective.

You will hear a presentation later this morning that provides the total system performance assessment perspective on engineered barrier. This is a follow-up presentation from the in-drift environment and the waste package presentations from our last meeting. There are still a lot of on-going work in this particular area, so it may be necessary to update the Board in future meetings on this particular topic. Later today, we will present our technical basis for unsaturated zone and the saturated zone performance.

In the area of performance confirmation, we believe we are making good progress, and you will be briefed on that. And we will also brief you later this morning on Chlorine-36, and later, on volcanism, and I look forward to your comments on both of these topics.

Tomorrow, Gary Lanthrum will provide a transportation update to you. In the past, we had to defer much of our planned transportation work due to funding limitations. However, we believe we are now poised to move forward on a number of fronts. Last year, the Secretary committed to issuing a Transportation Strategic Plan, and we are in the process of completing that document. We recognize the importance of interactions with states, tribes, and local governments in transportation planning, and we are now in the preparatory stages of activities in the institutional area. And, Gary will give you a briefing on that.

Finally, let me update you on some of the topics that are not on today's agenda, but that are very important to our Program.

In our budget request and Program plans, we have emphasized the importance of identifying opportunities to incorporate future technical improvements in the waste management system. I really do anticipate a significant science and technology program in Fiscal Year 2004. Projects in this area will not only help us take advantage of technological improvements, they will also allow us to realize cost efficiencies. You heard a presentation about this science and technology program in our May meeting, and we are planning to keep you updated in the future about this new program.

The next twelve months will be very critical for our Program. I believe we are on the right track and we are making good progress toward our goals, but we are confronting a lot of challenges, foremost among which are funding shortfalls, as you know. Our efforts to maintain momentum despite past reductions of budget have limited our ability to accommodate future cuts. Nevertheless, we have taken substantial steps toward developing a license application, and we remain fully committed to a high-quality license application. I am confident that, with adequate resources, the Program can accomplish this near-term work that is essential to our ability to reach our overall goal of commencing waste acceptance in 2010.

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

Thank you.

CORRADINI: Thank you, Margaret.

Questions by the Board? Dave?

DUQUETTE: Duquette, Board.

Margaret, if the House appropriations comes through at the low level you talked about, do you still anticipate a--

CHU: You mean the Senate?

DUQUETTE: I'm sorry. The Senate. Do you still anticipate a science and technology program?

CHU: You know, we have never encountered a situation like this before. It's kind of hard for me to predict what the outcome will be, but we have $765 million from the House, $425 million in the Senate. If we can get a mid point, that's our requested amount $591 million, and then definitely we will have a very good science and technology program.

Unless it goes way, way low, close to the Senate level, then the science and technology will be in jeopardy. But, I'm really quite hopeful there will be a good science and technology program, yes.

CORRADINI: Mark, and then Dan.

ABKOWITZ: Abkowitz, Board.

Margaret, is your program still accepting opportunities for changing the repository design prior to the design that's going into the license application?

CHU: I would say no, not in a significant way, because we are in the process of freezing we call it a baseline design. So, whatever the major changes down the road, we will have to have an amendment of sort to the license application. But, we'll be happy to hear your comments on specifics on design.

BULLEN: Bullen, Board.

You mentioned that you're going to have over 200 of the KTIs resolved by the end of this calendar year, and that you have incorporated the integration of the KTIs into a "story" process. Has that been an acceptable approach to the NRC?

CHU: Yes, we have been working with them, you know, continuously since last probably May, since May. What I said is we're submitting more than 200.

BULLEN: Okay.

CHU: And then I'm not sure how it will get resolved by the NRC, because it's really not under my control. You know, how it goes is really with combining related KTIs. So, they will be individually addressed, like before, up front, there will be an integrated "story" document to talk about these whole areas. So, they still can match agreement by agreement in the old way. What we're doing is integrating them together into a more comprehensive, overall technical "story."

BULLEN: Bullen, Board.

You touched upon the issue that I wanted to raise next, and that is how many of these KTIs do you expect to be resolved and how many unresolved at the time of license application?

CHU: You mean by LA time?

BULLEN: By LA time.

CHU: The license application time? Our plan is to address, probably try to address all of them. But the thing is there will be some continuing experimental work that will go beyond license application, and then that will be understood, there will be some kind of an understanding between NRC and DOE that these are the plan, these are the experimental plans. I hope I addressed it correctly.

BULLEN: Bullen, Board.

That's the exact follow-on question, because I was going to then ask how would the KTIs and their resolution feed into the performance confirmation plan, and do you see that as an integral part of the entire process of license application and continued science and issue resolution?

CHU: You know, performance assessment is a specific requirement as part of the license application.

BULLEN: Performance confirmation.

CHU: I'm sorry. Performance confirmation is part of the license application. So, we have to address that to NRC's satisfaction. And I imagine all these things will get integrated, so, if NRC, a high level of confidence, these are the remaining things and this is what we're going to do.

And, also, performance confirmation, my understanding, is a much more long-term thing, and it could be post-closure even. So, we have to address all these phases to give NRC confidence.

BULLEN: Okay. One last quick question because it follows on to what Dr. Duquette mentioned prior to this. Can you differentiate between what's going to be in performance confirmation for the KTIs and what may actually be addressed by the science and technology program? Or will nothing be addressed by the science and technology program because it's more out of the box type thinking?

CHU: Okay, my philosophy is science and technology, we have told ourselves is not part of the license application itself. Okay? That's clear to us. It really has two purposes. One is to think outside the box. That's one thing. So, down the road, what are the new technologies, and here's our whole waste management system. You know, it's more than the repository. It could be transportation. It could be anything, new materials, you know, advances.

And then, also, we would like to see what are the key uncertainties in the waste management area that people have encountered, not only here, but globally. And they are something that people have been struggling with for years and years, and when people put in an uncertainty that big, we're very interested to see how we reduce the uncertainty of those areas.

So, I can envision the science and technology program will have a relation with the performance confirmation program, or even down the road, if we have some new information during the license application review, if it's relevant, I think it should be presented as new information to NRC.

BULLEN: Thank you.

CORRADINI: Dick?

PARIZEK: Parizek, Board.

Margaret, at a previous Board meeting, you mentioned sort of a commitment to the saturated zone as a place where additional information could be helpful to the Program. Can you update us on your thinking on commitments to the saturated zone work?

CHU: Yes. There's actually quite a bit of work, that I know Bob Budnitz and the group have been talking to a lot of people, and then they have little workshops on the saturated zone. You're talking about the science and technology program; right? From that perspective?

PARIZEK: The saturated zone, and just as part of the framework for transport, radionuclide transport.

CHU: Right. You're talking about in the framework of the science and technology program; right?

PARIZEK: Or just in terms of continued concern in terms of just reducing uncertainty, whether it's done by the science and technology, there's a couple Key Technical Issues areas that sort of tap into that as well.

CHU: You know, there are a few areas that have been identified that we would like to pursue. One of the problems is we would like to do more field work, because we feel it's very important as part of the enhancement of the saturated zone. But we have a real problem. We have no water right now, because the State of Nevada will not give us water. So, this is something we are still trying to figure out what to do. We can do modeling and stuff, and we can do analogs and all that, but at this point, Bob Andrews, am I correct, we cannot pump water.

ANDREWS: That's correct. I would suggest that we save that for this afternoon, and we're going to talk about the saturated zone for a couple of hours.

PARIZEK: Okay, we'd be happy to do that.

The other thing is a number of the people that are in the Program have a WIPP experience and background, and it seems to me that there's a learning experience, perhaps for the Board as well, as to what is going on at WIPP, recertification is coming, and to visit WIPP to see exactly how that works. It gives us insight as to how you imagine perhaps the Yucca Mountain project would work.

CHU: You're absolutely right, and we are tapping into those people. So, there are a lot of lessons learned in that whole process.

PARIZEK: Yes. And for the Board to be upgraded on its understanding of that whole process, other than through the people that you brought into your part of the program.

CHU: Oh, okay, it's a good idea, yes.

CORRADINI: Priscilla?

NELSON: Nelson, Board.

I'm just going to make this comment because of the way the conversation has gone, and I think the Board has expressed the concern about the parsing of the scientific and technology efforts into maybe different time bins, maybe different organizational bins, regarding science and technology or performance confirmation or tasks that may be undertaken to support LA.

The sense of having there be integration of all the thinking and a direct ability expectation that information be in one place would be considered reflected and fed back into other parts is a concern, particularly when we've got this LA time, you know, a staccato, that changes focus. We saw some of this in some of the international meetings. I know a couple of the Board members went over to Sweden to a meeting, and it was sad that there wasn't anyone from the project to that meeting, because it was really interesting. But, I think the issue of integrating the science, for the sciences sake, and for the understanding intrinsically of the scientific processes underway, and the technological aspects of building and actually accomplishing what it started for is something I think all the projects grapple with. And it was really good to hear that kind of exchange.

So, I guess I'm not necessarily requiring a response, but just hoping that we can continue to see evidence of there being no walls in the technological flow and in the idea exchange amongst the different parts of the project.

CHU: I agree with you. Thank you. Yes, we are working on that.

CORRADINI: Other questions?

(No response.)

CORRADINI: Thank you, Margaret.

CHU: Thank you.

CORRADINI: Thank you very much.

We will have our second speaker. Over the past 24 years, John Arthur has served in several management positions within the DOE, including Manager of the Waste Isolation Pilot Plan, which we just were talking about, Manager of the Uranium Mills Tailing Remedial Action Project, and Assistant Manager of Environmental Operations and Services at the Albuquerque Operations Office. John is now Deputy Director of the Repository Development within the Office. And, so, today, John will give us an update on the status of the Yucca Mountain project.

John?

ARTHUR: Good morning. And, like always, it's a pleasure to address the Board. What a beautiful backdrop for this meeting today here in Amargosa Valley. I look forward to presenting a project update to the Board, and receiving your comments and recommendations as we continue on our challenges with the Program.

What I wanted to address to you all at the last Board, and I'll try to do it this time, is give a summary of, you know, accomplishments as well as issues we faced since that time. And, Margaret covered a lot of the organizational changes. Let me just add a few things, and then I'll get into specifics on license and the other areas of interest. But, I'm very pleased to have Joe Ziegler now selected as my License Manager. He brings over 28 years of nuclear engineering and licensing experience to this project. I was very pleased recently to offer him and select him for the position.

He has been with the Yucca Mountain Project since 1997, and prior to that, has worked Haliburton/NUS, Savannah River, as well as TVA. So, he's been involved in a lot of different licensing aspects in the past.

Additionally, at Bechtel SAIC, our main contractor for the program, a few changes. Peggy McCullough just joined the program. She will be in the Deputy General Manager position now to John Mitchell. So, she's doing a transition currently with Don Pearman, who many of you may have met in the past.

And then, also, I guess it was about two months ago, Mr. Larry Lucas joined. He came up from a Y12 facility in Tennessee, and he is now the Repository Design Project Manager. And, I'm going to talk a little bit about design a little later, and I want to maybe recommend the Board have a look at that, or have some discussions maybe at the January meeting. There's a lot happening.

I want to start first of all on some of our progress. If I can go to the first exhibit, please? In the last meeting, I talked, we're continuing work on the completed logic and schedule for our program. Currently, I have all the schedule assumptions, logic and planning, to open, successfully open a repository in 2010. And the schedule goes currently from 2003 out to 2011. It identifies the major activities, milestones, and decisions necessary to initiate repository operations. And here is just a sample, one section of the plan. It covers engineering and design for about a four year time frame, as well as surface construction, subsurface, waste package, and site development and operations. So, I just do this as far as giving an example, and I will touch on a few of these points when I get into design a little bit later.

Doing this has been a real learning exercise for me, and I know we have talked a little bit about differences between WIPP and Yucca Mountain, and we did something similar like this at WIPP back in I think it was around the early Ninety time frame. And, really, it laid out all the logic and assumptions, and we really promoted accountability and management to that effort, all subject to the right level of funding.

In the last meeting, I told you all that we are about 50 per cent complete. I anticipate we'll wrap this up by the end of this calendar year sometime. It's a major effort. It's really important for all of us to have Gary Lanthrum on board now so he can lay out the logic and detailed planning for transportation. And, once we get that all integrated, it will be ready to have it resource loaded. But, I'm really pleased because it's helped us all to look at really some of the risk and challenges, and if you don't identify them, you obviously can't manage them.

Next, I want to go to license application progress. Now, it's busy, but if you would, there's two back to back exhibits in the package, and one will be this one, which is from the May meeting, which is exactly the same I presented at the meeting back in Washington. And then I have one that came out of our most recent monthly operating review, which we held in Las Vegas here several weeks ago.

These monthly operating reviews are meetings that John Mitchell and myself hold to look at all the aspects of the license, design, and other key activities associated with the program. You know, license seems an area of major discussion, and we're also looking ahead at what it takes to successfully open in the 2010 time frame.

A couple key areas. If you compare between May and August, we have a high level at the top there that says Project Support, Licensing. Across, there's Repository Design. And we had two red in the May, and we're up to one red in August. Red means areas that we feel require significant management attention, and by that, there's either real challenges right now with either falling behind, significantly on schedule, quality issues, or in some cases, funding issues.

In the level two, we have approximately 39 measures down below there, if you look at those, and in May, we had eight in the red, and by August, three had gone in the red. Now, in the reports, it shows the details of what's going on in each of those areas, and as I told many of our managers before, I'm not so locked in on the colors as I am that we identify the issue, we know that the right managers are working that to get to the right desired level of quality.

A couple areas I want to discuss here. First of all, if you look to one of the areas that is still in the red is total systems performance assessment and progress in that area. We have had a number of improvements. I really thank Nancy Williams and many of the managers under her at Bechtel SAIC, as well as our federal staff. We were about 90 days behind on our schedule back in the May time frame, and there's been a lot of parallel action since that time, and as of to date, we assessed that approximately 50 per cent of the analysis and modeling reports that will be supporting the license are in various stages of completion. And that's been a lot of work. As of today, it shows right at about on schedule.

Obviously, in any major project of this magnitude, I like to see some contingency on the schedule, you know, maybe build a month or two of float into this, which we're trying to aggressively do.

The development of the Analysis and Modeling Reports for the TSPA obviously focuses on incorporating new information, improvements to the models, and data qualification and model validation. So, when we say something is complete, it also means that the right level quality of the models, all the data sets, and codes, other key areas, are appropriate and ready to go.

The technical agenda for this meeting as it relates to TSPA will cover the following discussions. Bob MacKinnon is going to update on the engineered barrier system performance as related to the performance assessment insights. And this will account for variability and uncertainty in the thermal-hydrologic-chemical in-drive processes in the modeling approach, as well as also examining the localized corrosion of Alloy-22 in detail.

Bo Bodvarsson will talk about Yucca Mountain unsaturated zone transport, presenting an overview of the conceptual, testing, and modeling bases for describing the unsaturated zone transport.

Bob Andrews will cover saturated zone flow and transport processes, as well as Jim Paces and Robert Roback on Chlorine-36, the validation studies. I recall a lot of discussion on that at one of the meetings, as well as internal of the project. So, we're anxious to show you where we stand on those studies.

Peter Swift will talk about the status of DOE's igneous activity studies, as well as providing a status of the igneous consequences modeling for the TSPA, including our response to the peer review panel and the Board concerns. So, we look forward to that discussion a little bit later.

Now, if we can move on. Margaret talked about by the end of this year, having 200 KTIs submitted. There is a lot of good work underway. We're real concerned, I know as well as NRC, they're about to probably go through the same challenge that our staff internal in Las Vegas has had over the last few months, and that's a large volume of KTIs and the reviews that will be required.

We have seen a marked improvement in the KTIs coming in as far as the level of quality. And, also, right now in our offices in Las Vegas, we're probably reviewing about 55 KTIs. And, again, it's a major undertaking, but it's always more pleasurable when it's a much more quality KTI.

As Margaret mentioned, the bundling approach that we're talking about is new to the project, as well as NRC. We're having a lot of reviews and discussions. Obviously, we want this to be user friendly to the ultimate regulator, NRC, and we'll have a number of discussions, technical discussions and workshops as we proceed through September and into October on this.

As we talked about, the approach arranges the agreements into 14 technically related areas, and presents responses in the context of an integrated technical basis. Whereas in the past, they would go individually, not always integrated, so, the reviewers were having to try to put those in the context of where we're going to be on the various analysis and modeling reports and other areas.

As Bob Andrews is going to talk a little bit later, the technical basis document presents the technical bases for describing the barriers and allocating performance, organized to be consistent with not only NRC's Yucca Mountain Review Plan, but also places the KTIs in context of the overall technical basis. And, having reviewed some of these, and continue to do so myself, I believe it's a lot more I guess I'd say user friendly on presenting this. So, Bob Andrews will talk about that a little bit later, and then also Bob and Bo Bodvarsson will talk about as far as saturated zone flow and unsaturated zone, and try to show some specific examples of these stories and how they relate.

Overall, it's a big challenge. The first two bars that you have there in essentially September and October are under review right now. And, then, after that, there will remain about 90 more, or so, to get done. And our goal, as we mentioned, is to address all those prior to the license application. We would obviously like to have resolved as far as NRC concurrence or approval on those. For those that do not have that approval, they will be addressed accordingly in the license application itself.

If I can now, I'd like to just go back to that last status, performance confirmation. We maintain our commitment to continue performance confirmation and other testing and monitoring programs. And, obviously, performance confirmation, as defined by NRC in 10 CRF 63, comprises activities specifically designed to confirm the technical basis for the licensing decision. And, Debbie Barr of our office tomorrow is going to talk about performance confirmation in her presentation, and will try to talk in particular to the question about addressing how we include our performance confirmation plan, how it relates to the license, and how we'll will have flexibility over time to modify that plan as we have new date from either science programs or other technical areas.

The next area I want to talk about is one I have a lot of energy on now, I mean, the license is obviously very important, and a major part of that is the total systems performance assessment, but repository design. And by that, it's the surface, the subsurface, the waste package, all the critical aspects.

I've seen a lot of stabilization, for lack of a better term, with design, and there's always a lot of concern that the design is changing here and there. But, when you're only 7 per cent on an overall final design, 7 to 8 per cent, I would expect that in any major project, and I would highly encourage or suggest to the Board maybe at the January meeting, we have Larry Lucas and some of our people come in and just give an update of where we are on that design. Because, remember, right now our schedules show about the May or June time frame of next year to finalize that design is required to satisfy the safety analysis in the license application.

We are developing right now sample license application section inputs to make sure we have the right level of detail to support the safety analysis and the license submittal. We plan to have significant discussion with NRC between now and probably mid November on that particular topic.

We're also using the WITNESS throughput computer model, which has already produced some useful results, including identification of various choke points in our various design itself.

We're also preparing a facility and systems design descriptions. A lot of those are underway in development right now to support the overall license application.

Also, one event that happened I believe it was after our last meeting was Bechtel SAIC awarded a critical subcontract for surface design support to Cogema, and they had mobilized in Las Vegas with a lot of support from the operations over in Le Hague, France. BSC is currently reviewing the material flow diagrams for the dry transfer facility and the canister handling facilities. So, there's a lot of activities underway right now, and actually, recently, we just stamped our first Rev A of the design drawing. So, a lot happening. We're doing the periodic monthly design review meetings, and that is proceeding very well.

Obviously, one of our goals is to make sure we just don't have the right configuration on that facility, but also, we have optimization for flow of the materials, obviously the right safeguards and security requirements, optimizations, as well as cost efficiencies. What we're committing to in that license is a major cost item, so we're all sensitive about designing in as cost effective manner as possible.

The next area is waste packages. It's one area right now that is moving very well. The closure cell prototype strategy includes prototypes for the waste packages, drip shields, and other key areas. An RFP was issued by Bechtel SAIC in July for the waste package, first waste package prototype. Award is expected, I believe it's still for the November time frame. We would plan on receipt of the first prototype as constructed in February 2005. So, that's a major milestone, too.

We're working with the Idaho National Environmental and Engineering Lab to make sure that we work in one of the cold cells up there to actually take that first waste package prototype and actually do the various work as far as prototyping some of the welds and other key areas. So, again, I want to leave you with the idea that not just progress and focus on the surface design, but also the waste package underway.

Systems associated with the closure cell include control, welding, robotics, stress mitigation, and leak detection amongst other areas. Bechtel SAIC is performing an assessment and 80 per cent status review at INEEL of the waste package closure cell development.

As I proceed now into the summary, I want to pick up on a couple key points. As I've said, a lot of our staff, and I've come to full recognition for the ten months on this program, it's not just the license, it's also operating as a licensee. By that, I don't mean to de-emphasize the importance of design and license, but we also need to show NRC the right operating culture, and have that ourselves in the project in order to ultimately obtain the license.

Margaret did issue a critical letter over to Marty Virgilio of NRC in May of this year, and we had five major commitments, and I just talked a little bit about the license, I won't repeat on that, but one of the areas was procedural compliance. You know, every day on our program, I would assume we probably touch through our national labs and Bechtel and our federal colleagues hundreds of various procedures there, and for a period of time, we had significant non-compliances with some of those procedures.

What we've tried to do is a couple of things. We've been streamlining our procedural review and revision process, and also we'll have an effective compliance trend report in place within two weeks.  It takes time to make that degree of change, but we're trying to have better tools for our line managers to look at some of the issues we have and really manage those aggressively.

Corrective action program is another key area.  In our program, probably for a lot of good reasons, over the last decade or so, we've probably evolved into about five or six different programs that feed into corrective action. We're going to one, very similar as to what you'd see if you go to Palo Verde or major commercial licensing operations around the U.S.  One system, trying to cut out a lot of the bureaucracy with it, where if I had a complaint, or any of our 2500 employees, you can get that into the system, get rapid management attention, and get it addressed and work at the closure, and have a priority basis, so at least there's feedback to the individuals as to how it was dispositioned.

Our goal is to have that new system rolled out by September 30th, again, in two weeks.  We started to roll out two weeks ago.  We're going through training of our employees now.  To determine how long it takes to get to effective and full utilization, it will be some time yet.  I don't expect that to happen over one month, two months, or three months. But, we are moving aggressively on the direction of one program.

Another area is what's called safety conscious work environment. Employees can raise issues and concerns without any fear of retaliation or intimidation or harassment. Actually, I'm real pleased. We've done inside the project two Porley surveys. The most recent was issued back in July, and right now, as we talk, just two weeks ago, we completed an independent survey that was actually done by a company that's done many of the surveys for the Department of Defense, as well as the recent one for NRC and others. It's purely independent from us. There were about 60 questions that went out to all, I think it went to about 2500 employees in the project, and we actually had about a 57 to 58 per cent participation rate.

So, the statistics look good as far as participation. I would look forward to getting the results from that, I assume that it will be about three to four weeks, and we will issue those publicly when those results come in. It will probably take our leadership team, you know, some time after that to look at it and determine where is other emphasis required, and where we're comfortable with various things in the project.

The last area is accountability. It's not just setting expectations, but holding, whether it be federal, contractor or national laboratory, everybody to the same standards and expectations in the program. As we talk, we are flowing down provisions. It will be on each employee's appraisal, as well as the major contracts, for expectations of not just good technical work, but performing as a licensee, and we expect to award for success and take proper actions where that is not adhered to.

Let me summarize then and go to the last chart. On the license, we do have a monthly operating review, a roll-up, that's weighted. But, you just can't look at a license on one measure and say what per cent complete. But, our assessment as of the last meeting, it was about 34 per cent complete. Our program, as Margaret said, is committed to submitting a quality license application to NRC not later than December of 2004. We're making significant progress for the technical basis in the license application, including the Key Technical Issues, Agreement Closure, the physical document itself, the Preclosure Safety, which is behind, but as you see the design pick up, you can't do a lot of the preclosure until you have the right level design done. So, those are pretty well running hand in hand there, as well as the repository and the waste package design.

One comment I took from one of the questions I think that Priscilla raised, you know, there is a close coordination right now between the program, science and technical program, and the project. I know Bob Budnitz, I met with him last week, has a pretty aggressive schedule for review of a lot of proposals for the S&T program, and some of those may be more applicable dealt with for long-term repository under Joe Ziegler. So, we have close coordination right now on those programs, and as Margaret said, I see science and technology, just like in any program, even at WIPP, it will be involved in this program for a long time in the future. I mean, there's things I hope to learn even from future mining technologies and others that we can apply. But, in the license, we need to go ahead with the best available information we have at this time.

So, let me summarize there, and just thank you for having the Board here and the meeting in Nevada, and look forward to entertaining any of your questions.

Thank you.

CORRADINI: Okay, thank you. Priscilla, Ron, Dave, Dan.

NELSON: Nelson, Board.

Thanks, and I think the connection between S&T and performance confirmation is one that I'm sure we'll be listening for tomorrow.

But, I assumed that on your first real chart, by putting the line Subsurface Construction and indicating a January 1 of '05 initiation for procurement of tunnelling contractor, including a tunnel boring machine, was put on just for my benefit. And, so, I'm wondering what the planning is for re-engaging underground space on this project. Is it possible that first panel could actually be construed maybe as part of performance confirmation or additional information acquisition, at least it's designed to serve dual purposes like that? What's your thinking on that, and when might the underground be re-engaged in that kind of way?

ARTHUR: I might need some help from Nancy or someone, but let me just take it first. First of all, on the schedule that I have there on the procurement or acquisition of contractor, probably if I look at anything in this decision plan right now why it hadn't been released yet, one of the reasons is the overall acquisition strategy for the long-term of this program. And, so, I'm not going to say that's a hard fixed date. I'll be right up front with you.

Right now, internal to the program, Margaret, myself, Ted, and along with our other federal leadership, are developing what's called a long-term acquisition strategy, which is required under the basic project management roles to say ultimately how we're going to procure the various contractors to do the work, whether it be in transportation, construction, or other key areas.

Specifically in regards to panel one, I mean, we know that that's obviously the first panel that we're going to go into, and we'll probably learn a lot there. So, I mean, one of the first areas we're looking at is just going into that, operating and learning, and then moving into obviously other panels after that.

As far as other specific areas, Nancy, do you have anything to offer on that? I just want to make sure I'm getting some of the questions. Claudia?

NEWBERRY: Claudia Newberry, DOE.

Priscilla, in the first panel, we do have a dedicated drift that will be PC. So, there is one drift whose whole purpose is to support the PC program.

NELSON: Nelson, Board.

But, is this before? Where does this fit relative to the LA process, when you might be thinking about starting that?

NEWBERRY: We can't start underground construction until we have a construction authorization.

NELSON: Right. So, the question was that would be considered construction as opposed to science--

ARTHUR: Science and development?

NELSON: Yes.

ARTHUR: The preliminaries we're evaluating, but I would assume it would be, which would mean we need a construction authorization.

CORRADINI: Ron?

LATANISION: Latanision, Board.

John, first, thank you for the perspective that you've provided us, and also the efficiency in presenting information. You packed a lot into relatively few transparancies, but they help a lot. Thank you.

I want to turn to the fifth slide, if we could show that, which has to do with the KTIs. What impresses me is that there's literally a quantum change in the number of KTIs, and I wonder first of all about a couple of comments you made, one of which focused on the issue of improving the level of quality of the KTIs. So, it would help me to have some sense of what concerns you had about the quality of the earlier generation.

And then, secondly, I'm wondering whether or not, based on your experience, there is a precedent that would lead you to believe that you can deal with all these KTIs in the time intervals that we're talking about here, given the history.

And, finally, and this may be something that Rob MacKinnon may cover, but I'm very curious to know, as an example, what would be the kind of KTI that would be emerging from the point of view of the engineered barrier.

ARTHUR: Okay. And, if I can, I'll have Joe Ziegler, can you just come up? And I'm going to give you a chance to meet Joe first-hand as the new Licensing Manager. But, I'm going to take a first knock at the question.

LATANISION: Okay.

ARTHUR: On the KTIs, I mean, obviously we're moving at a much slower rate. As we said, we'll have 200 out by the end of this year. As far as quality, what I think we've seen is more responsive to what we anticipate is going to be required to satisfy NRC. Inside the project, we almost have an internal score card about how many hit the target the first time and were approved, versus re-works.

I think Joe and his staff, and I go based on their discussion with me weekly, are real pleased with the quality of what's coming in. It's responsive to meeting the necessary criteria and satisfy the technical question.

As far as the bow wave, as we call it, and I don't like that term because it usually means a tidal wave is coming, but our staff is working mighty hard, and I say staff, DOE, the labs, and Bechtel SAIC. It's been a lot of work, and actually we're ahead of schedule, knock on word, or whatever this is, for right now on these KTI. And we were behind. We had some real concerns, but a lot of that delay was trying to get things laid out in the new process or new way of doing business.

Joe, do you want to add?

ZIEGLER: Yeah, Joe Ziegler, DOE.

Multi-part question, so I'll try to answer the parts that I remember. But, the quality wasn't so much with the technical quality of the work that was coming from BSC.

It was more to do with follow-up questions from NRC. We also got way behind schedule at the earlier part of this year, as we were in a continuing resolution, and we just had problems doing all the work that needed to be done with the funding that was available.

There's also been quality assurance issues, more internal, not--we weren't submitting agreement responses for quality problems, but we have had quality assurance issues. They're pretty well known on the program, that we've been trying to work through the models and software and things like that. So, that delayed submittal.

The bigger part of the grouping or the bundling is that everything had to be part of a coherent story to be able to close these agreements. And the agreements were made individually, typically agreement by agreement over the course of a couple of years of technical interactions with NRC. In several cases, we responded to the agreements, but then NRC came back and said yes, but in the overall context, you didn't deal with A, B, and C, even though the question was X, Y, and Z. You answered the question, but maybe you didn't deal in the total context of the way the concern that was the basis of the question related to the overall performance of the repository.

So, and Margaret described this a little bit, so what we have done is that, one, it's a more efficient way of doing business, and we're hoping to close more of these things the first time around with NRC, is that we're trying to look at this total picture of the various processes that the repository works, and I think we divided it up into about 14 different processes. And we're grouping the agreements within those processes so that we don't just answer the agreement, you know, literally; that we answer the agreement and the concern associated with that particular agreement, such that in the overall context of not just the answer to the question, but how that question might relate to overall repository performance.

So, I think overall, we're going to do a better job and get better acceptance the first time around with NRC, and our goal is to get as many of these things closed before license application as possible.

And I can't remember the rest of the question.

LATANISION: The last had to do with the issue of some examples of KTIs related to the engineered barrier. Maybe that's something that will come up later. I don't know, maybe Rob can tell us that real quick. I'm not sure. Is that the case?

ZIEGLER: It will probably come up. There's KTI agreements associated with corrosion. I think there might be a couple on the waste package, the in-drift chemical environment of the waste package, and there's agreements associated with all those things.

LATANISION: Okay.

CORRADINI: Dave, Dan, and Mark.

DUQUETTE: Duquette, Board.

One specific question and a general question. The first one is I notice on the two I'll call them organization charts, that the engineered barrier system showed an improvement in performance in May, and again in August. What does that mean?

ARTHUR: And between the period, I think if I recall correctly, we'd gone from red, and I'm looking at the May first of all, it went from red to yellow, and then we must have an issue where it dropped down again. And, I think at that time, first of all, you're going to see a consistent area on a number of these areas. Some of the work was deferred, essentially the continuing resolution and budget issues, and then we had a catch-up of a lot of the technical work. So, right now, we're reporting that's in the yellow are, and actually it looks like it's continuing to improve.

I apologize, I didn't bring the specific report, and I'd be glad to share the actual variance analysis with you.

DUQUETTE: No, I was just curious as to whether--

ARTHUR: It did drop back down.

DUQUETTE: --the improved performance was yellow in locations for the engineered barrier systems, and I wondered if that's just you set goals, and I'm just trying to understand the process. You set goals, and they've exceeded their goals; is that what that arrow means?

ARTHUR: Well, it means there's improvement since the last monthly reporting. So, it meant that there was red, and the variance came out, so the manager itself, and these judgments are made by the accountable project managers, if they felt there was a desired level of improvement, so it came up to yellow, which doesn't mean it's done. It's still behind schedule, or has other quality issues. The goal is obviously before we go into license, to have most all these areas in green, with high confidence and cross-integration and other reviews.

DUQUETTE: The second one is a simple one, and I don't want all the details, but I wondered, given that there's so many KTIs coming up, if we could just get a simple list of what they are and when you're going to submit them? I don't want all the details behind them, but I would like to know what they are.

ARTHUR: We'll be glad to get that. I know there's another presentation later, we'll show them by the actual 14 key areas that we're bundling them. That's no problem.

CORRADINI: Dan?

BULLEN: Bullen, Board.

Just a quick question on Figure Number 5. You described the wave as a bow wave coming in, and I see it there on the figure. The question that I have is this must be predicated on the fact that you get your funding for the Fiscal Year '04 at the level that you requested. How does the shape of this curve, or the number of KTI results change if you're at the Senate mark versus the House mark? Is there going to be a decrease in the peak intensity there?

ARTHUR: I could probably almost repeat the briefing I gave about--was it wasn't a fun time to go through a replay on this project, with the whole team, John Mitchell and ourselves, was to try to realign a program about $130 million short this year, and it did create a bow wave, and I appreciate the hard work of people to play catch-up. But, if we go two or three months at a real reduced area, we have big problems on the on the license.

BULLEN: Thank you.

ARTHUR: No other answer to it.

CORRADINI: Mark?

ABKOWITZ: Abkowitz, Board.

John, if we could go to Slide Number 4, please? I just wanted to understand the interrelationship between some of the boxes on the safety analysis piece, in particular, the total system performance assessment. If you could clarify for me my understanding of the status of the TSPA? Is it not pretty much complete from the standpoint of the performance assessment that you intend to provide with the license application?

ARTHUR: I think our projection on the TSPA shows about, our estimate, Joe had, 55, 56 per cent complete. Why we show that still as red is the sub-details on a lot of things, until all the AMRs are complete, at the right level of quality, we'll still probably carry that red until we see everything that supports TSPA.

ABKOWITZ: Okay. But, the point is is that the projections that are coming out of TSPA are pretty much the ones you're going to run with in the LA?

ARTHUR: Joe, do you want to talk on the specifics there?

ZIEGLER: Joe Ziegler, DOE. And I'll get Bob Andrews to help with this if you can answer the question. TSPA is being rerun for the license application. So, it's not going to be the TSPA. It will have been updated. The AMRs are being updated, analysis and model reports that feed TSPA, so those analysis and model reports are being completed as we speak. I think we're making good progress on them. We'll do another integration review to make sure that they all integrate and the pieces fit together. There may be some updates to some of those AMRs before the license application itself. The TSPA will be rerun before the license application.

ABKOWITZ: Okay. Abkowitz, Board.

Let me explore that a little bit further, if I could. Running the TSPA is the culmination of the component pieces that we see beneath it feeding up. And you've committed yourself to a 100 per cent quality effort. So, if I'm doing the quality in one of these areas and discover that the models that I've used are flawed, how does that work it's way into the final product?

ARTHUR: You'll see a number actually go down. There's another--I should have probably brought a whole report, and I'd be glad to share that with the Board. There's one section of the report, and I didn't include it, that shows models, the total amount of models that will be required for the license, total amount of data sets. I think at one time, for example, the universe of data sets was at about 1500, of which we had "X" per cent done. If we have a quality issue that perpetuates across the system, we'll go back to reduce that per cent complete, and come back and do the necessary re-work. Obviously, our job is to build quality in the first place.

ABKOWITZ: So, that at this juncture then, are you committed to basically saying that there is no exact date for running a final TSPA. It will be driven by quality?

ARTHUR: I guess we have a schedule to run the TSPA right now, but we're not going to submit the license until everything is the right level quality.

ABKOWITZ: And can you give me an example, based on your quality management efforts to date, where you've rejected a model that's in one of those yellow and green boxes below the TSPA? Or are you focusing primarily on the quality of the justification itself?

ARTHUR: There's probably multiple reasons. And, Bob, why don't you discuss one of the variance reports.

ANDREWS: Yes, this is Bob Andrews of BSC.

Let me answer the first question about the schedule first. The feeds, whether they be data feeds or parameter feeds or component model feeds or extractions to the TSPA, are what happens in those boxes below the TSPA, and also the design related inputs into that. Those are in varying stages of completion and checking and review. They are not all done. The TSPA model for the license application is being developed as we speak, based on the inputs that it has from those models, and calculations that provide those data sets and parameters.

If there is an issue with model validation or data quality issue associated with any of those feeds, of course the data feeds will change, and the model will be re-evaluated and, as necessary, the TSPA inputs changed. Not always will that change the input. It might be an issue with respect to a particular process or uncertainty that needs to be better described or presented within the context of the analysis or model.

The actual schedule for developing the TSPA goes from now through essentially the end of this calendar year. It then goes through its own check and review process before the actual TSPA calculations are conducted, which is next February and March, and documented then in April and May. So, the documentation of the TSPA/LA for the license is completed in the end of May of next year.

And, going back to the quality issue, if anything changes with respect to any of those inputs between now and then, it is always possible to re-evaluate and rechange and rerun the actual calculations.

ABKOWITZ: Abkowitz, Board.

So, is it fair to say that the quality issue is essentially subservient to the TSPA schedule as opposed to the other way around?

ARTHUR: Absolutely not. I mean, the quality issue is an equal priority to the schedule.

ABKOWITZ: So, then on the record, you are committed to running your final TSPA only when you are totally satisfied that the quality in all of the components to the TSPA have passed, you know, a reasonable standard?

ARTHUR: That's the same commitment made to NRC in our letter of May, that we're not going to submit a license application until all aspects of the license meet the applicable requirements, including the quality assurance requirements document.

ABKOWITZ: Thank you. Abkowitz, Board.

I'd like to go to Slide Number 5 for a minute, please. The items that are shown here, do they count only after they've been acceptably resolved between NRC and DOE, or just submitted by you?

ARTHUR: That's DOE's submittal to NRC.

ABKOWITZ: Okay. I'm also concerned by the bow wave. I mean, I admire the work you all are doing, and I think that you're going to do positive things, but the cultural shift and the projections shown here are astonishing. And, so, what I would like to ask is that we keep this chart as a regular metric, and that the next time that you meet with the Board, you know, we have what you show as the new schedule will be shown as the old schedule, and you can show us how the performance of this middle process went with that, and also that we add another column, which are successfully resolved, so that we can monitor this in a more practical fashion.

ARTHUR: Right. And, that's the way we're planning on monitoring. This is the revised baseline due to a number of areas, is the rebundling, as I'm looking through this. The revised baseline, again, we're going to have a lot of discussions with NRC to make sure everything is acceptable with them as far as the process. So, that will happen over the next month, and we'll be tracking progress against the plan, as well as how well we're doing in getting them accepted and approved.

ABKOWITZ: Thank you.

CORRADINI: I want to take one more question. Board Staff? Dan Metlay?

METLAY: Dan Metlay, Board Staff.

John, as you know, one thing that's smack dab on the critical path, according to the regulations, is the certification of the licensing support network. On Charts 3 and 4, it's red in both cases. And that's got to be certified, at least my understanding is six months prior to the submission of the LA. So, that puts you June, July.

I'm wondering what the issues are there, and how you expect to resolve them in the next six months, seven months?

ARTHUR: Thanks, Dan. That's a question I should have answered before in the presentation. I appreciate it.

One, first, why it's red, we had the delay, first of all in getting out some of our criteria on relevancy and the kind of documentation that was going to be required. We've gone throughout the program. We've had some of our key personnel training people as far as the kind of records that are going to be required. And, then we also fell significantly behind for the last three or four months in getting documents over to our contractor, who's actually loading them in.

As of last week, actually, and I would assume that's going to come out in the yellow, but, again, I'll hold it red until I'm real comfortable we're going to move into the improvement area. But, we're seeing a catch-up game now as far as the documents going in, and the processing, screening, and other key areas. The Department's goal is still to have that certified by the June of '04 date, which is six months prior to the license submittal.

CORRADINI: Okay, I think we'll close now. Thank you, John.

ARTHUR: Thank you.

CORRADINI: We're scheduled for a break. We're a little bit late, but let's come back together and convene at 9:45. We'll talk about engineered barriers.

(Whereupon, a brief recess was taken.)

CORRADINI: Dr. MacKinnon, Rob MacKinnon, is Leader of the Uncertainty and Sensitivity Analysis Section in Bechtel/SAIC Company's Total Systems Performance Assessment Department, and is a principal member of the technical staff at Sandia National Laboratories. Dr. MacKinnon has over 25 years of experience in analyzing geosystems, including 14 years in performance assessment analysis of nuclear waste disposal facilities. Today, Dr. MacKinnon will give us an update on the performance of an engineered barrier system.

MACKINNON: Good morning. As Mike said, my name is Bob MacKinnon. I'm with Sandia National Laboratories in Albuquerque, New Mexico.

Before I talk this morning, I'd like to thank Dave Savugian, Patrick Mattee, and Martha Pendleton for helping me prepare the presentation.

As you know, at the last meeting, a series of integrated talks presented the project's technical basis and understanding of the processes that control the in-drive environment, and the processes that control the behavior of the waste package outer barrier.

This morning, what I'm going to try to do is describe how we take that technical basis and incorporate it into the total system performance assessment for license application and system modeling.

A summary of my talk, or an outline of my talk is as follows. I'll first start out with a summary of integrated presentations. And, this will just be to put everybody on the same page. And, then I'll give an overview of the engineered barrier system processes, and models that are incorporated in the EBS total system model.

Following that, I will give some key conclusions on aspects of the EBS that define if and when localized corrosion may occur on the waste package outer barrier.

And, then the remainder of my talk will be to show you that our approach will further verify those conclusions, and this will involve presenting how we use our models to calculate the entire range of environmental conditions on the waste package surface, including the treatment of uncertainties.

And, then I'll focus on how we take our localized corrosion model, and exercise it over that full range of conditions on the waste package outer barrier to determine, one, does localized corrosion occur, and if it does occur, how frequent is its occurrence, in other words, how many packages might be affected by localized corrosion. And, if we need to incorporate it in TSPA, how it will be incorporated in TSPA.

Then I'll give a couple of example results on some simple total system calculations that were done with the final impact, final environmental impact statement TSPA model, and wrap it up with a summary.

One caveat is that the conclusions and analyses that I describe this morning are preliminary, and they won't be final until they're submitted with the licensing basis. So, everything I'm describing this morning is preliminary. We've essentially received all of our feeds and our models, and we're in the process of putting together the TSPA/LA model. So, we're not prepared to present any calculations using that model this morning.

Let me give you just a brief summary of the presentations from the last meeting. Bo Bodvarsson summarized the technical basis and understanding for the coupled processes in the host rock, and the technical basis for modeling seepage and seepage chemistry into the emplacement drifts during both the thermal and ambient periods. And, Mark Peters described the project's technical basis and understanding of processes that take place in the in-drift environment, and in particular, how that seepage that enters the drift may evolve. He also addressed how dust deliquescence my evolve on the waste packages.

Joe Farmer presented the technical basis and understanding of the waste package outer barrier performance under a range of environments. So, again, the point of my presentation this morning is how do we take that understanding and incorporate it into the total system performance assessment model.

This chart was a focus of the last meeting. It's a schematic of how the evolution of the in-drift environment takes place. On the Y axis here is waste package surface temperature, linear Y axis, and time, logarithmic scale on the X axis. Two important thermal hydrologic variables are plotted: waste package surface temperature and relative humidity.

As discussed in the last meeting, you can essentially divide the post-closure evolution of the in-drift environment into three regions: this tan region, which represents a high temperature dryout region, this blue region, which represents a cooler region, and then an intermediate transition region. In this high temperature region, as the waste package surface temperatures rise and then gradually decrease, simultaneously, the relative humidities increase, and eventually temperatures and relative humidities will intersect deliquescence points for dust that may potentially be on the waste package surface, and then deliquescence may form. As temperatures continue to decrease, a deliquescence will evolve and eventually temperatures will decrease, because sufficiently low that seepage will enter the repository.

It's in this transition region where the seepage evolves and the dust deliquescence evolves, that it's very important that we have a good understanding of the chemistry on the waste package outer barrier. In this lower temperature region, as temperatures dropped, the waste package outer barrier becomes relatively insensitive to the chemical environment. So, the majority of my talk this afternoon will focus on how these models are implemented in this transition region.

Now, you can see on this plot, there are three waste package temperature curves plotted, the hotter 21 PWR and the intermediate hot 44 BWR, and the cool Defense package. So, you can see that there's a range of environments just due to the fact that we've got packages that have different power outputs.

A similar plot can be made if you looked at just the 21 PWR, because the 21 PWR, because of variability in heat transfer processes throughout the repository footprint, the 21 PWR packages will have a range of thermal conditions themselves. So, if you plotted, made a similar plot for the hottest package in the repository and the coolest 21 PWR package in the repository, you would get a plot similar to this. The point here is that there's a range of thermal hydrologic and chemical conditions that occur in the post-closure period throughout the repository. And, when those regions occur is dependent on spatial variability and uncertainty in key processes.

This is an overview of the models that will be included in the TSPA/LA/EBS system model. There's nine models shown on this figure, starting over here to the right, going counterclockwise. You will notice that there are a couple of models here with vast boundaries. That's to indicate that those models do not provide direct input to

TSPA, however, they do provide important input to models that provide abstractions to TSPA.

I'll start over here on the right and give you a brief description of some of these models. I'll focus mainly on Models 1 and 2 and 5, because that's the focus of my talk this morning. I'll start with the thermal hydrology model and the chemical environment model. Those two models together define the conditions throughout the drift at various locations. They provide important information to drip shield and waste package degradation models, to waste form degradation models, to waste form mobilization, and to EBS transport.

In particular, the thermal hydrology model provides temperature and relative humidities at various locations in the drift, for example, at the crown of the drift on the waste package and drip shield surfaces and in the invert. Temperatures and relative humidities are a function of, as indicated here, is a function of time, position in the emplacement drift, but also position in the repository. They're a function of the power output from the waste packages, and importantly, the thermal conductivity of the host rock, and the infiltration that is occurring, or the percolation flux at the repository horizon.

Those latter two inputs, thermal conductivity and percolation flux were infiltrations at the repository horizon, are two key uncertainties in our thermal hydrology model.

The chemical environment model takes that output, temperature, relative humidity, in addition, partial pressure of carbon dioxide, seepage composition, and dust deliquescence compositions, and calculates the evolution of the chemical environment at various locations within the drift, and in particular, on the waste package outer barrier.

Now, as described in the last meeting, and as I mentioned earlier, Bo Bodvarsson presented the technical basis for using the drift scale THC model to predict the range of seepage compositions that enter the drift. Mark Peters described how that information was taken to develop lookup tables that represented the chemical composition as a function of temperature, relative humidity, and partial pressure of carbon dioxide. It's those lookup tables that are implemented in TSPA, and that are represented by this upper half of this bubble.

The drift seepage model provides the seepage flux into the repository. And, as pointed out in the last meeting by Bo Bodvarsson, one of the key features of the drift seepage model is that when drift wall temperatures are above boiling, seepage doesn't enter the repository. I'll come back to that issue later.

Models 4 and 5 model the drip shield degradation and waste package degradation. I want to point out a couple of important points about the drip shield degradation model. Our current models and analyses for the drip shield conclude that the drip shield will not fail in the nominal scenario during the post-closure period. However, damage to the drip shield can occur during the seismic scenario class, because of ground motions and drift degradation.

In the remainder of my talk, I'm going to focus mainly on the localized corrosion model. Can you see this pointer? I'm going to focus on the localized corrosion model. This model requires, as input, the relative humidity and the temperature on the waste package surface, in addition to pH, partial pressure of carbon dioxide indirectly, because that controls the chemical environment, chloride and nitrate concentrations.

So, our models will be implemented to determine the values for these various parameters for the entire range of conditions in the post-closure environment, including the treatment of key uncertainties that go into the prediction of those values. The pattern backgrounds here indicate, and I won't mention these models further in my presentation, but we have models that predict the evolution of the chemical environment inside the waste package. That model provides input to the waste form degradation models. It also provides input to the waste form mobilization models, that is, what are the solubility of radionuclides, what's the concentration of colloid-bearing radionuclides, and EBS transport. EBS transport is used to calculate the release of radionuclides from the waste form through the waste package, through the engineered barrier system, and to the host rock. That's a summary of the EBS models that will be included in the TSPA.

Okay, let me summarize some key conclusions that really define if and when localized corrosion may occur. Many of these conclusions were presented at the last meeting. The first one here, and I've mentioned this previously, drift seepage will not occur for crown temperatures above boiling temperature. The technical basis for that conclusion was presented by Bo Bodvarsson at the last meeting.

It's highly unlikely that dust deliquescence on waste packages will initiate localized corrosion. And, this is based on, and presented by Joe Farmer at the last meeting, based on analyses of data gathered in the dust deliquescence testing program, and analyses that indicate that these deliquescence solutions tend to be carbonate and nitrate type brines with relatively high concentrations of inhibiting nitrate.

If seepage water reaches waste packages, conditions suitable for localized corrosion may occur during the thermal period. This was pointed out at the last meeting. We think the probability of its occurrence is low, but there is a slight chance that calcium chloride type brines may seep into the repository during the thermal period. However, it should be noted that in the nominal scenario, the drip shield will not fail. That's a conclusion we have reached from our current models and analyses. And, therefore, seepage cannot reach the waste packages during the nominal scenario during the thermal period, and occurrence of localized corrosion is highly unlikely.

Now, seepage may contact the waste packages if the drip shield is damaged, and drip shield damage in the seismic scenario class does allow seepage to reach waste packages, and conditions for localized corrosion may then exist following an early post-closure seismic event. So, if a sufficiently high level of ground motion occurred during the thermal period, it's possible that the drip shields could be damages, and seepage contact the waste package. In that case, it's possible, but we think unlikely, that localized corrosion will occur.

Now, you can ask the question what do I mean by highly unlikely, and may. Well, what I want to walk through in the remainder of my presentation is how we are going to use our models, exercise the localized corrosion model over the complete range of thermal and chemical conditions that can occur, and evaluate whether or not localized corrosion does occur, and if it does occur, on how many waste packages might it occur on. And, so, hopefully, our outline or approach to answer how unlikely it is.

So, let me start with quickly defining the environment on the waste package surface. Key parameters contributing to the chemical environment include incoming seepage, composition of dust deliquescence on waste package surfaces, temperature, relative humidity on the waste package surface, and then, of course, evolution of the chemistry as the temperatures and relative humidities change with time.

And, in particular, we need to calculate temperature and relative humidity, pH, nitrate concentration, chloride concentration, and the ratio of nitrate to chloride. So, these are key inputs to our localized corrosion model that will be implemented in TSPA.

So, what I'm going to do next is describe to you how we calculate these quantities, and how we account for spatial variability and uncertainty.

The thermal hydrology in the engineered barrier system is calculated with our multi-scale thermal hydrology model. This model represents repository footprint shape and location with respect to stratigraphy. So, it's an approximation, a 3-D approximation to the thermal hydrologic environment. It includes repository scale and temporal variability in percolation flux. It includes uncertainty in percolation flux and thermal conductivity. And, this uncertainty in percolation flux and thermal conductivity is represented by five complete cases that are simulated for TSPA/LA.

Three cases involve a low, middle, and high infiltration field to capture the uncertainty in infiltration. And, each of those are computed with a mean host rock thermal conductivity. And, then we have two other complete sets of simulations that couple low infiltration with low thermal conductivity, and high infiltration with high thermal conductivity, to capture the upper and lower ranges of the thermal hydrologic conditions and, in particular, on the waste package surface.

Now, results from these simulations are abstracted for all of the waste packages in the repository. So, what we abstract out of these simulations and store for implementation in the total system performance assessment, for example, the temperatures and relative humidities, is a function of time for each one of these cases.

This slide presents some representative results. On the left, you see we have a plan view of time when drift wall boiling ceases. And, on the right, we have a representation of peak waste package temperature as a function of time. Now, this is just simply peak waste package temperature.

Now, the point here is if you look at this plot, that time when drift wall boiling ceases ranges anywhere from 200 years to approximately 1400 years. And that variability is due to a number of factors. One is the spatial variability and heat transfer processes. More heat is transferred at the edge of the repository panels than at the center, so that temperatures are higher in the center of the repository. And, also, we have variability in the infiltration field. Together, those produce spatial variability in waste package surface temperatures. Now, these are plots of just the 21 PWR packages.

Also, over here on the right, it gives you an idea of where the peak waste package temperatures occur, and what they are. You can see again in the center of the panels, the waste packages are hotter, and temperatures reach up to near 175 degrees C.

CORRADINI: Can I ask a point of clarification?

MACKINNON: Yes.

CORRADINI: So, let's take P1. How many drifts are represented? Like 20 drifts? Just so everybody understands. I think I get it. Across, right? There's lines of drifts. So, approximately?

MACKINNON: Eight drifts in P1.

CORRADINI: Eight drifts?

MACKINNON: Right.

CORRADINI: So, this is an averaging across the eight drifts of what's occurring in each drift?

MACKINNON: This is actually a plot of each PWR package in--

CORRADINI: Each drift?

MACKINNON: --each drift. That's right.

CORRADINI: Okay.

MACKINNON: So, the point I want to make here is that our model accounts for spatial variability, in addition to different waste package power outputs. And, next I'll address how we treat uncertainty.

These are plots of calculations at a representative center location in the repository. The point here is I just want to illustrate the impact of uncertainty in the host rock thermal conductivity on the waste package surface temperature. For example, we've got three plots here. The high temperature corresponds to low percolation flux, low thermal conductivity. That's one of the cases we'll have in TSPA. That would represent the hotter end of conditions. This lower curve corresponds to high percolation flux, high thermal conductivity, with the mean value temperatures represented in the middle. And, down here, are the corresponding plots of relative humidity.

Over here, shows the effect of different power outputs, and these curves are the same curves that were represented on that earlier figure. So, again, our model represents spatial variability, as I described in the previous slide, and the impacts on temperature and relative humidity due to uncertainty and thermal conductivity of the host rock.

Now I'll briefly describe the engineered barrier system chemical environment model. This was described in part by Mark Peters at the last presentation. I'll briefly run through these top bullet, my main focuses on here.

This model takes the incoming seepage compositions and abstracts those into eleven bins of representative chemistry. Those bins are then used to develop bin histories that are used to represent each of the incoming water compositions. We've got five different incoming water compositions, and the technical basis for those water compositions was described by Bo Bodvarsson at the last meeting.

We have 55 USGS dust deliquescence samples. Those samples were analyzed by our geochemistry team, and binned into six groups with common chemical characteristics. Models were implemented to evaporatively evolve the seepage composition, and calculate the evolution of the deliquescence chemistry for different values of PCO2 temperature and relative humidity. And, those tables are implemented in TSPA. So, this is the second time I've pointed out.

Now, because these tables are developed for five different incoming water compositions, and six different dust deliquescence compositions, we use those to represent uncertainty in the evolution of dust deliquescence and seepage.

So, we account for uncertainty associated with the incoming seepage. With each incoming seepage history there is a partial pressure of carbon dioxide history as well. We represent uncertainty in the composition of the dust deliquescence that forms on the waste package, and then also we have uncertainty contributed in our calculations from uncertainties in thermodynamic data and modeling assumptions.. And, these uncertainties are all included in our calculation of the in-drift environment and the environment on the waste package surface.

These are preliminary results, but really I just want to illustrate how we will calculate the range of conditions on the waste package surface. On the right is a plot of waste package surface temperature and relative humidity. And, for this set of waste package temperature and relative humidity curves, we ran a probabilistic 100 realization case where we sampled uncertainties in dust composition, and uncertainties due to our uncertain inputs in our model calculations.

This plot on the left shows the mean concentration ratio for nitrate to chloride that was calculated for each one of our different dust deliquescence compositions. We have six compositions, and there are six mean compositions represented here. You will see that there is an upper bound curve and a lower bound curve, and these two curves bound the range of nitrate to chloride ratio that was calculated on the waste package surface for this specific temperature and relative humidity curve.

And, on the right over here, you can see we plotted for dust water Number 4, the uncertainty bands in the calculation of nitrate to chloride for that specific dust water. So, this really illustrates how we will calculate the range of conditions on the waste package surface. We could plot similar results for pH, nitrate by itself, chloride by itself. And, what we will do is calculate the range of conditions on each waste package that we have in the repository. So, we will go through this exercise for each waste package relative humidity curve.

And, out of that result, we will have covered the complete range of environmental conditions on the waste package surface. Then, we will take our localized corrosion model and exercise it over that range of conditions to evaluate does localized corrosion occur, and if it does occur, how frequent is it.

We outline how we plan to go through that exercise. We're in the process of developing a model that's implemented using our GoldSim software. GoldSim software is the primary simulation engine that controls our calculations. This module will couple the in-drift thermal hydrologic and chemistry information with the localized corrosion model. We'll sample all of the uncertainties, conduct multiple realizations, exercise the localized corrosion initiation model over the range of potential post-closure environments.

Output from this exercise will include one or more uncertainty distributions, cumulative distribution functions, for the fraction of packages that experience localized corrosion, if indeed it does occur. We expect that if it does occur, there will be a very small number of waste packages with low probability, and our plan to incorporate those in the TSPA would be to sample these distributions at the beginning of run time in TSPA/LA to represent the number of waste packages that would fail due to localized corrosion.

This is an illustration of how that model will work. It couples the lookup tables for the various chemistry inputs, including the dust deliquescence evolution, seepage composition evolution, the seepage histories, and the partial pressure of carbon dioxide histories, in combination with the temperatures of waste package and relative humidity versus time curves. So, all of this information is going to be brought together. Uncertainties will be sampled. Multiple realizations will be calculated to produce a PDF similar to this.

Our localized corrosion model is our initiation model. It uses empirical regression coefficients for corrosion potential and crevice repassivation potential. These equations were developed using both Yucca Mountain project data and Center data. Both of these data sets were combined to represent a wide range of environmental conditions. The project data tended to focus on the intermediate to high concentrations of calcium chloride, for example, whereas the Center data concentrated on lower end concentrations of calcium chloride. So, together they span a wide range of environmental conditions.

The regression equations that were developed from this data include dependence on temperature, pH, chloride concentration, and nitrate concentration.

So, let me summarize quickly. Crevice repassivation potential is a function of temperature, pH, chloride, nitrate concentrations, and also the ratio of nitrate to chloride concentration.

Long-term corrosion potential is a function of temperature, pH, chloride, and nitrate to chloride concentration.

This question mark is not a measure of my lack of competence on this equation. In fact, I think what happened was that when they--they assured me that this would not happen naturally. This is from a PDF. If it would have been a power point figure, this question mark wouldn't be there. But, what should be there is a delta. So, localized corrosion initiates when the delta potential, and also this should be less than or equal to zero.

One thing I do want to add, too, is that in these regression models that were developed, we account for the uncertainty in the parameter coefficients that represent those regression equations. So, we've got uncertainty here in the parameter coefficients, as well as uncertainties in the inputs in that model.

This is just a summary of the independent variables and uncertainties, and I'll walk through these quickly. In-drift thermal hydrologic environment, temperature of the waste package and relative humidity on the waste package surface. We've got five cases that represent uncertainty and variability in thermal hydrology.

Dust deliquescence, crown seepage, and gas compositions. Uncertainty is represented by five seepage and PCO2 histories, and six dust deliquescence waters.

Evolution of the in-drift chemistry requires these as inputs. These inputs are uncertain, as I just described, and they're propagated through to the calculation of pH, nitrate, and chloride. In addition, we have model uncertainties associated with these calculations, and they are accounted for as well.

This is a simple schematic of how the calculation will be carried out. We start up here with the system parameters. We plan to represent infiltration variability by sub-region. We divide the repository into five main sub-regions, each sub-region defined by the infiltration in that region. So, we like to have similar infiltration in each region. Waste form variability, we've got different waste package types, and we have five TH cases that represent thermal conductivity and percolation flux uncertainty. And, for each one of these cases, then we are going to run a series of probabilistic simulations.

We plan on sampling water type, dust type, localized corrosion uncertainty coefficients, and the chemical environment uncertainties, and then loop on each waste package to simulate localized corrosion initiation under those conditions, and we will complete this loop on every waste package in the repository. And, then also complete this outer loop that covers the range of uncertainties.

And, out of this calculation, we will assemble cumulative distribution functions of the number of packages that experienced localized corrosion, if indeed it does occur.

Now, we haven't actually done or completed our calculations for the implementation of localized corrosion, so I'm not going to present any of those results this morning. But, what I am going to do is just present some example results on the potential impact of localized corrosion. if it did occur, on system performance. And, in this example, I relied on previously published results that are given in the risk information to support prioritization of performance assessment models.

And, what you see over here on the right on this top curve is a simulation that was done for waste package neutralization analysis. There is no drip shield failure at all in the repository, and all waste packages are completely failed in this simulation.

So, the waste package is completely failed. Since the drip shield is intact, all releases are diffusive. You can see that peak annual dose rates, this should be milligrams per year, annual dose rates are up around 20 millirems per year for this example. So, this is a very extreme case.

As I noted earlier, localized corrosion will not occur in the nominal scenario because the drip shield will not fail, and if dust deliquescence initiates localized corrosion, then what would be the potential impact? So, what I did was I decided that let's go ahead and estimate based on the assumption that 1 per cent of the waste packages in the repository fail due to dust deliquescence. We expect the fraction of packages to be much less, but I chose the number of 1 per cent.

So, I made the assumption that this peak dose rate scales linearly with the number of packages failed. In this simulation, we had roughly 12,000 failed packages. So, 1 per cent represents 120. And, so, a result is about .2 millirems per year. So, this is just a simple example on the potential impact to system performance due to the initiation of localized corrosion due to dust deliquescence.

Now, I should add that if localized corrosion did occur due to dust deliquescence, waste package failure area would likely be much less than 100 per cent. So, this is assuming that 1 per cent of the packages fail completely. That's just to give you an idea of the potential impact using existing results.

Now, as I noted, it's possible that the drip shield will be damaged in an early seismic event. So, we selected to run a total system performance assessment calculation using the final environmental impact statement model. We developed this simple example. We assumed that drip shield damage event annual frequency is 1 times $10^{-6}$. Waste package degradation is due to localized corrosion only. So, I'm making the assumption that waste package degradation due to localized corrosion occurs.

The initiating event that damages a drip shield has to occur within the first 1500 years because it's unlikely that as the repository cools, that localized corrosion will occur. It's more likely to occur early in the thermal period. So, we need an event that happens in the thermal period.

As I stated previously, seepage will not contact waste packages unless a disruptive event damages the drip shields. It's unlikely that localized corrosion will occur after 1500 years.

Our seismic abstraction model indicates for this level of seismic activity, that 3 per cent of the surface area on the drip shields will fail. I use that number, and I also made the assumption that 10 per cent of the waste packages contacted by seepage within 1500 years after closure experience localized corrosion. That's a fairly high number. We expect it to be significantly less than that. But, again, this is just an example problem.

I also assumed that 10 per cent of the surface area on the waste packages that experience localized corrosion is failed. So, again, this calculation presents a potential impact on system performance. 10 per cent of the waste packages failing due to localized corrosion caused by an early event in the first 1500 years, and that 10 per cent of the surface area on those waste packages are failed. And the probability weighted mean annual dose for that calculation is around .02 millirems per year. That's just an example.

I'm going to summarize my talk. Our models will account for variability and uncertainty in in-drift TH processes. I presented to you some key conclusions relevant to localized corrosion, and in particular, drip shield damage in the seismic scenario class allows seepage to reach the waste packages, and conditions for localized corrosion may exist following an early post-closure seismic event.

And, we feel that it's highly unlikely that dust deliquescence on the waste package will initiate localized corrosion. And the models and the approach that I presented here today will allow us to quantify how unlikely it is.

Also, I presented two examples that estimate the impact of localized corrosion due to dust deliquescence and the evolution of seepage on the waste package. And, with that, I conclude my talk. I'll take some questions.

CORRADINI: Thank you, Bob. Questions? Dave, Dan, Ron, Paul.

DUQUETTE: Duquette, Board.

Let's go to Slide 22, which is the one just before this one, I think. You say it's highly unlikely that dust deliquescence on waste packages will initiate localized corrosion. I would then like you to go from there to Figure 13, please.

If I take a look at the diagram on the right-hand side, and I realize that it's well known that corrosion under dust can start in about 50 to 60 per cent relative humidity. A very quick analysis of that indicates that somewhere around 500 years, your temperature will be somewhere around 100 to 110 degrees, and your relative humidity will reach the 60 per cent value. And, I would argue that from the data that you've presented so far at previous meetings, that you would have a very high probability of having crevice corrosion at least occur at that temperature and relative humidity.

And, so, I would take issue with the argument that it's very unlikely to occur. I think it probably will occur if you have dust in the system, especially if I take the water that has the lowest nitrate to chloride concentration. So, I think there's still a major issue as to what the relative humidity will do and what the temperature will do, and what that chloride concentration will be relative to the issue between the two.

I do want to give you a great deal of credit for now analyzing data, and I know you've done it in the past, but what happens if you do get localized corrosion? I mean, that's really the direction you ought to be taking. But, I just want to disagree with the comment that it's unlikely to occur. I think that this indicates that it's likely to occur rather than unlikely.

MACKINNON: Let me make a few comments.

As I described on the earlier slide, the data, the model that's based on the data shows that the initiation of localized corrosion is a function of several variables, including not only nitrate to chloride ratio, but also pH and nitrate and chloride concentrations themselves. So, it's a rather complex model. And, so, just by looking at these nitrate to chloride concentrations, what you really need is a complete picture. You need the pH and the individual concentrations of these ions as well to conclude whether or not localized corrosion occurs.

Now, we have done some preliminary analyses with our models, and our model tends to indicate that nitrate to chloride ratios above .1 are beneficial. It looks like the nitrate to chloride ratios for our environmental conditions have to drop below .1. But, you're right, it's a complex process, and it depends on the values of the other variables in the system. And, that's what our model is going to allow us to do, is to exercise that model over the range of conditions, and evaluate those variables to quantify whether or not localized corrosion will occur.

DUQUETTE: Thank you.

CORRADINI: Dan and Ron and Paul.

BULLEN: Bullen, Board.

Actually, I have a series of questions, but I'll try and keep it short so my colleagues can have a shot at it, too.

Can we go to Slide 7, please? My first question is with respect to drift seepage not occurring on the crown when it's above the boiling temperature. And, I guess the question that I have for you is how does this compare with the data that you've identified from the drift scale heater test where there was a discoloration spot that was identified maybe associated with dripping from a rock bolt? It seems to me that the temperatures were greatly in excess of boiling, and yet we still had some seepage.

And the follow-up question to that is isn't this a function of sort of the flow rate? Now, we had a three inch rainfall in Las Vegas in 90 minutes, which may have had a different percolation and infiltration rate associated with it. It may have been a one in thousand year event. If something like that happened, would you expect that there's any way to overwhelm the temperature effect at the crown and actually have periodic flow from the crown surface?

And the final point from that is, and I harden back to my long-term memory on the large block experiment where it rained on that, and homogenized all the heater temperatures to 96 degrees C. when we had fast flow there. So, I guess I just wonder how you can tell me how the data support the claim that there is no seepage when it's above boiling?

MACKINNON: Well, I'll give you an answer in part, and then I'll let Bo address the issues of data.

But, I'm primarily familiar with the seepage model itself, and my analysis of the seepage model and its implementation in TSPA. And, based on my review of that analysis and model report, is that the model is exercised over a wide range of uncertainties, uncertainties in fracture properties, for example, capillarity, fracture permeability. It's also exercised over a wide range of percolation fluxes.

It's validated against test data, and that's presented in the analysis and model report. In addition, it does a fairly extensive analysis looking at alternative conceptual models. In fact, in particular, the focusing of flow in discrete fractures. And, the conclusions of that analysis are that seepage will not occur under the range of conditions considered when drift wall temperatures are above boiling.

If that doesn't answer your question, I'll--

BULLEN: Bo, go ahead. Are you going to address the drift scale tests?

BODVARSSON: Yes. This is Bo Bodvarsson, Lawrence Berkeley Lab.

The drift scale test, you're right, we saw some coloring at the ceiling, but it's not at all sure that that's due to seepage. It could be localized. A lot of the present data doesn't come from seepage, and the boiling point was way far away from the drift. So, it's very difficult for me to understand that.

The other part with the thermal seepage, we did two different models. One of them was extremely conservative. This is a model that we actually focused flow into a single fracture, and this is actually documented very clearly in the thermal hydrology model--that is an AMR that is completed now--that we actually focused water and let it go straight down a fracture that connects straight to the crown, and then we investigate the amount of water we can focus to get through the boiling zone, and that turned out to be very difficult for all the parameter values that we had obtained at Yucca Mountain to actually get the thermal seepage in.

That is supplemented by our standard model that was utilized in TSPA, which is the regular dual permeability model that also shows that we have great difficulty getting seepage into a drift in the thermal period. And the reason is simple this: there is a tremendous amount of power coming from the waste packages, and there is not a lot of water going through the mountain, and the amount of energy needed to vaporize that water is not very much. So, that's the reason for that.

There was a fourth component to your question, but

I forgot that one. Is that okay?

BULLEN: Actually, you've answered the part that--I can move on to a couple more things, and then I'll leave some more time.

You mentioned that these TSPA calculations were based on models that were developed for the final EIS. Are the corrosion models in the final EIS TSPA temperature dependent?

MACKINNON: No. These calculations that I presented did not have any waste package failure, except for on the one example, there was one early waste package failure in each realization.

BULLEN: But, the corrosion models themselves were not temperature dependent. So, your assertion that as you got past the 1500 year pulse, you basically said, you know, the localized corrosion goes away, which sort of begs the question if it goes away after 1500 years, and you're below the boiling point, why you go there anyway. But, that's the rhetorical question. You don't have to answer that one. I always ask that question.

I guess the question is that these were not temperature dependent corrosion models, will they be in the final TSPA that you're going to do?

MACKINNON: Yes. The waste package, and right now, the current waste package degradation model that will be implemented in TSPA is a function of temperature.

BULLEN: Okay, last question I promise, Mr. Chairman.

Drip shield seems to be very important, and the drip shield failure mechanism is only seismic. Is that because rocks fall in and it corrodes, or is there actually movement of the drip shield? And the corollary question to that is how are the drip shields supported? Are they just on crushed tuff, or do we still have an iron base rail system that they sit in? And, how does that fail over the 1500 years of high temperature, high relative humidity? And, how do you deal with the fact, I mean, you're very much dependent on these drip shields, because you don't fail any of them, when you failed all the waste packages and you survive, and when you fail a few per cent of them, you get a rise?

I guess I just am very concerned that the drip shield stability, including drift stability, I mean, if rock falls, the only way it fails is by corrosion, how do you deal with that?

MACKINNON: Okay, you've got several questions.

BULLEN: Four in one.

CORRADINI: You get to pick one.

BULLEN: Okay, so we can get to other members. Pick one that appears appropriate.

MACKINNON: I'm going to pick the most difficult one.

BULLEN: Thank you.

MACKINNON: Well, have an extensive set of analyses that look at several failure mechanisms for the drip shield, including induced cracking, stress corrosion cracking, general corrosion, microbial influenced corrosion. And, the analyses, many of them are nearly complete, document the technical basis, and validation of the models that support the fact that the drip shield will not fail in a nominal scenario.

BULLEN: I look forward to seeing those. Thank you.

CORRADINI: Ron, Paul, Priscilla.

LATANISION: Latanision, Board.

I want to turn to Numbers 16 and 17 in your presentation. Let's go to 16 first. The second bullet refers to work done at both the project and at CNWRA. My understanding of the work done at San Antonio is that (a), it does not exceed 95 degrees Fahrenheit, (b), it looks at cold worked material, and (c), it looks at cold worked and aged material. And, also, I should add (d), it looks at welded structures.

Now, I don't know whether the project has looked at--I know it's looked at different temperatures--but I don't know whether the project has looked at the dependence on cold worked structures or on welded structures from the point of view of crevice corrosion initiation. But, it seems to me very clear in the Southwest Research work, that there's clearly a dependence. And, I would argue that on your regression analysis and the equations shown on Slide 17, that you ought to be incorporating such issues as cold work and welding, and so on and so forth, and looking carefully at the susceptibility in terms of localized corrosion. Any comment?

MACKINNON: Well, I can't address the specifics of your question. I can address certain aspects of your question.

I think you would have to have a waste package corrosion expert here to answer those questions.

But, first, let me say that your conclusion that the Center data covers a lower end temperature range, well, that was one of the reasons why the data was combined with the project data. Because I'm sure you're familiar with the project data. It focuses more on higher temperatures and more concentrated solutions. And, in order to develop a model that we could implement without extrapolation, that's our goal, that we needed to use the Center data that was developed at these lower temperature and chemical conditions.

Now, in my discussion with the corrosion experts, and I may be misstating it here, that the use of the crevice repassivation potential from those CPP tests is a very conservative measure of when localized corrosion occurs. And, therefore, they have concluded that the use of that data, and the use of crevice repassivation potential is a valid approach.

LATANISION: I'm not arguing with the concept that that change, that Delta E is a reasonable way of looking at this. What I'm concerned about is that we have a waste package that includes wells that are going to be peened or processed in some way to induce residual compressive stresses. There are going to be aged, given their history and service in the repository, and what I'm pointing out is that there seems to me to be an omission in terms of these expressions that do not take into account that issue of waste package fabrication. And, it concerns me. I mean, if in fact--well, you know the Board is concerned about the temperature issue. I mean, that's no secret.

But, I think if choosing or going to a lower temperature operating mode is not a possible alternative, given the stage of the art in terms of the preparation of a licensing application, then the question that occurs to me is how much variability, or how much margin is there in the question of the waste package design. Because, if welds and peening and thermal aging are issues, then that seems to me to be the only other alternative, is to look at the design of the package, because I'm quite concerned about localized corrosion. I mean, I really do think there's an issue here.

MACKINNON: Well, your concerns are noted, and I'll take them back to our corrosion experts.

CORRADINI: Paul?

CRAIG: Paul Craig, Board.

Most of my technical questions were covered. One technical question, a general one. When you looked at the uncertainty, did you take into account the enormous range of uncertainty in the thermal conductivities, especially in the lower lith?

MACKINNON: Well, that's a good question.

CRAIG: The data is really very limited.

MACKINNON: Well, actually we have, I don't know if you've seen this analysis, but we did have analysis completed that looked at all of our field test data, looked at well logs, and there were some modeling assumptions made, and that information, in combination with geostatistics, we developed a set of I believe 50 realizations, spatially variable thermal conductivity fields that are uncertain. And, so, we have had an effort in trying to quantify the uncertainty in the thermal conductivity. And, then our objective here is to represent that uncertainty, and we feel that we've done a reasonable job.

CRAIG: Okay. Well, the concern there would be that because of the sparsity of data in the lower lith, especially your uncertainty bounds on the temperature, may be under estimated and, indeed, the time spent at the higher temperatures may be greater than is suggested by those curves.

But, I want to move to a higher level consideration here. There seems to, judging by your presentation, and let's go back to Figure 7, which is probably the best one to look at this, where you talk about the importance of the drip shield to prevent seepage water from reaching the waste packages. And, later on in your neutralization examples, you maintain the drip shield, by and large. So, the drip shield is emerging in your presentation as having an exceedingly high level of importance relative to its importance in the past where the C-22 alone was considered to be adequate to do the job.

The message which is coming across to me is that the C-22 is now perceived to be vulnerable, and it requires--requires the drip shield in order to work. And, that means that two things have to happen. One is the drip shield has to actually be there, which suggests that maybe it should be installed right away rather than waiting for a long time, and, secondly, it needs the kind of analysis which has been going, and experimental work which has been going on in C-22. And, as we know, the more recent work on C-22 has disposed just these set of problems which had not been previously anticipated. That was Joe Farmer's work last time.

It would be very, very interesting to know what would happen if one were to do similar work on the titanium proposed for the drip shield, because it's now looming as at least as important as the C-22, and possibly even more important. Or am I missing something?

MACKINNON: Well, let me make a comment. Really, my intention this morning was to convey to you that we are taking a reasonable technical approach to evaluating the range conditions on the waste package surface. And, that's what our focus is.

We don't know if localized corrosion will occur. And, in fact, the project's technical basis and test data that was presented and discussed at the last meeting indicate that localized corrosion probably will not occur. But, we want to further verify those conclusions.

Now, indicating that the drip shield will not fail is simply part of our analysis. We haven't concluded that the drip shield is necessary, because we haven't concluded that localized corrosion will occur. What we want to do is simply do a reasonable technical job on evaluating the entire range of conditions on the waste package surface, including uncertainties, and do a fair evaluation of whether or not it is a problem. That was the point of my presentation.

CORRADINI: Okay. I want to move on to the next talk. Priscilla will get the last word in.

NELSON: Nelson, Board.

I wanted to tell you how much I've enjoyed seeing Slides 10 and 11, for example, and getting some graphical inputs on spatial variability and trying to handle uncertainty in time. And I encourage there to be additional casting of these issues, because I think they're important.

I guess what I'd like to phrase for you is a question. Considering what you're assuming about natural circulation, ventilation, which I'm not sure what happens in this model, considering variability in thermal conductivity, spatially and through time, in the different materials, considering the interactions between drifts, and the fact that not everything is done all simultaneously, so you have real time effects, and considering the fact that you've got a drip shield that potentially, if it's used, creates a separation between the in-drift environment and the end drip shield environment that could potentially be something to be understood, I wonder overall if there's thinking about identifying what you might think of as microclimates, not so micro as we're looking inside of crevices, but the general arrangement of these parameters, include seismic rock fall that may shut off circulation, that may change environments locally and spatially, and cause conditions to be far from the average, and might localize the processes of corrosion. Are you thinking about thinks like that as you do this, or are you keeping this global focus on the overall mountain? I mean, is there a plan to look at the local microclimate consideration?

MACKINNON: Well, we actually have an effort right now. The title of that analysis and model report is In-Drift Convection, and I believe Mark Peters presented some of the results from that analysis at the last Board meeting. I think he showed a film clip. And, really, the focus of that analysis is to look at this waste package scale heat transfer processes, what occurs between the waste package and the drip shield, and between the drip shield and drift wall, how the natural convection mixes conditions in the repository. So, we are analyzing those more detailed processes with the idea that if anything rears its head that's potentially important to total system performance, that we will include it in our TSPA model.

NELSON: I guess I remember what Mark was drawing, and it's a very complicated possibility. There's lots of parameters involved. So, the idea of I think just really searching for those microclimates that are really problem causers and trying to identify what triggers them in the scenario, there's room to have what Mark was doing, interfacing with this kind of an effort where you're looking at a larger scale spatial variability as well. If it's not being done, I think it would be good to have a think on that.

MACKINNON: Okay, thank you.

CORRADINI: The east side of the room feels unwanted. So, Mark, the last, last.

ABKOWITZ: Okay, thank you. I think I've used my chip for today. Abkowitz, Board.

If we could go to Slide Number 5? And this is just kind of a view at 30,000 feet question. The last 25 to 30 minutes have been spent discussing a variety of issues related to the waste package and the environment that might be there and whether corrosion will occur or not occur, and I just want to make sure that we can get this back to the big picture. Is it appropriate the take-away from this slide is that the waste package temperature never goes above 90 degrees Centigrade, all these problem disappear; is that correct?

MACKINNON: I would say no. I would say that based on--well, it depends. You know, based on, for example, on our localized corrosion model, as I said, it's a function of several different independent parameters. One of those is temperature. So, I could not say definitively that, for example, our issues with localized corrosion would go away. And, in fact, the higher temperature operating mode keeps water away from the waste packages. And you do not get low nitrate to chloride ratios until you get out to higher relative humidity and cooler conditions.

So, I guess my answer to your question is I couldn't say definitively yes.

ABKOWITZ: Yes.

CORRADINI: Thank you very much.

Okay, our final presentation of the morning on the agenda is a group presentation on studies of Chlorine-36 at the Yucca Mountain site. We will begin with an overview by Bill Boyle of the Department of Energy, and then James Paces of the U.S. Geological Survey, and Robert Roback of Los Alamos National Laboratory. They will tell us about the Chlorine-36 studies that have been conducted by their organizations.

Dr. Boyle is the Director of the Postclosure and License Acquisition Division in the Office of License Application and Strategy in the DOE's Office of Repository Development. Dr. Boyle has degrees in geology and civil engineering and about 20 years of experience in site characterization, design, and review of repositories and other types of underground excavations.

Dr. Paces is a research geologist in the Yucca Mountain Project Branch of the U.S. Geological Survey. As a member of the Environmental Science Team for the last 12 years, Dr. Paces has participated in characterizing the Yucca Mountain site through isotope, geochronological, and geochemical studies of surface deposits, groundwater, whole rocks, and fracture minerals.

And, finally, Dr. Roback is a Technical Staff member with the Geochemistry Team at Los Alamos National

Laboratory. Dr. Rock's interests and expertise are mainly in geochemistry, with an emphasis on isotope geochemistry and geochronology, and he also has considerable professional experience in structural geology, field geology, and tectonics. Dr. Roback has served as the Principal Investigator in the Los Alamos's Chlorine-36 project since 2001.

As I understand it, Bill will start off with an overview and introduce the other two speakers.

Bill?

BOYLE: Good morning. Thank you for the introduction and the opportunity to make this presentation. I'd also like to thank Katie Miller and Martha Pendleton for putting my presentation together. And I'd also like to thank all the scientists involved in the Chlorine-36 studies through the years. It's a challenging problem, and it's fascinating, and the discussions have been very interesting.

Although Mark Peters has made many brief presentations on the progress of the studies through the years, it's been a while since we had a dedicated presentation on this topic to the Board, so Dr. Reiter of the Staff suggested that there be a discussion of the background of the Chlorine-36 measurements. So, my role is to provide some background on Chlorine-36 measurements, and also context for the talks by Jim and Bob.

We measure chloride because it is soluble in water, and its presence in water, or as a salt, can perhaps tell us something about the amount or speed of the water that carried it.

The use of Chlorine-36 as a dating isotope is well established, particularly for groundwater samples and ice core specimens. The project has extended the method to leached specimens of crushed, fracture, unsaturated porous rock.

Of naturally occurring Chlorine, about 75 per cent is Chlorine-35 and about 25 per cent is Chlorine-37. Naturally occurring Chlorine-36 is relatively rate, occurring only as parts per trillion. The common unit of measure of Chlorine-36 in age dating studies is relative abundance, expressed as a ratio of Chlorine-36 to all chloride present. The Project's results have an approximate range of 200 to 8000 times $10^{-15}$. Los Alamos and the USGS have chosen to represent this ratio differently on the axes of their charts. Whether or not the exponent is plus 15 or minus 15 on the charts, if the number in front of the exponent is the same, the ratio is the same, and the results are the same. In the rest of this presentation, I drop the exponent entirely and only use the multiplier before the exponent.

Chlorine-36 is produced naturally by cosmic rays striking the atmosphere. The degree to which cosmic rays interact with the atmosphere is largely controlled by earth's magnetic field. Because earth's magnetic field varies in time and space, the production of Chlorine-36 varies in time and space as well. The data shown here are for the latitude of Nevada. The upper chart shows a calculated ratio, and the calculated ratios are matched to measure data. In the lower slide, the axes are slightly different, 50,000 and 40,000 years.

The present day value prior to 1945 was about 500. About 40,000 years ago, the ratio was about 1000. Knowing the initial ratio, and that the half-life of Chlorine-36 is about 300,000 years, one can date waters or salts that contain Chlorine-36.

The weapons testing in the South Pacific after World War II created another method of age dating using Chlorine-36. The testing produced a transient spike in the ratio, a spike much greater than had been produced naturally. This spike is referred to as "bomb pulse" Chlorine-36. If one finds evidence of this spike, it is evidence that some part of the water is no older than the bomb testing. In the presentations today, any ratio greater than 1250 is taken as evidence of bomb pulse.

In addition to Chlorine-36, there are other data sets relevant to water flow through time at Yucca Mountain. The USGS had measured many other data sets. This is a cover from one of many reports relevant to water flow through Yucca Mountain. The diverse data sets include the distribution and amount of secondary minerals such as calcite and silica that have been deposited in fractures at Yucca Mountain; stable isotope ratios; and radioisotope age dating.

Integrating these diverse data sets, the USGS was able to tell a history of the unsaturated zone flow at Yucca Mountain over approximately 10 million years. As would be expected with many data sets, not all fit the history equally well. The Chlorine-36 data set is one data set that did not fit as well as some others, as is documented in this report and other reports.

The differences are not indications of the goodness or badness of any of the data sets. The differences could be the result of different temporal resolutions of the methods. If elevated Chlorine-36 indicates a preferred path for water in the last 50 years, it is not clear that any of the USGS data sets could similarly identify such preferred paths.

However, if one postulates that the preferred paths are relatively stable in time and place for longer durations, then the expectation becomes that one might see evidence in the USGS data sets of these preferred paths. Although such evidence was found, in other places the evidence did not match.

Because of the new nature of the technique being applied to crushed, fractured, unsaturated porous rock, the project decided to conduct a Peer Review. This is a cover from the Peer Review Report.

One conclusion of the Peer Review is that the elevated Chlorine-36 ratios do represent a bomb pulse component.

In addition to the challenging issues that led to the peer Review and its recommendations, there was another motivating factor that led to what is referred to as the validation study, the subject of today's presentations. This other motivating factor is that a fundamental tenet of good science is that the results of a test ought to be reproducible by an independent group.

With these considerations, the project decided to proceed with a validation study. The USGS and Lawrence Livermore National Lab. were chosen to make the independent measurements, but Los Alamos would continue to make some measurements as well, to facilitate comparison and understanding. The very first public display of any of the results of the validation study was made at a meeting of this Board in May 2000 in Pahrump.

For those at Pahrump, they saw that the initial results of the USGS/Livermore validation study did not match the prior Los Alamos result. This slide clearly shows the nature of the difference. The blue squares don't match the clear circles. The investigators from the different groups quickly identified possible reasons for the difference. The main cause of the difference was suspected to be variations in processing and leaching of the specimens.

As time went by, possible causes of the differences were eliminated. Jim Paces will identify some of the eliminated problems. An example of a possible cause of the difference is the sampling method, that is, coring versus hand sampling. However, there is evidence that use of either technique may not substantively affect the results.

You will see that Los Alamos has found bomb pulse when coring was used and also when hand sampling was used. In addition, you will see that whether coring or hand sampling was used, the USGS/Livermore results do not show clear evidence of bomb pulse. The Los Alamos and USGS/Livermore results do differ from each other, but both data sets are internally consistent, and Los Alamos shows that either sampling method can produce results that have bomb pulse values.

As the work progressed and insights were gained on eliminating differences in results caused by variation in techniques, a wonderful meeting occurred in Denver in January 2002. The two groups had produced results that matched. And that's shown, the upper slide is a concentration, and the more important slide is the Chlorine-36 ratio, Los Alamos versus Livermore, and if all the points fell on that line, it would be a perfect match. The match is pretty good here.

This meeting produced an even more wonderful result. It was remembered that years before, Los Alamos had made ten measurements on core from Niche 1 in the Exploratory Studies Facility. Of the ten measurements, nine had bomb pulse, and the tenth had an elevated reading that was almost bomb pulse.

What was even better is that some of the core had been preserved. The remaining core was obtained, split between the two groups, and the Chlorine-36 ratio measures.

Optimistically, one might have thought that with this retest of Niche 1 core, the two groups would have produced the same result, either high or low. Instead, the results were heartening for those that like consistency. Los Alamos reproduced its bomb pulse readings from before, including the highest ratio ever measured from the Exploratory Studies Facility, and the USGS and Livermore reproduced their consistent result of not finding clear evidence of bomb pulse.

So, where are we at? A report is currently in review to present and summarize what is known from the Chlorine-36 measurements in the Exploratory Studies Facility. The report also looks at other data that are relevant to possibly explaining the differences in the Chlorine-36 data sets.

One of the most interested groups in that report is shown on this title page. This is a title page from a task that the Department, the Project has with researchers from UNLV and elsewhere. Because Los Alamos and USGS staff had, and have, much higher priority work to support the license application, they were not readily available. The project brought in a new group, this one, not to simply make yet another measurement of Chlorine-36, but instead with a goal of trying to determine why there are differences in the results to date. The task was started last month, and is estimated to last 18 months, and at least two of the investigators are present in the audience today.

The study may not be able to discern the reasons for or resolve the differences in the results. If that happens, the project will continue as it has for years on this topic. The project's model for unsaturated zone flow will continue to be consistent with the Los Alamos fast path, bomb pulse data. Although the USGS/Livermore results do allow other conceptual models to be considered, for example, all the water at the repository horizon is very old, the UZ model will continue to be consistent with the Los Alamos data set, because it is the most conservative data set the project has with respect to system performance.

Some have raised an issue that as long as the difference exists, the scientific credibility of the project is at stake. I do not see how this is so. As mentioned before, all good science is founded upon reproducibility. The project has tried to reproduce the Chlorine-36 results and has not been able to do so fully.

What would undermine the credibility of the project would be to force results to match in some arbitrary way. All the groups involved are thorough and profession. They have all closely examined their test methods and results, and they all stand behind their results. The face that the data sets do not match yet may simply be science at work, particularly in applying a new technique.

Jim and Bob will also show data about Tritium, another age dating isotope with a bomb pulse. Although they agree on the measured data, they interpret them differently, but in both cases, the interpretation is consistent with their interpretation of the Chlorine-36 data.

And, at this point, I'd like to defer questions and let Jim and Bob make their presentations, and then we'll all take questions at the end.

PACES: Thanks, Bill, for the introduction. And, thanks to the Board for your continued interest in this topic.

I am happy to report that the analytical stage is over in this investigation. We've had a couple of months to think about the data, do some evaluations, and as Bill mentioned, we do have a rather lengthy report working its way through checking and review at present. And, what I'd like to do in the next ten to fifteen minutes is try to summarize this rather lengthy report.

Bill did a good job in sort of capturing the earlier history. I'd just like to show this by way of justifying a little bit more what was done. Initially, the data set came about by following the TBM as it constructed the ESF. And, so, as you see in this diagram, we've got distance from the north portal on the X axis, and the Chlorine-36 ratio on the Y axis, and the data are shown in different symbols for when they were reported in different reports.

There are several rather exciting and surprising things that came out of this investigation, and that was initially, that there was abundant bomb pulse Chlorine-36 reported at depth at the repository horizon in the Topopah Spring welded tuff.

A further surprising thing was that after Station 44, and in the second year of investigation, for the most part, no bomb pulse values were observed after the ESF Station 44. And, June Fabryka-Martin, the principal investigator at the time, explained the data by a rather elegant model requiring bolts that cut the PTn and allowed rapid flow through the non-welded units. Also required variations in infiltration, as well as PTn thickness. But, the elevated Chlorine-36 values have always been difficult to reproduce.

As Bill also mentioned, in January of 1999, there was a request for a validation study. Various different participants have been involved, and that has changed a little bit over time. However, the goal of that validation study was basically to verify the presence of elevated Chlorine-36 over a limited area where it had been reported earlier.

We chose to focus on the Sundance Fault zone as the primary target. This is a 165 meters zone from which there was a large percentage of bomb pulse values observed by June and her co-workers, and we felt that it maximized the probability of reproducing a bomb pulse signal which was commonly sporadic throughout the northern ESF.

So, what we ended up doing was getting the project to drill a series of bore holes typically on five meter centers. The bore holes are shown here as vertical lines, essentially four meters depth, and this is the fundamental new samples that we used to try and reproduce these data.

This next slide is going to be on Niche 1, and we're going to spend some time talking about Niche 1. I failed to point out that that occurs right there at Station 36 plus 55.

So, as Bill also mentioned, we did have this marvelous opportunity to compare samples that had been analyzed previously at Los Alamos by June Fabryka-Martin. Here are the three bore holes represented schematically. The red intervals are the ten analyses that were done and reported back in 1998. The remaining material was split between Bob Roback and us in Denver, and we tried to, as much as possible, have overlap in these samples.

We were concerned about not having enough Chlorine to measure, so we combined multiple intervals. And, we consider these samples to be very critical in this evaluation, because they do represent, as nearly as we have possible, materials that were identical analyzed in both different places.

The initial results. Initially, Livermore was completely responsible for the analytical aspects, and they decided to leach the material in an active way, what's become known as the active-leach method for seven hours in a slowly rotating tumbler. And these leachates ended up having high chlorine concentrations and low Chlorine-36 ratios, values between 40 and 275 times $10^{-15}$. And, these were the data that were reported to the Board in the spring meeting in Pahrump of 2000.

After that period, all parties agreed that this was too aggressive of a method for extracting chlorine, and so it led to experiments on other leaching methods. Basically, it was determined that passive leaching extracted most of the labile chlorine after only several hours. And, the result also indicate that they're relatively insensitive to small differences in either the particle size or the amount of times that were used for leaching.

So, the final protocol involved passive leaching of between 1 and 2 kilograms of rock for one hour. We felt that although this challenges the ability to analyze the chlorine that we got out of it, the shorter leach times would have the greatest chance of identifying the youngest, most labile chlorine components.

Therefore, from now on, I'm only going to talk about the results of passive leaching, and basically we ended up having 34 analyses of Sundance Fault core and six analyses of Niche 1 drill core that were crushed at various different places, including the Sample Management Facility about 20 miles north of here, or maybe it's 30 miles north of here. At any rate, we also had material that was crushed at Golden, as well as in Denver at three separate laboratories. And, for these data, we're going to compare the validation results to the original LLNL results.

You can see that we have much lower concentrations of chlorine and much lower Chlorine 36 ratios, varying from 137 to 717, compared to 363 to 4105. This is a graphical representation up here in the upper right-hand corner, where we're showing the original data as the yellow symbols, and the new data as the red symbols.

Bill also showed these data. This essentially was material that was crushed at the SMF, leached at the USGS, and then we split the material. We took a liter bottle and we sent it to Bob, we took a liter bottle and we sent it to Greg Nimz at Livermore, and they both prepared it in their own way, spiked it, analyzed it either at Livermore or at Livermore and PRIM, the PRIM Laboratory at Purdue, and as Bill mentioned, we found that we got quite a bit of consistency, both in terms of the chlorine concentrations and the ratios that were obtained from this study.

Therefore, this we feel is an indication that the inter-laboratory differences that do exist can't be caused by either the spiking methods or the target preparation methods or the accelerator mass spectrometry step of the process.

Bill also showed these data, and this is a comparison of the USGS/Livermore analyses shown in purple, versus the original Los Alamos data, and basically, there are two very separate trends. Again, this is all the passive leaching data, not the earlier active leaching data.

So, what we see here then is that for the USGS/Livermore data, there is a horizontal trend at low values. There's no correlation between the ratio and the concentration, and when we plot it versus--actually, this is plotted against the reciprocal concentrations, and we do this because mixing relations will show up as straight lines on this type of a plot.

We find this significant because, in particular, these high numbers for the reciprocal concentration indicate very low chlorine contents. These are the most susceptible to contamination, and the fact that we see a uniform value, between 300 and 500, indicates that we don't see substantial evidence for mixing of different sources.

The original LANL data, on the other hand, shows the highest Chlorine-36 in the samples with the lowest chlorine concentrations, and this trend could be consistent with mixing of a high Chlorine-36 concentration with a meteoric water, or this little bitty red triangle down there represents rock chloride, leachable rock chloride with a very, very low concentration. Again, this is now comparing core samples to tunnel wall samples.

As Bill also mentioned, we have the ability now to look at samples of core that were analyzed at USGS/Livermore, and core that was more recently analyzed by Bob at LANL. And, again, we see very different results. Bob has, and he will probably talk about these, he has seven analyses from these three different bore holes. Results range between around 1000 and 8500, and four of those seven analyses are indicative of bomb pulse.

A curious thing which we didn't expect, I don't think, is that the fine fractions, unlike June's original data, Bob also analyzed the finest fraction, as well as the coarser fractions, and he ended up finding the highest concentrations, which we did expect, and the highest chlorine ratios, which we didn't expect, in those fine fractions.

The USGS/Livermore data for six analyses had Chlorine 36 ratios ranging from 226 to 717 times $10^{-15}$, and they're statistically identical to all the rest of the validation core. Therefore, because we're looking at the same material from the same boreholes overlapping, we interpret that this is an indication that you can't explain these differences by a difference in sampling approaches.

We also felt it was important, and I think this was a Peer Review Panel recommendation, that we measure other isotopic tracers in order to get a better handle on what the Chlorine-36 was telling us. As Bill mentioned, Tritium is an important isotope in that regard. You have to get water out of the rock in order to analyze it. So, in these, actually there was a total of 50 bore holes in the validation study. Pore water is extracted from the welded tuffs, were analyzed for chlorine, and the data themselves, as a total, we interpret the cutoff value for these as around two Tritium units. And, there's a supplemental slide which shows the justification for this, and I won't get into that right now.

All of the validation study data, the data from the validation study core, was lower, or within error two Tritium units. There are elevated Tritium values in ESF south ramp, and in the ECRB cross drift. However, bomb pulse Tritium and Chlorine-36 generally aren't spatially coincident. And, I think we would admit that the Tritium data in the cross drift, and again, there is a supplementary slide, if we get there, that shows some of the perhaps difficulties in extracting the pore water and analyzing it. We feel that those data need some additional work.

We also, Mel Gasgoyne up at AECL analyzed a lot of these samples for uranium isotopes, 234, 238 uranium isotopes from both the Sundance zone and the ECRB cross drift. Basically, he found no differences in those two data sets, and wrote a paper on that in 2002.

Strontium was measured in leachates of Niche 1 drill core, and the results there are basically there's no statistical differences between the pore water from Niche 1 versus other areas, and the values from Niche 1 indicate to us that there's a strong likelihood that the pore water has a substantial residence time in the PTn.

So, a few slides on summary here. The USGS/Livermore data from the validation study bore holes don't show a bomb pulse signal despite the shorter leaching times that we were using, and the resulting Chlorine-36 concentrations. Again, this is important because if you're going to find it anywhere, you should find it in these very short leaches.

Basically, we did have agreement when we leached them in one place and sent them off to both different laboratories. There were no differences. So, we're agreeing within analytical error on that part of the study.

USGS/Livermore data shows that the Niche 1 core samples are indistinguishable from results from the rest of the validation core, and the new LANL data indicates that the Niche 1 core samples yield bomb pulses rather routinely, and including the highest value seen in the ESF. And that, again, Tritium data were measured. They may indicate areas of rapid percolation. But, they're generally not coincident with the same areas that have high Chlorine-36 results.

Some of the remaining issues. Again, I don't think we can say that there's conclusive results regarding the presence of bomb pulse. We're unable to reproduce the original data. However, Bob was able to continue to identify elevated values.

I think it doesn't need to be said that interpretations remain controversial, but I think we can exclude a couple of causes. And these, I believe, include differences in sampling strategies. And I think that the

Niche 1 data clearly can rule out problems between coring versus, you know, sampling off the tunnel walls themselves.

We also evaluated at USGS the differences between mechanical versus hand crushing. This had come up as a potential reason for differences. The data we have indicates that there's no substantive differences between hand and mechanical crushing.

I think that different leaching experiments indicated that this is a rather surprisingly robust system, in that small variations in both grain size and leach times don't dramatically effect the results. And, I think we can easily exclude the target preparation and AMS analyses.

What can't we rule out? I guess there's a possibility of contamination. This looks like it's an analytical problem to us. There is a possibility of contamination with low Chlorine-36 source in the USGS/Livermore environment, so that the bomb pulse values that we should be seeing are somehow masked by this component that we're adding.

However, we don't think there's a real strong evidence for that. There's no correlation between concentration and Chlorine-36 ratios. There's no systematic differences in the ratios that we see for samples crushed at the different laboratories. There's no evidence for anomalously low Chlorine-36 in any of the blanks that we've run, including a silicon crushing blank to try and evaluate how much we were adding in the crushing process itself.

And, in general, the validation results are broadly similar with the ESF south ramp samples analyzed by Livermore where no bomb pulse samples were reported.

I don't think we can exclude the possibility of contamination with elevated Chlorine-36 source, either in NTS environment, the Nevada Test Site environment, or in the LANL laboratory environment that would result in bomb pulse values.

June Fabryka-Martin in an earlier report did a good job of identifying possibility of contaminated equipment that was used in test Cell C in Area 25 that was brought over and used for collection of some of the surface based cutting samples. We also see the correlation of high Chlorine-36 values in low chloride samples, which could be susceptible to Chlorine-36 addition. Unfortunately, we don't have any crushing blanks per se that were measured at LANL, so we can't really address that. But, in several reports, Chlorine-36 contamination has been recognized in laboratory environments at LANL. It has not been described in very much detail.

We also see small, but systematic, elevations in Chlorine-36, both in LANL blanks and when we do regression intercepts of different data sets, and we admit that the measured blanks that exist don't necessarily provide enough elevated Chlorine-36 to rationalize the large values. However, there does seem to be a systematic difference in all samples between what we see in our study versus what Bob sees.

I think that we have some recommendations here. We think there is a need to do a detailed evaluation of the sample handling and processing that's been done in the past. We need to really rigorously evaluate the crushing and environmental blanks. We started to do that at both institutions, but that probably hasn't been done sufficiently at this point.

We probably need to look at additional determinations, both at LANL and at USGS, so we can use various different samples, both the existing validation study core, as well as samples that have been previously crushed at LANL. And, then we also have this interesting set of samples that were extracted pore water with elevated Tritium that we can verify the youngest of that water by looking at the Chlorine-36 leachates.

And, as Bill demonstrated, there is another independent validation of the validation study that's taking place right now.

So, with that, I guess, Bob, you're going to take over?

ROBACK: Thank you all for the opportunity to speak, my first chance to speak here in front of the Board.

Let me first say that a lot of what Jim said, I agree fully with. We have systematically been able to eliminate a lot of possibilities that might explain the differences in our data set. Unfortunately, we have not been able to nail down the real reason, despite our best efforts.

But, what I want to just present today is what we've done at Los Alamos, and try to get across to you that we have generated internally consistent data sets, data sets that are consistent with the earlier data sets produced at Los Alamos over the last several years, and that they are internally consistent within themselves, too, and that these data sets are very difficult to explain by analytical artifacts in and of themselves.

Just a bit of history, because it's I think quite pertinent to this whole discussion. I took over the project just a couple years ago in Fiscal Year 2001. There was a complete changeover in personnel at Los Alamos for this project, with changeover in technicians, PI of course. Because of the Serro Grande fire, we were forced to locate a new laboratory. We did so. The laboratory was in a non-radiological facility in a non-radiological part of the laboratory.

We, as part of the study, made a number of modifications to the sample processing methods, and in addition, we sent samples to Livermore, whereas they had been traditionally sent almost exclusively to PRIM. So, we had samples analyzed at both places. An important part here is that all of these changes then render this study as a self-validation, if you will, a validation of previous Los Alamos results.

Just to summarize, we worked on a number of different types of samples. We processed cross drift samples using the traditional methods, if you will, the first set of samples where I was more or less learning the methods. We also processed a number of cross drift samples looking at the effects of different leaching methods, an active versus a passive leach. We varied leaching times, particle sizes. I'll present some of those data.

The validation samples that were leached by the U.S. Geological Survey, and you've already seen those data presented, the Niche 1 results, I'll talk a little bit about those. And I also feel it my duty to speak a little bit about the blanks that we've monitored at Los Alamos.

First of all, let me talk about the sequential leaching studies that we did, because we were very interested in determining what would the effects of sample processing be on the Chlorine-36 ratios. The question came up early in the study when the first Livermore data set produced extremely small Chlorine-36 ratios with correspondingly high chloride values. So, we said, well, this really needs to be investigated.

Before we started, we had a conceptual model of what might happen. The ranges of Chlorine-36 ratio were already covered by Bill. If a bomb pulse is present, it may have extremely large ratios. But, for the most part, over the last 50,000 years, ratios are going to fall into the 500 to roughly 1200 or 1250 range, with ratios being fairly consistent over the last 10,000 years at about a ratio of 500.

So, the conceptual model is simply based on the fact that with continued leaching, or perhaps smaller particle size, the initial leaches should liberate the most labile chloride that's accessible on fracture surfaces, in the most accessible pores. With continued leaching, we might expect the ratio to drop, approaching a value of Holocene or perhaps Pleistocene waters. With continued leaching, we could expect one of a few different things to happen. Values might increase if we start to access older salts that are salts between 10,000 and 50,000 years. They may stay the same if we have just a consistent source. Also, we could liberate rock chloride with its very low Chlorine-36 ratio of roughly around 40.

These are some of the results of the progressive leaching experiments. The upper plot are cross drift samples, with leach time plotted versus Chlorine-36 ratio. You can see for leaching of roughly a couple of days worth of time here, which is the time that June Fabryka-Martin typically leached her samples. And then the EVAL sample, this is the reference sample that was collected. We performed passive leaches as well as an active leach, where we shook that sample and noted the Chlorine-36 ratios.

The results are consistent with the conceptual model, where we have a few samples which have the highest Chlorine-36 ratio in the earliest leaches, and then a decrease in the Chlorine-36 ratio, and then for the most part, fairly consistent values throughout the rest of the leaches. EVAL was the same thing with an odd exception here, which perhaps could represent a Pleistocene meteoric salt component. The active leach showed the lowering of the Chlorine-36 ratios through time.

One thing that needs to be pointed out here is that these three samples are three different size fractions from the same sample. They all yield small Chlorine-36 ratios, and very consistent Chlorine-36 ratios that stayed the same throughout the entire leach process.

So, just to summarize, in seven of the ten passive leach samples, the Chlorine-36 ratios were fairly uniform with time, and most of them are consistent with Holocene or

Pleistocene meteoric salts. However, three of the samples did show a decrease in Chlorine-36 ratio with time, with the highest ratios in the first leaches. And this could be interpreted to reflect a small component of a bomb pulse signal, although none of the ratios were actually elevated enough to be bomb pulse.

Eleven fractions from the same sample have uniformly small Chlorine-36 values. The interpretation there is that you've got a uniform source, and could be uniform adding a low chloride source that's either partially decayed Chlorine-36 or perhaps a uniform rock chloride source. And, of course, the active leach samples did show the decrease in Chlorine-36, which is the reason that we have now gone to this passive leach method.

What are the implications for previously produced Los Alamos data? Most of the data are generated in a 48 hour leach, will not reflect significant addition of rock chloride. It seems like we were there well before that, if it were present. But, rather, they reflect meteoric sources, Pleistocene and Holocene, and you cannot rule out the possibility, however, because all of these leaches were performed for 48 hours, for the most part, that some most labile Chlorine-36 with the most elevated ratio may have been missed. It may have been diluted.

So, let me compare results that I produced as part of the validation study to previous Los Alamos results, and these are for the cross drift plotted a distance from the start of the cross drift in Chlorine-36 ratios. You can see that the data are fairly consistent. Now, I plotted all of the data here, all of the leach fractions, and this large grouping here represents eleven fractions from one sample.

There's one sample that has a bomb pulse value of roughly 1300, and for the most part, though, the data are consistent between 500 and 1000, consistent with theoretical values of Chlorine-36.

Just another way to look at the data, the Chlorine-36 ratio plotted versus reciprocal chloride. Jim has shown one of these plots. The 11 fractions from the same sample here are highlighted in the blue circle, and you can see with the exception of these, if we eliminate those, that the data are very consistent throughout a wide range of chloride values with previously produced data sets. And, of course, the previously produced data sets generally had larger chloride values, and this probably reflects the longer leach time.

Also worth pointing out on this slide is the lack of correlation between chloride concentration and Chlorine-36 ratio, especially for the validation samples with the large range of chloride, but also for the earlier produced Chlorine-36 values where bomb pulse values are noted over a fairly good range of chloride concentrations, and really not a good linea correlation between the two.

I promised that I'd feel obligatory to discuss blank issues at Los Alamos National Laboratory. We've been fairly diligent in trying to deal with this. We take a number of laboratory blanks, especially before I moved into the new lab, the laboratory swipes of the counter tops and the hoods. Processed blanks are included within each group of samples. And, the outcome of that is that the blanks are always small relative to the sample size, and some of the examples are given here for small samples, and we did process a number of small samples for this Chlorine-36 study. The blanks are still typically less than 15 per cent, and much less than that for increasingly larger samples.

For the Niche 1 samples, which I'll talk about here in just a bit, which do show bomb pulse for my analyses, the blanks are 5 per cent or less for all. And, with the four samples that do show bomb pulse, we're down to about .2 per cent.

And, also it's worth pointing out here, it's very important, that earlier Los Alamos Chlorine-36 values typically had much larger chloride concentrations. And, so, you're requiring a much larger blank still than I'm noting here, and even much smaller in comparison to the actual sample size.

We did not evaluate the crushing blank at Los Alamos. However, to point out that all of our crushing and sample processing equipment is thoroughly cleaned, naturally. We follow sound scientific procedures there we believe, and they're rinsed finally with deionized water which should remove any labile chloride.

Also, to point out that when we crush our samples and sieve our samples, this typically will take us sometimes minute, at most a few hours, compared to the several days that we leave these samples exposed to the environment in the laboratory to preconcentrate the chloride solution, and in the same environment.

So, if we don't see it in the processed blanks that are processed right along with these samples for up to over a week, I don't expect that the crushing procedure for a few hours should make a significant difference.

Probably the most compelling argument against blanks, this has been pointed out before, I will re-emphasize it, are the systematic variations that we do see among sample groups. Earlier LANL data for the feature based versus the systematic samples, the lesson there is that if you look for Chlorine-36 and you mine for it in the fractures, focusing on maximizing fracture surface area in your sample, that you'll find it much more commonly than you will if you simply go out there and sample a rock on a systematic basis.

I mentioned that I was in the new laboratory with new processing equipment, new methods, and yet my data are internally consistent, generally in good agreement with previously produced data. I consider it an independent validation of the early data.

The systematic and reproducible differences among different size fractions and leach times for the cross drift samples and the Niche 1 samples. I already discussed the cross drift samples a little bit. This is a plot of the Niche 1 samples. Jim showed a very similar plot, again, Chlorine-36 ratios versus reciprocal chloride. The values produced in this study are in red. Earlier LANL results are shown here in blue in the box.

These are the finest size fractions, and the tie lines connect the coarsest fractions for that same sample. These are intermediate size fractions. So, we see a consistent relationship with the finest fractions, in this case having the highest chloride and the highest Chlorine-36 ratios, compared to the same fraction from the same sample. And, again, the intermediate size fraction plotting as intermediate values for chloride, both chloride and Chlorine-36 ratios.

This size fraction, 2 millimeter to a half inch, is the same size fraction as is produced in the earlier values, and you see a pretty good agreement among these samples with the smaller chloride concentrations in this study, I can readily attribute to shorter leach times.

Also worth pointing out is that these samples with the highest Chlorine-36 ratios also have the highest chloride values, as Jim mentioned, making them much less susceptible to blanks. And, if these are processed side by side, they're sieved, with the smaller concentration here, if the blank were an issue, these should be the samples most affected by the blank.

As part of the validation study, it was recommended that different isotopic systems be used to try to validate the Chlorine-36 results. So, I wanted to talk just a little bit about new Tritium data that has been produced by the USGS. And, I asked the question is this a validation of the bomb pulse signal? These are cross drift samples. Of 22 of the samples, 11 of them yield greater than a Tritium unit, and 8 samples yield greater than two Tritium units, with a maximum of 10.3 Tritium units.

I point out that any, and I quote "valid" here, but let's just move on to any valid analyses greater than .2 Tritium units is indicative of recent infiltration. And, the idea there is that .2 is the theoretical background for in situ production of Tritium for this site.

The analytical facility, Rosenfield, is very proud of their capabilities of measuring at these very low Tritium units.

Now, Jim has made arguments for a statistical cutoff greater than 2 Tritium units, but I think we need to investigate the validity of that, of a cutoff that is roughly an order of magnitude greater than a theoretical value.

Let me point out that most of the Tritium and Chlorine-36 data from samples that are co-located for these two different studies, and bear in mind that unfortunately the samples were not co-analyzed, but only co-located within a few meters typically, but most of the samples that were co-located actually agree. But, it doesn't tell us much because Tritium is below detection and the Chlorine 36 is below bomb pulse, so, they could all be of the same age.

Except to point out that one sample pair, which was collected within 4 meters of one another, that shows the second largest Tritium value of 9.8 Tritium units, and the largest Chlorine-36 value of roughly 5000 that are measured in the cross drift.

Of the other samples that showed either a bomb pulse Tritium or a bomb pulse Chlorine-36, none are co-located to within 12 meters. So, we really didn't sample the same thing.

So, I think, in my view, this is a fairly sound validation of earlier Chlorine-36 studies. I think, however, it does point out that there is a need for a coordinated analysis of the same sample for both Tritium and Chlorine-36. We acknowledge the difficulty of reproducing these values that the GS has had. However, to get 10.3 or 9 Tritium units or several over 6, well, I don't know how you can do it without a fairly significant analytical issue. I just don't have an answer for that.

I think that's the last slide.

CORRADINI: Bill, are you going to come back up? Okay. So, how do you want to handle this? All three of you?

Okay, Norm?

CHRISTENSEN: Christensen, Board.

Bill, this is to you. Clearly, we haven't settled on exactly whether or not in a definitive sense we've got bomb pulse, and you've indicated that not withstanding that situation, that the modeling going forward will assume fast flow was based on basically the Los Alamos data.

BOYLE: That's correct.

CHRISTENSEN: And I guess the question I have is what is the significance of that? If you were to do otherwise, how would that play out in the model? And, to what extent then, given that, is a variety of other decisions being made that are sort of hinged on the fast flow? Is it relatively small? I'm kind of getting at the question of are we in many ways doing a lot of other things in design, and so forth, assuming flow based on the Los Alamos result?

BOYLE: You might make the argument that by assuming or deciding that the Los Alamos fast path data are correct, that it is leading to drip shields or other changes in design.

But, I'll make a number of observations. Bob's last slide, he's making a strong case that let's pretend we never even measured Chlorine-36, the Tritium data alone indicate the presence perhaps of bomb pulse. And I would go a step farther and resort to using analogues, if you will, oil and gas reservoirs is what I'm going to turn to. What they show is side by side, you have rocks that are so impermeable over geologic time, that the oil and gas reservoir is still present. Yet, the reservoir itself is porous and permeable enough, such that we can get the resource out of it.

The question is what's the Paint Brush Tuff non-welded unit at Yucca Mountain? Is it so impermeable that it would not permit the water to go through over long periods of time, or is it fractured enough, such that there are fast paths? I think it would be a very difficult thing to prove that the PTn is essentially impermeable over the time frame of consideration for us, given that there is not only the Chlorine-36 data which indicate the possibility of fast paths, but Bob Roback makes a case that the Tritium do as well.

However any of the studies turn out, I think for the foreseeable future, we will probably have models based upon at least some of the water has the possibility of going through fast paths.

CHRISTENSEN: If I can just follow with one addition point, though? I agree, and I also feel at this stage that the conservatism, that is, the assumption that there's fast paths based on both the Tritium and the Chlorine-36, is something that is inescapable. We haven't resolved that.

One of the issues that's come up repeatedly is sort of the question, well, how important is this anyway? And, given what you just said and given concerns that the Board has regarding other issues that have come up this morning in design, having to do with the drip shields, and much of which are related to issues of percolation, I'd say it's pretty darned important. And that if one would like, and I think DOE might like, to argue that the mountain provides more defense than we currently argue, getting this one resolved would seem to me, particularly given the importance of travel time in the unsaturated zone, would be awfully important. Is that fair?

BOYLE: Well, I think it's a fair observation. If we could ever get to a point, one of the slides I showed was the data match slide, and the ratios were on the range of 200 to 300, and I brought up that, you know, those sorts of data allow one to consider conceptual models where the water is very old. Given a half life of 300,000 years, I'll leave it to people to make whatever guess you want at what the initial ratio must be, but that water would be very old.

If we could prove that, the mountain in and of itself, you know, we could probably just put anything in there with almost any kind of waste package, and performance would be fine. But, the difficulty would be proving that, that it would always be that way.

Now, as to the importance of this data, I think it depends upon one's definition of importance. If one takes the TSPA oriented point of view of looking at importance, the details of, you know, the amount of water coming into the mountain, if you will, particularly through fast paths, the results aren't that sensitive to that, that with the wonderful tool of TSPA, we can put today's rainfall in on the system, and we would still pass.

So, it all depends upon one's definition of importance. If we're judging importance by how well a barrier works in and of itself, then perhaps the data are more important. But, at a system level, perhaps not as important.

CORRADINI: Priscilla, then Thure.

NELSON: Okay, just a quick one. Nelson, Board.

I'm just still transfixed by the Chlorine-36 versus the Tritium measurements in the ESF, and one turns off while the other one turns on along the alignment. The statement here about coordinated analysis for the same sample of Tritium and Chlorine-36 would be interesting. Is that going to be done in this study that is just starting up with UNLV and New Mexico Tech., or is that just focused on Chlorine-36? Does the project have any aim to do this coordinated analysis question between the Tritium and the Chlorine?

BOYLE: I forget how many pages their proposal is. But, the investigators did bring up the possibility of looking at other isotopes, including not only Tritium, but other bomb pulse isotopes of iodine and technetium. And, so, I don't know that the proposal ever got at the issue of doing the measurements on the same, you know, specimens, if you will, you know, leaching them differently, or processing them differently for the different isotopes, but Professor Cline is in the room, and Professor Stetzenbach, so they have heard your question.

PACES: Could I address that just briefly?

In my supplemental slide, Slide 23, we have attempted to try and duplicate these analyses. And, they're not true duplicates in the sense that we had exactly the same material. But, that slide shows that there are great difficulties, and I just want to stress the importance that these analyses--this is very analytically challenging, both the Chlorine-36 measurements, as well as the Tritium measurements, and we shouldn't be too soon to jump to conclusions, we think.

Also, we have samples preserved that we extracted the pore water with elevated Tritium from. In theory, the Chlorine hasn't gone anywhere. So, those are the best candidates to try to verify--this is one of the recommendations--take those samples, crush them, leach them, see if we don't get elevated Chlorine-36, which we should see.

CERLING: Cerling, Board.

Sort of two, well, one technical question to both of you, which is about Tritium measurements. I was just wondering how were those extracted? And, then, were they analyzed by counting or by Helium 3 ingrowth?

PACES: The Tritium analyses were--the pore waters were extracted by vacuum distillation. I certainly did not do that work. I think that that typically extracts 95, 98, 99 per cent of the water by weight out of the rock.

Obviously, there is some handling that goes on from the time that they existed in the mountain until the time that they were condensed and sent off to Rosensteil. Rosensteil at University of Miami does enrichment and counting, and a fairly elaborate setup.

And, like Bob said, they report very low values for their blank. But, again, we feel that these aren't your standard 200 gallon per minute groundwater samples that we've quickly capped and sent off to the laboratory. There's a lot of steps in between from the rock in the unsaturated zone wall, drilling it out, handling it, you know, capturing it, distilling it, et cetera.

We don't really have a good test of what the true blank level is for rock that's been processed like that.

CERLING: And then just a question to Bob. On Figures 12 and 9, which shows the LANL validation results compared to the pre-validation result, in Figure 12, you've also got some data that doesn't appear on that slide. So, I was just wondering if those real high values that you show here, which are validation samples, you also don't have on Figure 9.

ROBACK: That's right. Figure 9 is just cross drift samples, and Figure 12 are Niche 1 samples, which is just off of the ESF. They are not shown together. You're correct. They are compared to earlier LANL data for both locations.

CORRADINI: Corradini. I have a question.

Since I'm not a chemist, and I'm not a geologist, I think in simple terms of all this, I thought I was going to get something to explain this, but now I'm more confused than ever. And, maybe everybody else in the audience isn't, but I am, so I get a chance.

I don't see, and it seems to me if there's a disagreement between two investigators in data, I first would want to see an experimental blocked diagram of every step you did to make a measurement, and everything you did to eliminate and exclude every measurement piece that showed consistency or inconsistency. And, the closest that came to that was Bill's original point of his data that you both did under the assumption we want to figure out where it's coming from. Then, you got the same results.

And, then in the second talk, there was a discussion of what now can be excluded in terms of the experimental protocol because you did get agreement. So, I'm curious from all three people, so I'm looking for an opinion, what things were excluded from the experimental protocol because you actually did get a match in eleven samples, and what things aren't excluded? And, for the things that aren't excluded, what is the path forward to start excluding them? That's my question.

BOYLE: I'll go first. But, I'll certainly defer to selection of some of the, you know, details of what's excluded and what's not excluded to Bob and Jim.

But, with respect to this block diagram that--

CORRADINI: It's just a conceptual thought in my mind to figure out how you do it.

BOYLE: Right. And, that was the path that was being followed in practice, if not actually having it diagramed on a piece of paper that was actually at the meeting in Pahrump in May of 2000. I think before even the presentations were made, the investigators had shown each other the results, and they were already trying to figure out what are the possible differences, and they had seized on the sampling as one major area. And, as the years have gone by and the various results have come in, things started to get knocked off one at a time, although some still remain.

Our best hope at this point with whatever remains as the possibilities is the validation study of the validation study by the UNLV and New Mexico Tech professors.

Myself personally, and Jim said it as well, I think I tend to agree with the points that Jim brought up, that coring versus hand sampling, that doesn't appear to be the issue. We can let all the physicists in the audience rest assured we don't think it's accelerator or mass spectrometry. Two different facilities were used. To put the chemists in the audience at ease, we don't think it's the target preparation, as Jim called it, you know, the wet chemistry precipitation and silver chloride. And, once we settle upon a leach time, a leach method, that doesn't appear to be the issue.

Whatever it is, it's something that's not immediately obvious. I'll say that. And, it does raise the possibility of it's not chemical contamination, if you will, that both groups, plus or minus, have similar chloride concentrations, but they differ in Chlorine-36, and it raises the possibility is there some sort of contamination by physics. You know, Chlorine-35 can go to Chlorine-36. Chlorine-36 can go to Argon-36. Is something at work such that something is changing the ratio in one or both of the locations, or even out at the Nevada Test Site? Whatever it is, it's something that's difficult.

And, I would like to bring it to people's attention, I think Jim mentioned it in passing, this really is a difficult measurement. I wish Greg Nimz of Lawrence Livermore National Lab were here. He's the one who runs the accelerator at Livermore. They are counting atoms, hundreds, and that's it in some of these targets, and as a non-physicist, it's astonishing that they can even do that.

So, it's a very tough measurement, and it doesn't leave much room for error. And, again, whatever the difference is, it's not something that's obvious, and we still have hope that the next study will find it. And, Professor Cline was involved in an earlier study for the project that involved a scientifically challenging and controversial topic, and she handled it very well with a lot of interaction with all the groups involved. And, in their proposal, they have suggested that that route will be followed again. So, we will be able to, the group from UNLV and New Mexico Tech will be able to benefit from discussions with Los Alamos and the USGS.

CORRADINI: So, I'm going to let the other two talk, but I'm going to pick on you a bit. I apologize.

So, is this a voting thing? The next one that comes up, the majority rules?

BOYLE: No.

CORRADINI: I want to know a closure mechanism by which--

BOYLE: No.

CORRADINI: Okay. So, you see where I'm going with this.

BOYLE: Right.

CORRADINI: Because what I guess I'm kind of hinting at from an engineering standpoint, not a scientific analysis, is I see no root cause analysis. There's a problem here, and I see no root cause analysis to determine what's excluded and what's still included in the root cause of the discrepancy.

BOYLE: Right.

CORRADINI: That's my underlying worry.

BOYLE: Yeah, we don't have definitive lists, although people have some good ideas. But, back to your question, is this going to be a vote? In discussions within the project, before going ahead with this yet third party measurement, some people advocated not having a third party, in part because there's a high likelihood it won't be dispositive, as the attorneys would say.

I can speak for these two groups involved. If the third party group has high measurements corresponding to Los Alamos's, but can't explain the lower measurements by USGS, they're not backing off, the USGS, and I wouldn't want them to.

Conversely, if the measurements all come in low, but there's no explanation as to why Los Alamos's were high, Los Alamos isn't going to back away from their measurements, and I wouldn't want them to. So, it's not going to be a vote.

If the third party group can find the smoking gun, fine. We're done. If they can't, then we'll probably just continue as we have, which is to take the most conservative data set available out of all those that are present, and implement it.

ROBACK: Just to speak briefly, you know, we have gone through this step by step to try to evaluate what potential cause of the differences might be, and at each of our meetings and in each of our conversations, that's been continually the theme.

But, I guess in our defense, these measurements do take a while, and each time we come up with a difference, we propose a test to evaluate these. And, then, you know, a few months later, perhaps several months later, we get some new data. And, lo and behold, well, they still don't agree. We eliminated that possibility, now we have to move on to the next one. We did do that in a well thought out fashion, I believe. We're not there yet. We have more tests in mind that could be performed, but perhaps the third party is the best thing yet to do.

PACES: I think that I'd agree with both Bill and Bob. In the report, we have a path forward, talks about some of the continued concerns. I tried to capture those in the last few slides, in particular the recommendations, and I think that we could probably get to the root cause of some of these differences by looking at the samples that already exist.

I don't think that we need to go out and have DOE drill a whole bunch of new holes, or a new sampling program, and that kind of stuff. And I think that those are outlined in the report. I think it's still a controversial issue, if you take a rock and you crush it at Los Alamos, whether you get a high value, or if you take a rock and you crush it and process the whole thing someplace else, there's the other contamination. I think it is an analytical issue.

Again, they are tough measurements, and I think with a modicum--you see, we stopped and we've sort of thought about the whole thing. I think we have a path forward now. Whether we take it forth from here, I think that the problem will be resolved, and I think there is an answer, and I think we will find it, maybe not us, but the project I think will.

CORRADINI: Dick?

PARIZEK: Parizek, Board.

I'm looking at sort of the conflict between the Tritium occurrences versus the Chlorine-36. I mean, you're down to a few atoms in 36 and have a bad day, you find some that may not exist, but the other values I guess are a little easier to measure. When you start talking about above two and up to eight, maybe nine Tritium units, is that in the same ballpark as difficult to measure, or is there something about the conceptual model of where you're finding the Tritium that needs to be understood? Is it consistent with a conceptual model where you found your Tritium, or didn't find your Chlorine-36? It seems to me there's this inverse relationship between it, too, that's a little bit hard to get onto here.

PACES: If your question is does the Tritium data where the post-bomb Tritium values are found, whether that's consistent with the conceptual model, I guess at this point, Gary Patterson of USGS has been the one who's been thinking most about the Tritium data. I don't think we have a conceptual model to explain the distribution of Tritium. It doesn't necessarily fit the conceptual model based on Chlorine-36.

I also believe, and I may be wrong on this, but I think I read not too long ago that one atom of Tritium out of one times ten to the 18 atoms of regular hydrogen causes one Tritium unit. So, again, it's, you know, there are certainly ways to make that measurement very difficult as well. And, we do process these samples a lot, and you can see the difficulty in trying to reproduce those values.

PARIZEK: As far as a model validation or calibration is concerned, again, if you have Tritium at those depths, that's I guess, and Bo may tell us about this this afternoon as to whether it's consistent with model calibration, whether it's the Chlorine-36 or the Tritium. Either way, if we're going to use it, we've got to know which one to rely on.

PACES: I'd like to make sure we're all very comfortable with the analytical issues before we go to a modeling issue.

CORRADINI: One last one. Mark?

ABKOWITZ: Abkowitz, Board.

This is not my area of expertise, and I'm beginning to feel pretty good about saying that. And I'm not going to get into--let me tell you where my perspective is. I'm almost looking at this as if I'm a member of the public, or from the lay person, and I sit here and I think we've got these highly respected scientists, and they're sorted out into two groups, and they can't agree with each other. So, they say, okay, we'll go one step further and try to do some more, and they still can't agree with each other. And, now they're talking about going some further steps to hope we agree with one another.

And, I harken back to the discussion that I had with John Arthur earlier today about quality, and the implications of different models on the TSPA, and I just can't help but think how many other, you know, hidden problems of this kind are embedded in the various sub-components of the TSPA, and are we even looking for them.

And, so, I guess, and I don't know whether this question is for one of the speakers here or for John, but is there a take-away from the chronology of what we're seeing here that needs to be applied to other aspects of the modeling processes that are the foundation for the TSPA?

BOYLE: I'll give it a try in part.

I think there's credit due to the project, because it was through the processes of the project, the people involved and their willingness to challenge results, that this ever came up as an issue at all. There were the existing Los Alamos data, and some people looked at them and said I'm not entirely sure about that. Are you sure your answer is right? We did a peer review. We did a validation study. I would say those same processes, the same individuals, the same culture exists in many areas of the project, if not throughout the project.

So, take, for example, might there be some hidden issue along these lines in the corrosion rate of one of the metals, I would hope if there were that it would have surfaced in exactly the same way that this one did.

The fact that this hasn't been resolved yet, as I said in my presentation earlier, may just be a reflection on the science isn't there yet, which isn't a bad thing. You know, we've had as reputable groups as we can find investigate it, and the fact that they haven't come to a resolution doesn't reflect badly on anybody. And, I think the fact that we've pursued this, you know, that the item surfaced just through the course of regular project business is a good thing.

ARTHUR: Just a follow-on from this morning, I mean, I truly believe it's good, and within the project these kind of discussions are underway, and we're trying to look across all the AMRs and other areas to identify that. It's almost like in the NRC space, the term of differing professional opinions, we have different scientific results, and I think the basis of our license and all the documentation needs to show where this exists, and the basis for us to make a decision, and to document it.

Everything is not going to even be solved by science in every case. This is truly one that's baffling us, and it isn't a vote. I wish I had the answer. We're going to move through, commit to this third study. We had a lot of internal debate whether we wanted to do that or not, because you could get a third set of results, and off we go again.

CORRADINI: Thank you, Gentlemen. That concludes our technical presentations for the morning.

We're running late, but we have five people registered for public comment, and I wanted to give them an opportunity to speak. The first one registered is Sally Devlin, a member of the public. Sally?

DEVLIN: Thank you very much for coming. And, I have permission from Henry Neff, our county commissioner chairman, to welcome you formally. So, now you are formally welcomed, and he apologizes for not being here because of his county commission day in Pahrump. So, now you know why he's not here and I am.

And, it's an anniversary for me. On August 13, 1993, I attended my first NWTRB meeting, and John Cantlon was the head of it, and he announced that the railroad would be going through Pahrump. That was the only plan, and it would cost $1.8 million, and I said, "Over my dead body."

And, then we broke up into rooms, and this doesn't count as my time, this is in your welcome, and then we broke down into rooms with the facilitator and the usual questioning, and I met my first nuclear reactor people who actually had nuclear power from Minnesota, North Carolina, South Carolina. And I was sitting next to Russ Dyer, who was on my right, and Bob Liu was on my left, and to the left of us were seven Indian tribes.

And my first opening remark at the meeting was one of these young gentleman got up and he says, "Don't bother with Yucca Mountain because we're going to kill all you white eyes." And, we grew from there. So, I don't know if any of my Indian friends, but that was my introduction to Yucca Mountain.

So, it's been a long time. I've been through many marriages, divorces, baldness, children, grandchildren and great grandchildren, and it's been an experience.

But, today, as you see me with my two bags, that's not my lunch. What it is is report, and now I start my little report, and that is I want to always thank everybody for sending me everything, especially the NRC. And, my friends at NRC sent me the 194 projects out of the 293 that they're attempting to do for the licensing, and in that were eight pages that pertain to the projects in Nye County, and what Nye County had not done. And, this, in turn, turned me on to getting Allan Benson to get me the Inspector General's report on the finances of Nye County. And, there were a lot of discrepancies there, 37 pages of that.

And, then that caused me to go into the pet money from Nye County, and so I got the DOE report which said $65,900,000, and then I got the county report which said $80 million something. And, then I got other reports, and the reason I'm saying this is for Margaret Chu. Margaret, as the Director, you are responsible for this, and I feel that Bush hates women and doesn't value our individual rights. So, therefore, I don't want, if anything fails, the burden to be on you.

So, what I have done, and I'm looking at the audience, it's about 30 to 1, men to women, and this is every meeting is the same thing. So, what I am saying is I always cover my butt and all of my reports have gone to many people that you know I know.

But, I have brought with me the documentation on all of this, and what the Nuclear Office in Pahrump and in Tonopah has been doing over the last ten years, and the discrepancy in the numbers.

I also brought with me three of the project contracts, and I have never seen this before. We now have two commissioners who are having open meetings before the commissioner meeting, with all the documentation. And, on these contracts, one of which is Tom Buco, and they gave $10,000 to some company to investigate him. And, Tom can tell you about that, and I always say go to the source, he's here and he can explain the $10,000.

The other contracts that I have are for $100,000 to investigate Forth Mile Wash. Well, you know I am now I am now computer literate, sort of, with the help of my tutors, and I have available to me all the reports on Forty Mile Wash since 1960 to 2002. And, I'm sure that just like this last presentation, we're not going to all disagree, but it is going to come out in the wash. Excuse me, I have to do that.

So, anyway, I have brought all this documentation, and I'd like to meet with Margaret, or whoever is in charge, because there is a discrepancy of $20 to $30 million. And, when I see contracts being award, and another one with the potential, without bidding--you're hearing this, they just pick it, out of towners, out of 100,000, 75,000 goes out of town. And, I'm talking to Don Watson, I'm talking to Bill Williams. They're involved in it, and they all get handsome salaries, and they are not doing any good for Nye County's economic development.

The other thing is, you know, I got into this on the railroad, over my dead body will you bring in the $1.8 billion railroad through Pahrump, and of course all the signs have changed, and so on. And, I've asked many questions about this.

Now, I've also brought in two of the reports, one on the protection of Nye County, which is a revised edition from 2001, another one 2002, there's not a word change.

The other thing on the transportation report, and I am blowing the whistle, is that there isn't a number in the transportation report. As you know, as of our last meeting, I learned to build railroads, I learned to build roads, both concrete and asphalt, and now barges. I don't have the price of a barge from those 381 pages that were sent me. But, I certainly know how much a railroad would cost, and I certainly know how much the roads would cost. And, any truck put upon our roads would sink.

And, as you know, last week, Tuesday, we were going up to county commission meeting, and we had a terrible flood, and this is another thing, the weather is hardly considered. And, Nye County can have any weather any hour of the day or night. So, Margaret, I invite you to invite me to see all this documentation, because it is for you. And, I want this as the public to feel confident in all the things that are going on, and I certainly do not with these financial discrepancies, with these terrible, terrible contracts.

And, the word is performance criteria. There is no performance criteria mentioned in anything, and of course the Congress, and you know I've given you all the GAO reports on results management, and that's what we're here for, and that's what all these hundreds of billions of dollars have gone to.

So, I will share with you, because I brought, and that's why it's so heavy, all of the original documentation, and you can see what's going on, and I don't like to use any derogatory terms, except that I had an altercation with Les Bradshaw, our head, as you know, and he didn't even know where Forty Mile Wash was. I can go out there, and I'm going out on the 27th on another Yucca Mountain Tour, and I'll show him. And Bob Milken who is here from Washington, who represents Nye County, all of these people are from out of town, they do us no good economically, and we never get a report from them. Why pay people if you're not going to have parameters. And that's not the end of it.

So, here's my lunch. I'll share with you. Not now. You bet, I brought it all. So, again, welcome everybody. I'm sorry to drop this bomb, but I think it is time that these things were looked into. And, if the IG's office has already looked into this, and they're not going to do anything about it, then they're remiss.

So, thank you for coming, and I know you're starving. I am, too.

CORRADINI: Our next speaker is Mr. Grant Hudlow, a member of the public. Mr. Hudlow?

HUDLOW: I'd like to also welcome you, and thank you for coming.

I'm happy to hear John Arthur talk about the change from management to leadership. This is the first time I've heard this normal leadership addressed in this project. He's showing us how easy it is to turn a project around when you know what you're doing.

So, those of you that are technically oriented, maybe you're not too interested in that. But, those of you in a management position need to watch what he does and see what he does. It's very simple, and it's very effective. He's already showing how to increase the productivity of the people on this project by 400 per cent.

One of the things I keep hearing people talk about, well, in the past, we had all these problems and, of course, Congress cut the money off for science back in the Nineties sometime. And, one of the things that happened at Topopah, the NRC came over there to try to con us into thinking that radiation is good for us, and I was appalled at the mess they made. But, out of that came that the information that the investigation into the murder of Paul Brown shows that we have gangsters, mobsters, Mafia, whatever you want to call them, involved in the Yucca Mountain project, and I missed that pattern. I should have seen it years ago.

The pattern is that they do the work, and it's not done at all. They get a contract, they do the work, they write a report, and it has no relevance to anything. So, that's thrown a monkey wrench into the works for the last 20 years, or so, and I'm sure that John Arthur, in his processes, will turn that around rather quickly.

We see that showing up in the KTIs. When I looked at the list that was sent to Sally Devlin, I was appalled. And, I realized that even with this mobster background in there, that the NRC should have picked that up, too. And part of the reason for these KTIs being--most of them are pretty irrelevant to the project--is that the NRC doesn't understand how this project needs to work. So, you're going to have to educate them. And in the licensing process is probably not the time to do that. You have to tell them what they need to hear in order to get the license. But, then, that makes you responsible for making sure the project works when you get all through.

And, I want to thank you again for coming, and I want to re-emphasize that Margaret Chu made a brilliant choice when she brought John Arthur on board.

CORRADINI: Thank you. Our next speaker is Ms. Judy Treichel, the Nevada Nuclear Waste Task Force.

TREICHEL: I have a question for Margaret that shouldn't be very difficult at this stage of the game. But, it is--I would like to know what the Department believes the disposal capability of Yucca Mountain is. Because, very recently, there was a court case where DOE tried to be able to reclassify some of its defense liquid waste, and it was turned down, and DOE has said that it's talked to Congress and other places, that it has to reclassify some of that waste in order to be able to say that it has lesser danger and does not have to go into a repository. Because, if it does have to go into Yucca Mountain, it would exceed the 70,000 metric ton limit, and we already know that the expected commercial nuclear fuel will exceed that limit.

So, since DOE has been turned down in reclassification, we already know that commercial is going over, I want to know what you believe the disposal capability of the mountain to be.

CHU: Judy, you know that the Congress decided that 70,000 metric tons is really a statutory limit that was given to DOE, and it's really not a technical limitation, as we can tell. If you ask me what is the technical limitation, to tell you the truth, we have not done a detailed analysis on that, because we are given that 70,000 right now, so, the design and license application, everything is based on that assumption.

So, until the Congress asks us to do a technical analysis, I don't have the answer for you right now.

TREICHEL: Okay. I just wondered with all of this going on, if you did have it.

Then, the only other point I wanted to make was to John Arthur with the presentation that he had done. You talked about 200 or more KTIs being in the works now and being expected within this calendar year, and if you add up on the graph that you showed, it doesn't come to that. So, I'm assuming that you're using a bundling technique already, and I had written to NRC regarding that, because they asked for comments, and I had some questions and concerns about it. And, then I was contacted and I told them to hold off on answering my questions and concerns because there apparently isn't any agreement about what the bundling process is between NRC and the DOE, and I know that you're meeting probably next week about that. And, if you haven't even defined what it is, if you have no agreement on what it is, it seems very strange that you're already putting it into practice.

ARTHUR: I'm glad to talk to talk to Joe and I at the break. Also, the chart that I showed this morning doesn't show some of the earlier KTIs. We say we'll have 200 addressed by the calendar year. That's total to date, including some that weren't on there.

As far as the agreements with NRC, I think I did say this morning that we're still working with them and their staff, and will probably have a technical change at some time to make sure that the rebundling approach is acceptable to them. But, no matter what, internal to our process as we see it, it's best to satisfy the original intent of the KTI agreements.

TREICHEL: Okay, thank you.

ARTHUR: Thank you.

CORRADINI: Thank you. Our fourth speaker is Don Shettle from the State of Nevada.

SHETTLE: I want to start by addressing a comment I think on the last talk by Dr. Abkowitz, who was questioning are there any alternative viewpoints of issues. And, I think the State of Nevada offers some alternative viewpoints of things, especially where we have done some experimental work, or otherwise, and especially on corrosion. And, I think this has had some influence on DOE, whether or not they admit it.

My main comments are on Robert MacKinnon's talk on EBS performance. And, I think we can agree on one point, maybe only one at this stage, and that is at least that the thermal pulse, it is unlikely that any pure water, dilute solutions, will penetrate the vapor barrier, in other words, the heated rock above the repository drifts under the high temperature operating mode.

However, under this mode, there is a refluxing zone above the drift. This consists of a boiling zone near the drift, the vapor steam rises, condenses in cooler rock above that, and then drips back down and may dissolve some rock in the condensation zone, and drips back to the boiling zone by gravity. This is a cyclical process that can result in the boiling solutions becoming concentrated. And, when you concentrate the solutions, you get what is known as boiling point elevation. And, as an example of this, the UZ pore water that is used in many DOE experiments and representations, I think the one that's in the paper by Rosenberg, that can boil up to at least 144 degrees Centigrade.

The boiling points referred to by DOE are ambiguous, because the boiling point again is a function of solution chemistry, although I believe that when DOE refers to the boiling point, they refer to the boiling point of pure water, which is 95 degrees Centigrade at the altitude of Yucca Mountain.

The point is brines or more concentrated solutions can be generated in the refluxing zone while the repository rocks are above what DOE calls the boiling point of pure water. And, the question is how much of these brines can be produced in this refluxing zone, and if they can be produced, they could penetrate through and seep on to the EBS. And, there are also experiments and modeling calculations of fingering of solutions in fractures. But, if you look at these reports, you will see that these all refer to essentially pure water solutions. There is no accounting for the change in chemistry of solutions that may result at Yucca Mountain.

So, as I said before, the question is how much of these concentrated solutions can be produced, and, therefore, penetrate to the EBS. And, I don't think that this has been addressed thus far in TSPA. And, for results that we have presented previously at this meeting, and even more interesting results that we will be presenting at future meetings, if given a chance, we believe that the EBS models are not conservative, as presented by DOE.

Thank you.

CORRADINI: Thank you, Mr. Shettle. Our final commenter is Mr. Mel Gascoyne from AECL Canada.

GASCOYNE: I have a couple of comments which might help people to understand a little bit more about the Chlorine-36 Tritium problem. Ten Tritium units, which is the highest concentration that was observed, does not a bomb pulse make. Ambient rainfall in that region is around the 6 TU limit, whereas, the Chlorine-36 data we're talking about are factors of 10 to 20 times higher, and clearly are bomb pulse type of values.

The second point is that Canada also had an accelerator mass spectrometer facility at Short River up until reasonably recently, and I understand from there they had great problems sometimes in dealing with spikes of Chlorine-36 that would be measured when they're doing these ratio measures. It was very uncertain where these spikes came from. It could have been the dust particles in the air. But, the Short River facility had two operating nuclear reactors. Chlorine-36 was ambient in the environment there.

So, the possibility of the same appearing at Los Alamos could perhaps be looked at, and would be very difficult to trace.

The other aspect of Chlorine-36 is that I understand that some early equipment that was used in excavation of the ESF came from the Nevada Test Site. And, as far as I'm aware, there's no measures taken to ensure there were no bomb related Chlorine-36 attached to this equipment. So, that might account for some of the earlier Los Alamos data which saw these high levels in the tunnel, and then as you go further down the tunnel, you don't see them anymore because they're all washed away.

But, I actually did submit a written comment based earlier on Dr. MacKinnon's presentation about the question of corrosion. Why the effect of microbiological activity not being recognized or included in the corrosion model? It's already known that microbes and spores are abundant in dust in the ESF, and these will become active when deliquescence or seepage points are established.

In addition, the presence of nitrate provides a nutrient and microbial growths would probably develop and flourish.

I don't know whether any other presenters are here, or whether they're all at lunch now, but those are my comments.

CORRADINI: Thank you very much. Submit it to Linda in the back. Good.

Okay, I think we're at lunch, or maybe even a little bit after. We're going to take an hour break, and be back here at 2 o'clock.

(Whereupon, the lunch break was taken.)

AFTERNOON SESSION

CERLING: Welcome back to the meeting. I'm Dr. Cerling taking a handoff from Dr. Corradini.

As our Chairman said in his opening remarks this morning, one of the main purposes of this meeting is to hear this afternoon's presentations on the performance of the natural barriers of the proposed Yucca Mountain Repository System.

The concept of a multiple barrier repository design with both engineered and natural barriers has long been a key concept in the radioactive waste disposal program of a deep geological repository.

As most of you know, the licensing regulations that the U.S. Nuclear Regulatory Commission will use to evaluate the safety of Yucca Mountain repository require that the proposed repository consist of both engineered and natural barriers.

At our last meeting, we heard about the expected performance of one of the engineered barriers, the waste package. This afternoon, we will hear about the performance of the natural barrier, specifically flow and transport through the unsaturated and saturated zones.

Although there's only two speakers on the agenda this afternoon, I'm told we should expect a large amount of information to be presented.

So, the first speaker will be Robert Andrews, and so this afternoon's session begins with an overview presentation by him. Dr. Andrews is the Performance Assessment Confirmation Manager for Bechtel SAIC Company. In his prior position, as the Performance Assessment Manager of OCRWM program for the last several years, he's directed all activities related to ongoing site and design evaluation. He manages and coordinates the technical investigations of the BSC team, including the national laboratories, in support of the science and performance assessment products of the license application.

ANDREWS: Thank you. I want to take this opportunity to thank the Board for organizing this afternoon's session around the natural barriers.

But, before we go into the details associated with unsaturated zone flow and transport, and saturated zone flow and transport, I wanted to put these into the context of the other components of the total system for post-closure repository safety, and what we're doing with respect to these things that we've called Technical Basis Documents, their birth, and what's going on with respect to those as they describe elements of the components of post-closure safety, and the technical bases behind those elements of post-closure safety.

So, if I can go onto the next slide, we're going to have just a little background, and then go onto what these things are, these Technical Basis Documents, their goals, their objectives, their content in a very general sort of sense, and why we're doing them, and then some concluding remarks. But, mostly this is an introduction to Bo, who follows me, and to myself, who follows Bo.

Okay, in May, we presented three integrated presentations, one that related to chemical evolution in the rock, and the thermal hydrologic evolution that's tied to that chemical evolution in the rock that Bo presented, one that related to taking that information and evolving that chemical evolution with the thermal hydrologic conditions inside the drift that Mark Peters presented, and continuing on with a presentation that Joe Farmer gave with respect to what does this chemical evolution in the rock and in the drift, and the hydrology and thermal conditions related to that, mean with respect to waste package corrosion, and the degradation processes of the waste package.

There was a series of posters, or actually it was all one poster at that time, where all three of those integrated processes were presented in one massive poster, and then Bo and Mark and Joe took an afternoon to walk you through the elements of that, and give you an opportunity to question the technical bases behind the elements of that.

The Board, in their letter back to DOE said they found that a very useful way of presenting what can be a lot of technical information, and presenting it in a more integrated fashion.

The Department agrees with that Board conclusion as documented in the Board's letter, and have embarked on producing what we've called Technical Basis Documents, and tying these Technical Basis Documents to the key technical issue agreement responses that relate to each of the technical areas for which we have Technical Basis Documents.

The Department presented that approach in a letter to NRC on June 20th. We presented that to the Advisory Committee on Nuclear Waste shortly thereafter, I think June 24th or 24th, or something like that, and we now have an opportunity to tell this Board that same approach and methodology.

I think somebody alluded to, Judy or somebody alluded to there is an NRC technical exchange on this subject next Tuesday, assuming it stays dry enough in the Washington, D.C. area and all the federal employees can weather Isabel.

We've called these integrated technical documents Technical Basis Documents. You sometimes see a shorthand

TBD. It does not mean to be determined. It means Technical Basis Documents.

When we gathered together, and I don't mean that in a religious sort of way, but we got together after the last Board meeting and say, okay, this was a good approach, developing it from beginning to end from data, through models, to results of models and analyses, finally into an input or relevant input into a post-closure compliance case, or safety case input.

We realize there's a lot of technical information that can be presented. What are these technical bases? The technical bases were developed as a way of integrating essentially KTI responses. The KTI responses that were talked about by Margaret and John this morning cover a wide variety of KTI areas, and develop a wide variety of technical and scientific disciplines.

Placing the KTI responses into the context with which they are relevant to the post-closure safety analysis we felt, and I think NRC believed, was a useful way of integrating and presenting technical information.

It's important to point out that the technical information that is developed in response to the KTI agreements, the real technical information that's in direct response to the KTI agreements, finds its home in quality products, quality assurance products. Those are data sets and scientific notebooks of your data. Those are analyses and model reports, euphemistically called AMRs, and those are within the engineering design, drawings and calcs and specs and et cetera. That's where the ultimate controlled information resides, and now you try to, if you will, summarize that and directly respond to the question of the KTI agreement, and present the technical information in a way that a broad variety of interested parties can (a) understand it, and (b) interpret the uncertainty and how that uncertainty affects the output of that particular component of the system.

So, if I can go on to the next slide, when we gathered together to look at how to best document this and produce this, we, about seven or eight of us sitting in a room, looked at a lot of different ways to, if you will, thread the needle through the system.

Some of the ways were related to scale, some of them related to the processes that are active at different scales, some of them related to time, some of them were related to state variables, temperature, flux, concentration, pressure, et cetera. And, some of them were related to sequential aspects of the system, which I think we've presented to this Board and we presented in the site recommendation documents, more or less following the water drop through the system.

We looked at these cross-cutting ways of documenting things by scale, by the key processes acting at different scales, by the key state variables within those processes that are kind of the boundary conditions, if you will, that act across different scales of the system, and came up with this kind of mapping.  But, we realized even with this mapping, that was not probably a way to document the work, but it was a way to organize, you know, the thought processes and the relevant aspects that needed to be considered when you documented that work.

So, what we ended up with is on the next slide, and I'll have to walk through the next slide.  The next slide, in hard copy, I think has all 14, if you will, components of the post-closure performance assessment documented.  What I'm going to walk through now is just those one at a time so you don't have to search and find out where Number 3 is on the list.

So, John, let's go ahead and walk through this, starting with the first component, and now I am going to start the old traditional way of water moving through the mountain, and water starts with climate and infiltration, and the processes that affect net infiltration across the mountain and, therefore, the net percolation flux at the repository horizon.

Going on to the unsaturated zone flow, and the processes, the effect, the distribution of percolation flux at the repository horizon. Bo is going to talk about this at some length. His upper poster here that you see provides, in a single poster, the summary technical basis for our understanding of percolation and unsaturated zone flow, and the distribution of that unsaturated zone flow between fractures and matrix.

The next one, Number III is the water seeping into the drifts. This Bo talked about last time, you know, how much water can seep into the drifts. We're not going to spend a lot of time on it in this particular meeting.

Each one of these, I should say, has a document that's being produced with the associated KTI agreements that are tied to each component as we speak. Three are in review within the Department of Energy right now, or I should say four, and one is scheduled to be delivered to NRC's biosphere either at the end of this week, or the beginning of next week.

Number IV is the mechanical degradation, so it's the drift degradation processes, and that includes the higher probability, the $10^{-2}$, $10^{-3}$, $10^{-4}$, annual probability of seismic events that are included and do affect the degradation of the drift and processes there.

Number V is the in-drift chemical environment. That you heard a little bit about from Mark Peters. We recently submitted the summary interpretation of that in-drift chemical environment to the Department last Friday.

And going to one of Ron's question I think earlier this morning, there are 14 KTI agreement items specifically tied to the in-drift chemical environment. About two or three of them relate to chemical evolution in the rock and the models and uncertainty associated with the chemical evolution in the rock. A few of them deal with chemical evolution of seepage water. A few of them deal with chemical evolution of dust on the drip shield and on the package, and a few of them relate to the relevance of the testing environment, the waste package testing environment, and the chemical environment used for that waste package testing environment. So, those KTI responses are included as appendices to the summary Technical Basis Document itself.

Number VI is the waste package and drip shield degradation, the corrosion processes, localized corrosion processes, and other degradation modes, including the effects of microbial induced corrosion, and other processes.

Going on to VII, we're now inside the package, with the waste form, the cladding, the solubility, and the chemistry, and thermal and hydrologic conditions inside the package after the package has degraded. Again, I'm more or less following the degradation processes and following the path of water and its affect on the system.

Going to VIII, colloids, after a lot of discussion, we treated colloids, even though their source is at the source term and the degradation processes at the source term, and the information we have at the source term, the KTI agreements on colloids cut across all systems. There's about eight KTI agreements directly related to colloids, and those KTI agreements mostly relate to how colloids are consistently treated in the waste form and the engineered barrier system in the unsaturated zone and in the saturated zone.

So, because of the nature of those KTI agreements, we put all the colloid information into its own stand alone Technical Basis Document.

On to IX is engineered barrier system transport. So, this is transport from the waste form, through the waste package, through the invert, and back into the rock and the processes and the thermal effects and chemical effects of those processes in the drift and in the package, and its affect on transport back out.

Going to X is unsaturated zone transport, and Bo is going to talk about that one after I get done here. There's a poster for that.

XI is saturated zone flow and transport. I'm going to talk about that at some length later on this afternoon.

XII is the biosphere. This deals with nuclides and their uptake, both in the igneous activity scenario, i.e. the possible presence of an ash deposit, and the natural occurrence that may occur from a low likelihood waste package failure and release through the natural system.

On to XIII is the volcanic events themselves and igneous activity. There are six KTI agreements tied to igneous activity and the consequence of igneous activity. So, there's a Technical Basis Document and appendices associated with that that's in the process of being produced as we speak.

Going on to XIV is the possible effects, both the initiation effects and consequences, of low probability, i.e. $10^{-6}$ per year, $10^{-7}$ per year, $10^{-8}$ per year seismic events.

So, this pretty much encompasses the total system, and addresses the KTI agreements that relate to those components. It does not address all the KTI agreements, however. There are KTI agreements specifically related to pre-closure design and pre-closure safety. They are not addressed in this suite of fourteen. Seismic events are addressed in these fourteen, but not the pre-closure design aspects and the KTI agreements related to those pre-closure design aspects, like the drift stability issues during operations.

Criticality, there's about six KTI agreements associated with criticality. They are being addressed in a separate one by one addressal fashion. There's KTI agreements on TSPA itself. This is the individual component parts of TSPA, but there's about seven or eight KTI agreements on the TSPA itself, the development of the TSPA, the controls on the TSPA, the validation of the TSPA, et cetera. Those KTI agreements will be addressed, as I said earlier, in the May time frame of next year when the TSPA is done, or scheduled to be done.

And, there's other KTI agreements on features, events and processes that are being treated in a separate fashion.

So, these fourteen are hitting the principal inputs into the post-closure safety analysis represented by TSPA, but there are some other sideline ones.

The next slide just lists them in words. So, I can go on to the conclusions. The project is in the process of developing these fourteen Technical Basis Documents. Attached to these Technical Basis Documents are appendices, and each appendix addresses one or more individual KTI agreements.

Those of you who are familiar with the KTI agreements, there are several that are very related within a particular topical area. Just speaking for saturated zone for a second, there's three KTI agreements specifically related to retardation characteristics of the alluvium. But, they all are pretty much the same wording, just different aspects of retardation in the alluvium. So, we bundled those together into one appendix, and addressed them with the additional information that's available since the SR to support the alluvium retardation characteristics.

The first ones, as I say, DOE has some of these in review right now. Biosphere is the first one that's being completed, not because of importance, it just happened to be that's where it was in the schedule. They were done with their analyses and model reports first, and so they're done with their Technical Basis Document and their KTI responses first.

So, the next three talks, actually, it's two talks, it's three posters, two talks, Bo is going to talk about these two posters up here, and I'm going to talk about the saturated zone flow and transport.

There's some backup slides there for you. Of course when we talk about this with NRC, those backup slides are very crucial. But, I think it's useful for this Board to understand how these fourteen components of the post-closure safety analysis and performance assessment, how they relate to NRC's Yucca Mountain Review Plan Abstraction Groups, and that's a fairly clean mapping, and how they relate to the KTI groups themselves. So, I think that's more for the Board's benefit.

I think there was a question earlier on can we see the mapping of the individual KTI agreements into these, and we do have that, and I think we're trying to find someone to actually print it off for you. There's the June 20th letter to NRC. That mapping has changed a little bit, and we'll present that to NRC next Tuesday.

So, with that, I'll stop. If there's any general questions, I'd be happy to field them, you know, the purpose, the goals, the objectives, the content of these. Otherwise, we'll just go straight into unsaturated zone and saturated zone.

CERLING: Leon?

REITER: Leon Reiter, Staff.

Bob, a quick question. It looks like the real basis for the Technical Basis Documents are really in the AMRs. That's where the meat of the material is addressed.

Do you have available a mapping of the AMRs into TBDs and when the AMRs will be ready? Will they be ready at the same time? Give us some idea about that.

ANDREWS: To answer the first question first, yes, there is a mapping of the AMRs into the Technical Basis Documents. Some AMRs address multiple Technical Basis Documents, as you can imagine. You know, seepage, for example, or thermal seepage, has a direct input into the seepage Technical Basis Document, but an indirect input into the understanding of the chemical evolution inside the drift, because they become related there.

So, a particular AMR might be used in multiple Technical Basis Documents to support the evolution of that particular component.

The second part, I think as John said this morning, about half of the AMRs are completed through their check review and approval process. There's about 80-ish AMRs, I'm going to round all the single digit numbers here, it's actually 85, I believe, and the other 40 are in varying stages of check review and approval.

So, we are using those AMRs, the analyses and model reports, and the supporting information. I don't want to say that it's only the analyses and model reports. They are a principal reference, but as you will see when we talk about saturated zone, there are a number of other directly relevant references and supporting data that support the technical understanding of saturated zone flow and transport. It's not all in the analyses and model reports.

So, the timing of their release I think probably DOE should answer. But, they're in varying stages of development by ourselves, by the labs, et cetera.

REITER: I assume if you're getting a TBD, you want to review it, so you would want to have all the information, the technical basis for the statements made in there, and that I assume would be in the AMRs, whatever there is. Can we assume that all that will be available at the same time?

ANDREWS: No, I wouldn't assume that. But, maybe DOE would like to answer, you know, when the AMRs will be released. Or, they might want to save that one for later.

ARTHUR: We were just talking about them, just like on KTIs, we'll be glad to get the schedule to you as to when AMRs will be available, because Bob is saying it right. There's some that are complete, some are in varying stages, and we just want you to know when we send them, they're complete, or if we send them before they're fully complete, what assessment we have as to where it stands in the process. I think you'd prefer the complete ones. But, we'll get you a schedule on that.

CERLING: Bo Bodvarsson is our next speaker. Dr. Bodvarsson is the Director of the Earth Science Division at Lawrence Berkeley National Laboratory. His areas of research focus on geothermal reservoir engineering and nuclear waste disposal. This afternoon, he will discuss flow and transport in the unsaturated zone at Yucca Mountain.

BODVARSSON: Good morning, everyone. It's been an interesting day for me. I left yesterday from Berkeley and I didn't look at my travel things, and I knew I had to open in Pahrump, so I opened in Pahrump, woke up this morning bright and early with my regular walk for an hour, and I went to the meeting, which was right there in the community center in Pahrump. I saw one car. I saw on car outside there, so I said, well, they're really limiting attendance. And I walked inside and there was a cleaning lady. So, I asked the cleaning lady where is everybody else, and she looked at me funny.

So, then I tried to call a few of our people to figure out was the meeting actually in Nevada, or was I totally in the wrong state, and I called my office and there was nobody there. Finally, I called Bob Andrews and Bill Watson and I was told it was in Amargosa Valley. So, I had to take off and drive like crazy to get here this morning. So, it was quite interesting.

Again, my name is Bo Bodvarsson. I'm going to talk about unsaturated zone flow and transport. I'm going to start by circulating a book which contains 40 journal articles from all four national labs enrolled in this project, as well as the U.S. Geological Survey. And, we are very proud to get the journals, scientific work, that certainly I consider the scientific work to be moderate to high scientific quality.

I waited a little bit for you to ask, but nothing came. So, I'm going to talk a little bit about this. In the outline, I'm going to talk a little bit about Yucca Mountain geology, processes for flow and transport, lessons learned from site characterization. I'm going to try to tell you what have we learned over the last 20 years by doing this, give you a little testing update, then concentrate on UZ flow, which is the top poster here, and then concentrate on UZ transport.

And, basically, I want to give you all the technical bases, all of the data that we have in UZ flow, all of the model predictions that we have done, all of the processes that are involved, and what we have learned over the last 20, or so, years.

I'm going to give you the short version first. And, if you listen carefully to this, you don't really need to look at the slides. This is really the thrust of the whole information I'm going to tell you. And, I know some of you can't see it very well, but it's very important to look at the flow patterns there. We decided, or actually Ike Winograde recommended Yucca Mountain some 20, 25 years ago. He said the attributes are low infiltration, lots of drainage, that means high permeability of the fractures, zeolitic rocks below the repository, and limited effects on faults, or something like that.

We have found in most cases, for flow, this to be exactly right. We have found low infiltration, and over the last 20 years, we've spent a lot of time quantifying infiltration. We have found that the permeability of the rock is so high that you don't build up any water tables anywhere. You can actually have many times for flux through the mountain, and it all drains perfectly. And, we found out that we have sorptive material here below the repository. All of it is true.

What we didn't know, and still don't know real well today is the details of flow. Even if we know that the flow is some five millimeters per year at the top on the average, which fractures does it flow through, we don't know. How many fractures does it flow through? We don't know. And that's very important to recognize.

So, you have a lot of flow patterns, and lots of flow paths in the Tiva Canyon, porous medium starts it out, then you have flow focusing here in the Topopah Springs, and then down here below the repository, you have either the vitric Calico Hills or the zeolitic Calico Hills.

And, this for flow, we talk about processes, which I'll get into--the calibration activities, the results of the models, where is the water going, and how fast, calibration activities, and validation. How can we validate this model? The same for transport. Processes, testing, validation, and results and validation for the transport models.

So, that's kind of what I'm going to describe in this talk, so please don't hesitate to stop me, ask any questions you like, ask me to go through it.

Starting with a little bit of geology, you see where, and it's difficult for you to see, but you see a lot of layering through the mountain, layers of different thickness. Particularly important layers are the Paintbrush layer, which is the second one here, because it's a porous medium layer. And, why is that important? It's important because we believe that water goes into the mountain only once every five to ten years, then a bunch of water goes into the mountain, then nothing for five to ten years, then a bunch of water goes into the mountain.

Now, in Tiva Canyon, this top unit, if you did the measurements, you will see that. When you get to the porous medium units, you don't see it, because porous medium has a much high porosity, and the water splits all around and becomes more steady state than uniform. That's why Dan Bullen's question before, I want to answer it now because I forgot it the first time. He asked if you have a lot of rainfall, like we had recently, will you see it at the repository horizon, will you get water seeping into a drift? The answer is no, because of this Paintbrush unit that splits everything up.

A few years ago when we knew there was going to be a big precipitation year, we actually monitored a lot close to the Ghost Dance Fault, and elsewhere, to see if you saw that, and we didn't see that. And, we think that's the verification of that unit, a very important unit.

So, below this unit, because it spreads things out so nicely, you have pretty much uniform flow coming out of this unit. It varies a little bit spatially, but you don't have discrete flow passing that unit. It's like a porous medium. It's like things like that, this is all a porous medium unit.

Then you come down here, though, into the Topopah Springs, and that's where you start to get these discrete flow paths. In the upper non-lithophysal unit, you have lots of fractures that interconnect. But, in the lower lithophysal unit, you don't have a lot of fractures that interconnect, and that's where you see the discrete flow paths.

So, we think that there's flow paths some 10 to 100 meters apart in the mountain, just like that. But, we've only seen one. This is the one. It's the only one we've seen, in Niche 2. And, you see that there's a photograph of it. It's the only flowing fracture at Yucca Mountain that we've ever seen, in Niche 2. And, look how it looks. It looks like a saturated fracture with a considerable amount of flow into the matrix next to the fracture. Very interesting.

Below here, this is the repository horizon, the lower lithophysal, below here, we don't have as much information, because we haven't drilled as many boreholes that deep. So, we don't know the importance of lateral flow too much of this, as much as we have in the upper region here.

This graph here just shows our numerical grids. It shows that we discretize very finely in the repository horizon, and coarsely away from it, because mostly we want information about the flux going through the repository horizon for the UZ flow model.

BULLEN: Bullen, Board.

You asked a clarifying question, so, since you raised it, can you go back to that previous slide? You mentioned that there was a homogenizing effect to the Paintbrush Tuff. Can you inundate that, or overcome that? And, if so, how much? Or, is it impossible to inundate it?

BODVARSSON: The answer to this is you can. It's only a matter of degree. If you were to have a huge amount of water in one location, it wouldn't spread out. It would all go through. But it would take, I would say, at least $10^3$ to $10^5$ more flux than we have now to achieve that.

This shows some of the ideas from Ike Winograd, the USGS and others, infiltration and flow. We had lateral flow spreading out in the PTn. Faults can be important. Perched water can be important in some regions. We have discrete fracture flow in the Topopah Springs, and going all the way to the water table.

Now, what are the important processes for flow? One process is our present and future climate, and how it infiltrates, lateral flows, because then it helps get less water through the repository horizon, interaction between matrix blocks and fractures, faults, perched water, and coupled processes. And, over the last 15 years, or so, we have been trying to quantify those, and I'll tell you what we have learned.

Transport. All of the processes for transport depend on the flow. So, if you don't know the flow, you can't estimate transport, because transport depends on where the water goes. And, actually, I think this is the key problem in a lot more areas than nuclear waste, in CO2 sequestration, and impact of climate, and water resources, in contamination and EM sites, the nature of flow is always the key issue to all of those problems. If you knew that, the irradiation would be solved, CO2 problem might be solved, and all of the others. It's a very important issue.

Drift shadow effect becomes important, and I'll tell you what that is. Sorption, matrix diffusion, daughter products of radioactive decay and colloidal transport, and we'll talk about all of these.

But, first, let's look back. Let's look back and see what is it that we have learned in general terms from surface based, underground testing, lab studies, and modeling.

Starting with geology, this is going to be very high level. We've done millions and millions and million of geology, as well as hydrology and geochemisty. What have we learned? What have we done, first of all? We've done extensive surface mapping, trenches. We did stratigraphy of all the lawyers, to understand what the layers are. We did detailed line and full maps of fractures on the drift walls.

So, basically, we mapped fractures on the surface. We identified faults. We figured out where all the layers were, and then we mapped all the tunnels, all the fractures in the tunnels.

What has been important to us? What we have learned is we can map millions of fractures in the tunnels, but it doesn't give us the fractures that flow. So, we put all of these fractures with the right statistics, and this is statistics of the different units, how many fractures you see in different units, and what their spacing is, and all of those, we put them into our model to represent the mountain the best way we know how to represent it.

But, then when we matched the data, like geochemistry data, thermal data, isotopes, we have to take most of the fractures out. We have to have much less fracture/matrix interaction because we want to match the data. So, the important part here, that only a very small fraction of the fractures, number of fractures, which is $10^9$, or a trillion fractures, actually carry water.

The stratigraphy has been extremely useful. Any flow problem is on the first order controlled by the geology. It's always controlled by the geology. The low permeability layer here, water is going to flow around it, regardless if you're in Siberia or here or wherever you are.

Detailed fracture mapping, even though I say we don't use half of these fracture, or only a fraction of them, has been very useful to get the statistics. And, understand we have this many 10 meter fractures, we have this many small fractures. This is how they all connect.

Geophysics. We also spent a considerable amount of time on seismic, electromagnetic and other studies in the mountain, put sensors on the top of the mountain, in the mountain, tried to figure out something about fluid flow, hidden faults, perched waters, and all of those things.

It has not been as successful as we had hoped for. The primary difficulty is we can't get current into the ground because the conductivity is so low, we can't get a very good source term. If you can't get a source term, it can't go very deep, and it doesn't reflect and you can't see it, A.

B. Looking at perched water, the saturation and perched water bodies is about the same as in non-perched water bodies, because of the water is in the matrix. We can't see that either.

C. Hidden faults have such limited geophysical contrast, if you will, it was very difficult to see that, too. We tried it, though, and I don't regret trying it. But, the most success has been in radar tomography, very successfully detected saturation changes in the drift scale tests, Busted Butte.

Surface to underground seismic imaging has allowed us to see the intensely fracture zone here that has been posited. We need improvement in geophysical tools if you want to detect large hydrological features or hidden faults.

Water flow, infiltration. What the state of the art was, if put in shallow boreholes, it measured saturation changes using neutron tools. It measured chemistry. It estimated precipitation. It did surface runoffs. It measured stream gauges and water flow. And, then it did water bucket models for wetting front migration.

Lessons learned, which I think is the same at Yucca Mountain as in any arid climate area in the world. These are very difficult approaches. And why is that? It's because you're subtracting two values which are the same order of magnitude. Precipitation shoots. Evapotranspiration shoots. You subtract shoots minus shoots, you get something that is very, very uncertain, because that's a small number compared to the big numbers.

But, what has been so successful I think is the geochemical and temperature data to satisfy ourselves that this map that Alan Flint and the Survey came up with that has 20 millimeters per year, high and lower values, validated, and I think we have done that, and I'll show you that next. No, after this probably.

Saturation. Water flow evaluation, matrix properties. This has been also I think very successful. We measure saturations from cores. We measure matrix potential from the cores also. Matrix potential are difficult to measure. This is the match with the data from our models, and this is the match with the data from the models. We can see the models match fairly well the data. And, actually, this is the potential--these are the permeabilities that come out of it. Sorry about that. These are the permeability values.

The reason this has been successful, this has allowed us to upscale core properties into large scale properties, because the saturation and moisture tensions are hundred meter properties. The core values are centimeter properties. And the problem in all geological studies is this upscaling. How can I use this measurement and this scale to the hundreds of meters scale, and this has allowed us to do that.

What has been less successful is water potential measurements, because there is no tool available that I know of that can measure accurately water potential for one bar, and that's pretty much what the water potential is in the mountain.

Does that matter? I don't think that matters too much, because if it is two bars or a half a bar, I don't think it's very important.

We also found it's practically impossible to separate the effects of fractures from matrix blocks, because when you have a sensor and you stick the sensor somewhere, it sees the volume, and that volume contains both fractures and matrix. And, fortunately, our models, the dual continuum models, dual meaning matrix continuum and fracture continuum, they need separate properties for each one of those. So, this has been difficult for us. And, then some core drying has affected this.

This has probably been one of the more successful measurements that we have done. The Survey put instrumented boreholes in the mountain and air pressures at different locations with hundreds of meters below the surface, and the attenuation of lack of the surface pressure, which is basically the water flux moving through, allows us by matching this data with models that we do very accurately to get permeability values of scales that range from a fraction of a meter to kilometers. And that has also been very useful for our models.

So, this pneumatic data has been extremely successful for us, and this has now started to be used at other DOE sites at Hanford and elsewhere, where this technique is extremely useful.

Now, percolation flux and how it leads to infiltration. Percolation flux and infiltration is about the same. Infiltration, water comes in, water has to come out, because for thousands and thousands of years, you're not going to store water there. So, the total amount of flux at the repository horizon must equal pretty much what we put on top. There are small variabilities due to lateral flows here and there, fracture flow, matrix flow, but it should be about the same. And, this is what we have found.

We have all the separate indicators, these are also highlighted here, and they include infiltration data, saturation data, pneumatic data, geochemical data, temperature data, all of this we used over the last 10 to 15 years to pinpoint that the flow through the mountain is only 1 to 10 millimeters per year, very useful information.

So, the percolation flux and infiltration data has been very well constrained by geochemistry. I think this map is of particular importance. This is the percolation flux derived from total chloride, total chloride content. Since we know fairly well that chloride source, we mix it with water, we get that total chloride percolation flux values, which again, I agree very well with Alan Flint's map of chloride infiltration through the mountain.

Geochemistry. I mentioned that before. What has been done? Geological Survey, Los Alamos, and others have collected pore water samples, gas, perched water samples, looked at bomb pulse analysis, as you heard before lunch.

Lessons learned. This is the Chlorine-36 data. These are the strontium data, and you see the strontium data, there's a direct shift in the isotopes here where the zeolitic rock is, because the zeolites absorb strontium. And, then you have the calcite data, deposition of calcite in fractures over millions and millions of years.

Number one, what has been useful is we are able to model all of these processes. We are able to use them all in our models. Each one separately is not extremely useful to us. Many of them together are extremely useful for us, because it gives us confidence in what we are trying to do.

So, what have we learned? Total chloride, calcite, strontium, Chlorine-36 are useful for different flow phenomena. Total chloride I find out to be most useful because it gives me the total percolation flux with pretty good confidence. Temperature data does the same thing. The controversy you heard on the bomb pulse finding, or you heard in this meeting and last meeting and the one before, and maybe the next, too, I want to tell you a little bit from my perspective, because I think it's important, and you probably know this already, but that's okay.

I believe less than 1 per cent of the flow through the mountain is the so-called preferential flow paths, fast flow paths. Most of it is slow flow, this map here. This small amount of water does not really impact performance. It does not affect seepage, because seepage doesn't care if water goes fast or slow. It just cares is that a lot of water is trying to get into my drift. That's all it cares about. It doesn't affect global flow or large scale flow, and it doesn't affect transport either, because transport is controlled by this map here.

So, it's interesting that it doesn't have a huge effect on what we are trying to do here in terms of what we need for TSPA, or our confidence.

Perched water. As you probably know, there's a perched water body in the northern part, close to the zeolitic rock that is very large. It affects transport. You'll see it in this slide here. You have mixing and perched water, and when radionuclides are carried down here, you have a lot of dilution, or some dilution due to the perched water bodies.

The U.S. Geological Survey has done pump testing to see how big this is. They have sampled chemistry to see what's the chemical nature of this water. And, they emphasize the geochemistry, which is very important for all of us. The geochemistry of water, pore waters, at Yucca Mountain are very benign. It's about 1000 ppm, very low chemistry, and they pump into a lot of other waters.

Therefore, it's very difficult to concentrate the water enough to get very bad conditions inside the drift. Also, it's difficult to concentrate it so much that it has great effect on transport, because pretty much it's just water with a little bit of chemistry in it.

But, the perched water studies have been very useful, and we must take them into account in our models, and we do.

Flow patterns below the repository. I mentioned before when I talked about this map here that we know much less about what happens below the repository than we do at the repository above, for obvious reasons. We have tunnels at the repository. We have boreholes that are shallow, and some deeper. We just don't have as many boreholes that go deep.

So, what we have found, this is not backed up a lot by real data because we don't have a lot of data, that we think given the properties of the faults, that they control to some extent the transport below the repository. And, I'll show you that a little bit later.

Fracture-matrix interaction, conceptual models, and we talked about this before. We talked about there are a trillion fractures at Yucca Mountain. We talked about we stick them in the models, and many of them we have to take out, because we don't have that much interaction.

Now, I want to tell you a little bit more details about this. This is true with respect to ambient flow. You see these fractures flowing and these fractures flowing, and the rest are not really flowing, because you have the flow paths some 10 to 100 meters apart. However, when you come to thermal problems, like drift scale, like PH model or TAT models, steam that is boiled off next to the drift does not discriminate between fractures. Steam will go into all the fractures located next to the drifts and condense in them, and the surface area for imbibition is going to be much bigger than what we think it is in the ambient case.

So, lessons learned. Transport is more sensitive than flow to the fracture-matrix interaction because of diffusion. Condensate imbibition in the matrix blocks is very important to represent all the fractures for the processes.

Now, I will briefly shift gears. Before we start to talk about the UZ flow model and transport model in detail, I want to do a few minutes of what Mark Peters usually does, give you a little brief update about what has happened since the May meeting in terms of testing, very quickly.

Some of the tests have been curtailed in the ESF and ECRB. There are some that remain. The drift scale test in the ESF, coupled processes are a Cadillac of tests, very important tests, very vital for us. Secondary fracture minerals/fluid, inclusions and hydrochemistry by the Survey, which is very important work and is continuing.

Alcove 8, Niche 3 continues where we look at seepage and matrix diffusion, and ECRB moisture monitoring, where we are investigating why are we getting water in the drift is continuing.

First, the drift scale test. As all of you pretty much remember, we had the heated period for four years. We are currently in the cooling period. Temperature in the heated drift is currently, in August of this year, 84 degrees, which is about 16 degrees below boiling. The highest temperature within the rock formation is still close to boiling, 95 degrees. Why are they higher in the rock than in the drift? The reason is the wing heaters, the temperatures are highest in the rock closest to the wing heaters. The cooling in the drift is more effective on the cooling close to the thermal couples in the wing heaters.

Also, there have been some nice validation studies. This is the ground penetrating radar. Here you see velocity tomograph that shows basically the red zone is the dry area where there's only steam present. This is the dry area around the heater test at the end of the heating. So, we have a few meters of dryout, total dryout.

Rewetting of this dryout when we did an earlier--we subtracted the results, and it shows most saturation increases due to condensation where the dryout was, which is totally predicted by our models. What is also predicted by our models is the fact that we can't collect any more water. Water is not seeping into the boreholes anymore. And, why is that? It's because temperatures are going down. The boiling is going down drastically, because the heaters have been turned off. And, if you don't boil anymore, you don't get steam, and you don't get condensate, and you don't sample in the boreholes.

U-Series isotope studies. They're continuing at the USGS. These are important studies, too. They are interested in determining again where water flows in the mountain. They are establishing vertical profiles of U-Series variations in cores. They have observed disequilibrium in Uranium isotopes and Thorium/Uranium ratios, and this disequilibrium reflects water-rock interaction on $10^3$ to $10^5$ year scale.

What does that mean? It means the kinetics of these reactions is very, very slow. And, that you have disequilibrium for a long, long period of time.

They are now looking at lateral variability in Uranium disequilibria from the ESF, and they have calculated Uranium Series ages ranging from 3,000 to 140,000 years.

The final thing about this, they are also collecting chemical and isotopic analyses of pore water. And, again, the sulfates and nitrates indicate perhaps there are some microbial activities.  The high values in the PTn of chloride suggests that we have climate change about 10,000 years ago, and we are now in a drier climate than we used to have in the past.  And, also, these data suggest water-rock interactions with very slow kinetics.

Alcove 8-Niche 3.  This is one of the tests very much on the radar screen for the NRC, and the reason is that they are sort of like KTIs, and they talk about KTIs tied to this test.  This test gives us an opportunity investigate seepage into the drift at the 20 meter scale, injecting water one meter above the niche.

It also allows us the chance to look at matrix diffusion, which is retardation of chemical front moving from Alcove 8 up top, to Niche 3 at the bottom.

This is collection, and the injection of water at the top, collection at Niche 3.

This shows seepage rates in Niche 3 from ponded experiments that was in Alcove 8, and it shows decline in the seepage rate with time, and that's because when you put the constant head on top, the flow rate going in decreases with time and, therefore, the seepage decreases with time. And, I think the seepage is about 10, 15, 20 per cent of what you put in, which is consistent with our seepage model on a much, much smaller scale. So, this has been very valuable from this standpoint.

When you see the word preliminary in here, it just means that these data haven't appeared in a signed off AMR. It means that they are going to be on an AMR, and they're not any more preliminary than the rest of them. They will be checked, but they are still very good data sets.

Moisture monitoring behind the bulkhead. As many of you know, every time Dave Hudson back there goes into the bulkhead, he opens it up and looks around, he sees water generally pretty much at the same location, which is around this area, about 25+02 to 25+40, or something like that. It's around here.

We have done a lot of thinking about what is causing this water. Is it a condensate? Is it seepage? This is still not settled, unless anybody here knows the answer, please let me know, but I think we still don't know this. The key to this is chemistry.

Now, why is chemistry the key? It is because if it's condensate, it doesn't have any silica or chloride, because condensate is just pure steam almost. If it is seepage water, we know exactly what chloride content to expect and the silica to expect, because it's in equilibrium with the prevailing temperature. So, we have taken more measurements of silica and chloride and are trying to test this out more.

What's going on there, there is--is driven by temperature gradients. I believe this is condensation. I believe there is no seepage occurring. I believe this is simply due to temperature gradients. And, let me tell you why, and I've said this before, and stop me if I say it too often.

When you perturb rock anywhere, that means you put a borehole in rock with infinite permeability vertically, or a tunnel in rock with infinite permeability horizontally, be it geothermal a borehole, oil and gas borehole, groundwater borehole, whatever, or a tunnel at Yucca Mountain, you have fluid. This time, the fluid is air, other times it might be oil, other times it might be hot water, geothermal. You will always have flow in the openings because you are creating a permeability that is generally orders and orders of magnitude than what was there before. And, you always have small pressure variations in two intervals within a borehole or a tunnel or whatever.

So, I think everybody can always expect in a vertical borehole to have internal flow in the borehole, or in a tunnel, to have air coming in alongside and leading into the formation on the other side. If air comes in and is cold, and leaves when it's hot, you don't have condensation because the mass fraction of air, mass fraction of water in the air space is higher the higher the temperature.

Now, if you go from a high temperature into a little bit lower temperature, even though it's only a few degrees, the water comes out a solution out of the air, and you get condensate. And I think, personally, that's what's going on there, and this may be a result because when we drilled the ECRB, you generate a lot of friction. A lot of heat goes into the formation. That generates uneven temperature variability. Does that make any sense?

Now we'll go directly into UZ flow and transport. First, I'm going to show you we have seven AMRs which support this. Many of them are completed. This one is undergoing the checking. This one is completed, signed off. Most of these are signed off. We still are in various degrees of completion. I think that's one thing you used, Bob; right? Various degrees of completion.

Unsaturated zone processes. We talked about all of them before. Again, now we're going to look at the data that allows us to understand these in more detail.

The data that we have been working with for UZ flow are geological layering, surface infiltration, water saturation, water potential, pneumatic data, temperature profiles from boreholes, and geochemical data. And I'm going to show you all of our calibrations, and almost all of this work is from our current AMRs.

We mentioned this is the numerical grid we use, very detailed. This is the present day infiltration map.

This is the numerical code we use, TOUGH2 family of codes. It's a typical multiphase, multidimensional, multicomponent code, and we have to have heat in it to match temperature gradient. We have to have multicomponents to get geochemistry, and it must be multidimensional for 3D, and have liquid gas in it.

This is the TOUGH/React code that we use for transport chemistry. It has reactions, and this is used a lot for T&T studies, because it has about 20 chemical species, the clays, the minerals, the different water species, aqueous complexation, mineral dissolution, gases, cation exchange, surface complexation, and chemical heterogeneity.

I'm just going to give you a few examples. You have seen some of them before. Pneumatic pressure, liquid saturation and water potential, temperature, chloride along ESF, and calcite in WT-24.

This shows two boreholes, SD-7 and SD-12. The big ripples you see here at the bottom is the surface signal at the ground surface. This is about a few hundred meters in the ground, which is much attenuated. You see some of the highs are attenuated. The same with SD-12. Matching this simultaneously gives you large scale permeability structures.

Saturation we mentioned before, and moisture potential. Moisture potential are not that relevant, but you see here in PTn, you have very low saturation because this is porous medium. In Calico Hills vitric, you have very low saturations because it's more like a porous medium. And, then you have perched water where you have saturation conditions, and this is fairly well matched.

Temperature data. Again, they are matched, and I was thinking when I heard the question before, I think it was from Paul Craig, that said we have so much variability in thermal conductivity in the lower lithophysal rocks, are we taking that into account. And the data are so limited, that was your question, Paul, wasn't it, something like that?

And, I was thinking the data are very limited, but one thing we could do, and I think it might be very useful, is that some of the boreholes extend through the lower lithophysals, if we do sensitivity studies and see the rock thermal conductivity values, can you actually match the data, and what thermal conductivity values on a scale we'll not be able to match it, it might allow us to constrain some of the thermal conductivity. Because, in actuality, the profiles are all the resource of one dimension or heat flow coming up from the bottom, which is pretty much a steady state, since this has been going for a long time. So, it's something to think about, because thermal conductivities are important for the project.

Chloride profiles along the ESF. Now, this is the whole ESF where we've got chloride that was measured by I think it was LANL, USGS. This is the infiltration data by Alan Flint. This is lower infiltration rates. So, you might say this data goes all over the place, but just with the little bit lower infiltration rates, you can't match the data at all. So, that's why I think the total chloride gives us great confidence in percolation fluxes at the repository horizon.

This, on the other hand, I would personally say the opposite, that this does not constrain the model as well as I hoped it to constrain the model. Here, we have 2 and 20 and 5 or 6, or so, millimeters per year, and actually I tried to find some other data with lower values, or higher values. I did not get that. I know your comment. It was a good comment.

The fact of the matter is why is this not as constraining? The problem with calcite data is, in my view, this is my view, is that we know what calcite we have now, but we do not know how much actually precipitated in the past and has dissolved since then. Is this the hole in one inventory or is there more before? That's the problem with calcite data. And, if you assume this to be all that has ever been deposited, you get some information, but the constraints on our model are not good enough, because we don't know the kinetics well enough, the fracture-matrix interaction area, et cetera.

So, with that, we have calibrated the model, and now we get into results. What could we get from this model? And what we get from this model is flows at the repository horizon. And, Bob said this right before. We get matrix flows, fracture flows, and then we have flows at the water table. All of these, the water flow at the repository, allow us to quantify seepage, because having the total flux is necessary to estimate seepage in all of the drifts. The flow below the repository and at the water table allows us to go after transport, because we need that for transport.

Now, we have calibrated the model. We have developed the model. How do we know it's right? And, that's what is called validation of UZ flow models. This is a common practice with all of our models. Every model in an AMR must be validated, so we must find independent data sets where we actually try the model and ensure it to our confidence that it actually works as intended.

So, we were lucky enough that this Board recommended the ECRB cross-drift. So, that was a golden opportunity for the project to say we are going to do the cross-drift. Now we want to predict everything we can in the cross-drift, and we did that. We predicted air pressures. We predicted temperatures. We predicted the chemistry. We predicted anything we could do. We also predicted the geology. Can we predict that we are going to find the lower lithophysal within 2 meters, or so?

I think overall, and I think there is a report on this, this was very successful. The geology was very well predicted, I think. We used construction water migration underneath the ECRB to validate the UZ flow and transport model, and we used the total chloride in the ECRB to validate the percolation flux. And, we used Carbon-14 for the pore water ages.

And, I'm just going to show you a couple examples, because I'm not going to go into that much detail. Let's look at what did we find from geology. I think almost all the large faults that we predicted, or actually the Survey predicted in this case, were pretty much right on in the predicted report, both in terms of type and size and offsets. Some of the smaller minor faults were encountered as expected. We expected that before. And we found them.

The characteristics of the predicted faults were very similar to that what we thought in the predicted report.

Solitario Canyon, for example, was encountered within a few meters of the predicted location. Orientation of structure, and stuff like that, was very consistent.

You saw me before use the ESf, the Exploratory Studies Facility, chloride to validate the model. Now I use the ECRB chloride not to calibrate the model. Now I use the ECRB chlorides to validate the model. Again, these are the infiltration maps that we believe from Alan Flint and the Survey, we saw drier values than we just did for sensitivity studies. So, again, I think the validation with the results are pretty good.

NELSON: Can you just explain your legend there again?

BODVARSSON: Oh, sorry about that. This is chloride concentration, total chloride.

NELSON: The legend, yes, the different symbols?

BODVARSSON: Oh, this one here? There are several different cases here. These two cases are when you use Alan Flint's percolation flux map for the prediction of the chloride, and don't change it at all, is one of those. The other one is where you allow for significant lateral flow in the PTn. That's the black one. That's the one that matches a little bit better than the other one, because if you allow--it seems like the chloride data tells us there is significant lateral flow in the PTn.

It's true where you have chloride on the order of

100 milligrams per liter, corresponds to infiltrates where it's about 1 millimeter per year, much less than what we think it is now. And the reason I put it on here is so that you will see that even though there is significant variability in the data, this variability is only between 4 and 8 millimeters per year in percolation flux. And, if you significantly reduce it, you go way up here, and if you significantly increase it, you go way down there.

Did that answer your question, Priscilla?

NELSON: I think so.

BODVARSSON: This shows just the cross-section of the model showing the chloride concentration throughout the cross-section.

UZ transport. Before we start UZ transport, are there any questions on the UZ flow, or clarifications? Did I go too fast?

BULLEN: Bullen, Board. Actually, I do have a quick question.

When you were talking about fracture/matrix flow and you said that there were fractures every 100 meters, and that you understood imbibition into the matrix block much better than you understood drainage. Is that a challenge or a problem if you're going to count on drainage to be the mechanism whereby you divert flow around the waste package environment? Or, do you think you have enough understanding of the drainage phenomenon and scenarios? Basically, it was the heated effect where you had sort of the mountain as it is now versus the heated mountain.

BODVARSSON: Yes.

BULLEN: And the imbibition there is actually a little bit better understood than the drainage? Is that the key?

BODVARSSON: No, I think I must have said it wrong, because drainage I think is pretty well understood to the level that we need to understand it. And, let me tell you why.

One of the concerns that I see has always been drainage between pillars. It's a big concern, lots of KTI agreements on it. The permeabilities from our air permeability testing indicate we can have 10,000 higher flux, and drainage between pillars is never going to be a problem for you.

And, so, I think drainage is fine, because the permeabilities are so high. The imbibition is a little bit more tricky because of the fact, like I told you before, you have to use all the factors in the analysis. So, I'm not sure if we are asked about imbibition, but the models of the drift scale test that we use give me a lot of comfort that we are doing okay in that area.

BULLEN: Thank you.

BODVARSSON: Yes, Priscilla?

CERLING: I was just going to say this seems like a good time just to take a few questions, maybe five minutes, and then we'll carry on again. So, Priscilla?

NELSON: Just a few quick ones.

One, your comment regarding Slide 25 where you started talking about moisture, and were commenting on micro-climate effectively, what was happening there, it seemed to feed right into an earlier question this morning that given the variability and, you know, you're going to have drip shields, you're going to have differences in temperature and differences in conductivity, that you are going to have those kinds of very local gradients that can change conditions. Would you generally support that kind of--

BODVARSSON: Yes. I remember a question from this morning, and it probably was a very relevant question. I thought the comment also was very relevant. There are a bunch of complex processes there. And, let me expand on that a little bit.

Number one, we have waste packages with different thermal outputs. Okay? That generates in itself, even though it has the same thermal output, you have drift convection, because that's just how we now from heat transfer. Now, when you have waste packages of different temperatures, you give rise to perhaps different flows of air, heat and water that gives rise to other effects like commonly called cold prop in some cases that may happen during the cooling period. During the heating period, this is not going to happen because the pressure in the drift is always going to exceed that formation. Let me clarify that a little bit.

I said in the last meeting that I thought it was like a half a bar to a bar higher in the formation. Going back to the models, I think it's only a fraction of a bar, like the air pressure in the drifts goes above rock formation. So, it's a tiny increase. And the reason it's so small is because of the permeabilities in the drift.

Now, superimposed on that is the effect I talked about where we generate the shoots permeability, and then you can have different temperature and pressure conditions here and here, so you can have global flow either this way or that way somehow. And, there could be many of these in the drifts, something like that. I don't know. Some might go this, some of them might go that. It all depends on the temperature and pressure conditions in the drift.

NELSON: Okay, thanks. I'm glad you're thinking about that.

Let me ask you about two other things very quickly. First, the change in the fracture wall porosity, I mean I've always imagined this process of boil-off and loss of water to be one which actually decreases fracture wall porosities because of salt deposition that occurs behind. And are you going to search for that in the drift scale test? Have you seen any evidence of that so that you might actually in a boil-off zone have a reduced porosity that might actually stop imbibition from happening on resaturation?

BODVARSSON: Yeah, we have thought a lot about this, and let me give you a short answer.

In the drift scale test, we haven't looked at that yet because we are still in the cooling cycle of the test, et cetera. More relevant information in following.

It was thought a few years ago that this would be a major problem because you would have all the salts there, and not only with the fractures and lower the porosity, but when you also leave that thing, you would get this very detrimental drying with high concentration of sodium chlorides and all this bad stuff, and our waste package would just have a horrible time, you know, and we don't want our waste package to have a horrible time, obviously.

What we find, though, is, and again let me emphasize this, the water at Yucca Mountain is so dilute that when we do our T&T models over hundreds and hundreds of years, there's just not enough chemicals in the water to have significant salts precipitate on the drift wall. Just within the next one year or so after you start rewetting, it's back to normal. It's just because there's so little chemicals in the water basically.

NELSON: That's something testable?

BODVARSSON: Yes.

CERLING: Paul?

BODVARSSON: Did that answer your question, Priscilla?

CRAIG: As usual, you're giving us a lot of information, making a very clear case, and what I'm attempting to do is to reconcile the kind of a case that you're presenting to us today with the kind of case that I see coming out of TSPA. And, I see some tensions.

The one-on analyses that we've been show suggests, at least the most recent ones that I've seen, that we've seen here, suggest that if you take away the engineered system, you're close to the regulatory limit at the 10,000 year point. Within statistics, you're at it. That would be one example.

The second example is the bay through curve for the unsaturated zone, and since I didn't remember numbers, this book had just come around, I looked up Robbins in this article in there, and I discover it's around 1,000 years, or so, which is consistent with what I had remembered.

So, when I think through what comes out of the TSPA calculations, I see a picture which is much more pessimistic than the one that you're presenting here. And, so, I'm struggling to try and reconcile these. Is the TSPA so conservative that we simply shouldn't believe that aspect of it, that we should actually in our minds scale the TSPA down so the dose is orders of magnitude lower? Or how do we go about thinking about this? Or is the TSPA such a mixture of conservatisms and non-conservatisms that we can't draw conclusions? But, I have this tension between the story you're giving us and what I've heard previously from the TSPA.

BODVARSSON: It's a tough question, but I'll try and answer it.

I see it a little differently than you, Paul, maybe because I'm very close to it and have been close to it for a long, long time, way too long a time actually. I need to go do something else, but that's a different story.

There are so many items represented in TSPA very, very, very well, like the repository horizon three dimensional flow is put in TSPA directly. Directly. It's used exactly like it is. The water table flow and all three dimensional flow is directly incorporated into TSPA.

Now, our seepage measurements, and our seepage models, are also put in the TSPA, but with some conservatism. And some of the conservatisms are there, and for good reasons in many cases. We don't know how the climate is going to be 600 or 2,000 years, so we assume very conservatively much higher infiltrations that may lead to seepage in some cases.

I think it's very considerable. I know Bill Boyle and others have said this for many years, it's very considerable that you will see no seepage into the drifts over a long, long period of time. So, perhaps we are a little conservative there. But, maybe we need to be.

Another example, transport. Again, these transport break-through curves I'll go through with you represent our UZ flow, and are pretty well calibrated against the vapor we have, and again we are a little conservative.

For example, with shadow zone representation, believing matrix diffusion as much as we have, because we don't have the basis that we want to have to be able to reliably put it in there.

So, did I answer that? So, I have a little bit in perspective, I think.

CRAIG: Well, the perspective that I'm hearing from you is that the TSPA is extremely conservative, and that things are likely that the real situation is likely to be much better. If that's the case, then it seems to me that we desperately need, the project desperately needs that case to be made. Because, as things are looking now to me, the mountain alone is not capable of doing the job with high confidence, and the metals are coming under heavy duty attack. And, when you put the whole package together, the situation simply doesn't look very good.

If the situation is as good as you're saying, it would sure be nice to see that laid out someplace.

BODVARSSON: Well, I believe the natural system can do considerably more than it has, and I was hoping the S&T work can allow us to establish that over the next few years. So, I agree with you to some extent.

CRAIG: Well, the conclusion that I would draw from all of this is that the project is marching ahead of the science. I don't expect you to answer that.

BODVARSSON: I wouldn't say that. I wouldn't say that. I would say, and I understand where people are coming from, if our waste package is so good that it lasts 50,000 years, that's--we've got to rely on that as a barrier. But, at the same time, I agree with you. I think the natural barrier again needs to be strengthened over the next few years. And I think Margaret believes that, too. So, I think we all agree with you to some extent.

I think the project is ready to go for the license application, because of the redundant barriers we have, but I think strengthening of it would be essential over the next few years. That's my opinion. Does anybody else want to say the right answer?

CRAIG: Just a couple of quick questions.

ANDREWS: Not that I'm going to disagree with Bo, but I think it's important to understand that when you present a, if you will, a nominal case, which Bo is presenting, for present day infiltration, for a present day understanding of infiltration, and the distribution of that to percolation, and ultimately when he gets a chance to talk about transport, there is uncertainty in that infiltration. There is uncertainty in that percolation, and there is variability in both of those.

That uncertainty, which is portrayed, well, in fact, he doesn't have it up there right now, but it is portrayed in the representations, and you propagate that uncertainty appropriately through the models that leads to uncertainty in seepage. It's not conservatism. It's uncertainty. And, uncertainty in chemistry and uncertainty in transport. And you have tried to adequately represent that uncertainty, based on present information.

And, things do change with time. I think we have to understand and represent that. We're looking at point time here for all practical purposes, and we're looking at 10,000 years time, and climate will change. We think we have an understanding of how it will change, although there's uncertainty in that that we also try to characterize and represent in the performance assessment.

The performance assessment models are these models. They're not something different. It's exactly these results that are used in the basis of the TSPA. It's not two separate sets of analyses or models. They both try to characterize the uncertainty, and propagate that uncertainty through to system performance.

It is true that if we had today's climate, and today's climate existed for the next 10,000 years, the performance of, you know, with respect to seepage, with respect to transport would be different than the fact that there's a climate change or a projection of a climate change, and uncertainty in that climate change, and the representation of that.

So, I do not want the Board or you, Paul, personally to take away the idea that there's some disconnect between the post-closure safety analysis and the post-closure science that Bo is representing here for the unsaturated zone. They are one in the same.

CRAIG: Thure, can I continue with this for a second?

CERLING: Yes, I'll let you continue with it. But, then we'll let Bo finish his talk.

CRAIG: Okay. Well, we're getting to what I consider to be really key issues here. I actually, Bob, didn't say very much about uncertainty in my comments. In fact, the only thing I've said all day about uncertainty had to do with my concern about the variability within the lower lith. and the thermal conductivity. That's a very big uncertainty related question.

But, my comments to Bo actually were not uncertainty related. The base case situation, one-on situation, if you take away the engineered barrier, suggests that you're very close to the regulatory limit. And, if you want to be confident, then the only conclusion I can draw is that you really need the engineered barrier.

Now, Bo's presentation here suggests that the base case, if you properly took into account conservatisms, would in fact be much better. But, I'm talking about a base case using a one-on kind of an approach, and it sure seems to me that you can't survive robustly unless you've got the engineered barrier playing a significant role, not just a backup, but a significant role. I think that's a robust conclusion based on all the data that we've seen to date.

Am I wrong?

BODVARSSON: Is he wrong, Bob?

ANDREWS: I think all the individual components, including the uncertainty in those components, for the TSPA-LA is still being developed. I mean, I think the Board has seen bits and pieces of it at the last meeting. You're seeing some more bits and pieces this meeting. I hope we have the opportunity to present the additional bits and pieces in future Board meetings, including things like, you know, seismic effects and volcanic effects, et cetera.

CRAIG: I can't assess what you're going to present in the future. My statement about robustness was based on what has appeared in the test.

Now, if you're going to tweak parameters and make things different, then it's going to be another situation and we have to look at it again. I think the only thing we can do is to look at the information that's presented to us as of this time. And, my question then is was my statement correct with respect to the need for the engineered barrier, based on information up to this point, or if it's wrong, is it correct based on information that will be presented to us sometime?

ANDREWS: Well, I think if you compare--Peter will talk tomorrow, and I think the Board has heard that the principal driver on post-closure performance in fact is not the waste package, it's the volcanic event, and the probably that volcanic event occurring and the ultimate consequences associated with that volcanic event occurring.

Yes, we have done calculations, taking away a whole function. No one expects a whole function to disappear all of a sudden. You have the uncertainty in the performance of a function, whether that be the waste package or drip shield, and that uncertainty you want to adequately capture. But, I don't think it's fair to look at a one-off, one-on calculation as the sole determinant of a barrier's significance to the overall post-closure performance.

CERLING: Let's let Bo finish his presentation.

BODVARSSON: Okay.

CERLING: You were on 42.

BODVARSSON: I think this was an important exchange to Paul. I agree with you. I think all of us would be comfortable if the natural system would meet the standard very well and the engineered barrier would meet the standard very well. And, I totally agree with that, and I think Bob does, too. I think all of us agree with that.

UZ transport. Did I go too fast before, or was it okay?

NELSON: You've got 21 minutes.

BODVARSSON: 21 minutes. Let's try to make that. UZ transport. We talked about all the processes before, and the most important statement is that all the transport processes depend on the details of the flow, and we don't have a lot of information about the details of the flow. And then all the other things that we need to consider in the transport model are scales that vary from the individual fracture scales to a drift scale to a mountain scale.

As always, we start with testing. What are all the tests we have done to gain confidence, and why we think that we've got a good data source for our models for calibration and validation. The main transport tests are Alcove 1 test in the ESF, Alcove 8-Niche 3 test, and Busted Butte. Busted

Butte for the vitric, porous medium for the Calico Hills, and Alcove 8-Niche 3 and Alcove 1 for the fractured part of the Topopah Springs, looking at fracture/matrix interaction, and matrix diffusion.

So, real quick, you all remember the Alcove 1 tests that we actually put water on the surface and we monitor how much water seeped into Alcove 1, and we put a tracer on the surface, and we investigated how quickly the tracer would go through this 30 meter length of fractured Tiva Canyon. We predicted seepage. We predicted tracer breakthroughs before we actually did the test, and the results found that matrix diffusion is very important in the tracer returns.

This just shows some of the data sets. The green is the computer model. The red is the actual data. This was different phases of the test. Here, we injected less amount of water and we got less seepage. We did another test for 200 days, and then we did a very high rate test, and got considerably more seepage in the last test. All of these, though, are way above the natural condition of Yucca Mountain. This is orders of magnitude above the percolation flux going through the mountain right now, obviously.

Then we did tracer tests, and we used the model to predict them. This happened to be actually tracer application concentration, and these are the concentrations that we found in Alcove 1, as well as the model results.

And, they showed a lot of matrix diffusion during those tests.

Alcove 8-Niche 3, I showed you this before. You know this is 20 meters of fractured rocks. The interesting part of this test is that you go from one unit to another unit. This allows us to test somewhat is there a barrier between those two units, and flow goes through them.

Several results. This happens to be the results for bromide here, and benzoic acid here, and these are the breakthrough curves at Niche 3, and the benzoic acid molecule is much bigger than the bromium molecule. That's where you get much earlier breakthrough times.

What does that mean? It's so big it doesn't go into the pores. So matrix diffusion is not very important. If you had much bigger molecules than the pore sizes, of course you can't have matrix diffusion retarding that chemical. So, this is a very good validation of the matrix diffusion concept for different sized tracers, because, of course, radionuclides also and colloids also have very different sizes, as we'll show a little bit later.

This shows the seepage, the model, and the data. This shows the water velocities from the bottom of Alcove 8 to the top of Niche 3. It's measured actually in a borehole, measured by neutron techniques the arrival times. And, they agreed pretty well with the data, and here are the results of the tracers. This was actually in a fault. This tracer test extended between those two alcoves and niches.

And, one interesting thing here is that it was not enough to have a fault plane, matrix diffusion through a fault plane. We had to increase the surface area between the fracture material, the fault material, and the matrix by a factor of 60 in order to match the data. If you didn't have 60 way above here, then you're way above in the concentration of the radionuclides. And, that suggests that the surface area for diffusion was significantly more than the fault of this test, which is very promising for performance.

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

BODVARSSON: Yes.

BULLEN: On the seepage, did you do a mass balance on water in and what you collected? And, can you tell us what kind of recovery that you got with respect to the Alcove 8-Niche 3 test?

BODVARSSON: It's like 20 per cent seepage, basically, if I remember correctly.

BULLEN: So, you got 20 per cent of the water you put in back?

BODVARSSON: Yes. And we get that in most cases, and actually it's a good question. One of the KTI agreements was actually to do a mass balance test where you tried to put this lock to collect around the niche, and we haven't done that successfully yet. So, we haven't done a true mass balance on this test.

Busted Butte is a facility away from actually the ESF and ECRB. It's an independent testing facility where we are actually testing the Calico Hills, the very top of the Calico Hills formation with this facility. This happens to be very highly permeable, porous medium, if you will. It almost breaks apart when you touch it. It was a high permeability, but it looks like the vitric tuff of the Calico Hills.

A lot of various tracer tests were done here with different boreholes, looking at plumes, looking at tracers, have actually mimicked the radionuclides to try to get at the effect of sorption on neptunium and other radionuclides, as well as to investigate the basic assumption in our model that this unit behaves like a porous medium unit.

This just shows the tests that were used to follow around, and then different injection packers, and observation borehole completions. The basic observation was that it behaves very much like a porous medium with a very well-defined plume pattern, and how we separate porous medium from the fracture medium, of course if you inject into a borehole, you get kind of a response around it, and we'll see that next.

This happens to be a fluorescein injected into this borehole here, and you see the tracer. This gets fluorescein, varying amounts around the borehole, indicating very little preferential flow path, and I think this happens to be a fracture that has very little plane in the transport mechanism. So, it justifies very much our assumption, at least as a porous medium.

This are tracer protocols from some of the boreholes that we used actually for validation of our transport models.

So, with all this testing, now we go into the model development and how we use these test data to validate our model. And, I'm going to start with the source term, the drift shadow concept, talk a little bit about the vitric and zeolitic rocks, and then numerical tools, and then some brief results.

As we talked about before, if there is no seepage into a drive, water moves around the drift, and you have to have a dry zone here at the bottom, and this dry zone here at the bottom is where you have diffusive flow and diffusive transport that may take thousands and thousands of years to get through this drift shadow zone.

What we do in TSPA now is if there's no seepage into the drift, TSP accurately allocates that these radionuclides cannot go into the fractures, because there's no flow in the fractures, they must diffuse into the matrix, because diffusion is only dependent on the saturation, and all the water is in the matrix, so the diffusion makes the radionuclide go into the matrix.

However, the current TSPA also assumes after it goes into the matrix, you have water flow all around them, just as if you had percolation flux, which is conservative in that respect. So, this shadow zone may extend significantly down from the drift, and there might be substantial benefit there.

So, this is one example, and I think Bob and I and all of us agree there are areas within the natural and engineered barrier system, and we talked about that before, that we actually could get more benefit, but we have to test those. Certainly this is one. Another one is transport through the waste package, transport into an invert where we assume that perhaps there is a continuous water flow from the waste package through the invert during the thermal period when everything is dry. So, there are several things that can benefit us in the long term that we could look at if the project decided to do that.

CORRADINI: If I could ask the question, since you pointed to me and I nodded.

The assumption right now is that there is a liquid pathway that allows diffusive flow in and out, regardless of temperature, if there is a, quote, failure in the package wall.

BODVARSSON: That's my assumption.

CORRADINI: Okay. And, as best I can tell, as long as I keep on thinking about it, I see no physical reason why that can be.

BODVARSSON: Right.

CORRADINI: So, it's not correct?

BODVARSSON: It's conservative.

CORRADINI: It's not correct.

BODVARSSON: It's not correct.

CORRADINI: That would be my interpretation of conservative, that is, it's so bounding as to be not--I can't see a physical way that can exist.

BODVARSSON: Bob is going to stand up and say it's almost correct.

ANDREWS: In a separate presentation, I think, and we can talk about it.

CORRADINI: That's fine. I just wanted to make sure because I think the concept, the fundamental concept here is is there is a transport path always available if there is a penetration of the package.

BODVARSSON: Yes. And, I think your point is very well taken because I have been arguing for quite some time that we need to take more credit for the high temperatures, and now we are taking credit for the high temperatures and protecting the waste package for above boiling temperatures, because we don't think any water is going to come in. This is an additional benefit that temperatures may help us.

Now, this is just the numerical model that fit a case that we applied the source, rather than putting it into the fracture where we have short travel times. This is only for 41 meters, or so. You can increase the performance just alone by putting it into the matrix by orders and orders of magnitude.

We talked about the zeolitic vitric, the importance of a zeolitic vitric, and I want to explain it in a little bit more detail. Imagine a fractured rock, like you have here, where fractures are already here, and then you have porous medium rock. So, the complexity in the transport is going to be tremendous. Here you have transport through the fractures, and all of a sudden, you hit the porous rock there, like the vitric rock, and you spread it out and delay it a lot. So, there is tremendous performance from the vitric Calico Hills if there is significant sorption in it from neptunium and other radionuclides. So, this physical process is important to keep in mind because the vitric part is mostly in the south, which is the blue stuff here, and the zeolitic part is in the north.

Again, we use the TOUGH2 family of codes now with the different radionuclide solutes, colloids, parents and daughter products, et cetera, and other particle trackers, similar codes, numerical, three dimensional codes.

Now, I'll just give you a flavor of some of the breakthrough curves, what the timing looks like here. This is Technetium-99, and we pick that because it's not conservative. What was the word you wanted me to use, Bob? Yeah, it goes with the water. It doesn't sorb, or anything like that. So, you have here just three climate states, what we expect to be the mean, the high infiltration and low infiltration. So, you see there is a significant effect of our infiltration. Here, you have the low infiltration. The 20 per cent is after some tens of thousands of years compared to the mean.

You take now a sorbing, and this is very slightly sorbing radionuclides. The kd for neptunium is generally on the order of one to two, or so. And, you see significant delay just because of this low kd. So, kd is very, very important is sorbing those radionuclides on the rock surfaces. Again, there is tremendous impact on infiltration on these results.

This shows now the results at the water table. After 100 years, you have very minimal breakthroughs. But, these breakthroughs correspond to--that we have here in the northern part, Pagany Wash, and stuff like that, because with the sets that we have for faults, most of the radionuclides in water tend to concentrate in the faults, and it goes to the water table.

Is that correct? And I want to give you a little-- I'll tell you again that we don't have accurate information deep as we do in shallow in the system, and we haven't tested faults as much as we kind of would like to, I would say. After 1000 years, again, you have more concentration along the faults, and then you have seen significant plume coming through the vitric part of the Calico Hills for Technetium.

PARIZEK: Bo, can I interrupt a minute?

BODVARSSON: Yes.

PARIZEK: Is that an assumption that waste packages would be in those fault zones, Pagany Wash and those others?

BODVARSSON: No.

PARIZEK: Even if it's near them, this would happen?

BODVARSSON: No, this means that the water that leaves the repository with the radionuclides, even though the waste packages are far away from faults, will tend to, due to lateral diversion, because the tilting of the layers, and stuff like that, will tend to migrate towards the fault downstream of the tiling. And then, because of the permeability of those, it goes vertically down in most cases.

PARIZEK: You're not using the faults as a way to get seepage onto the waste packages?

BODVARSSON: No.

Again, now, this is for neptunium, and again we just see the influence of fault and much lower concentrations here because of the sorption on neptunium. Again, preliminary just simply means this AMR has not been signed off yet.

Now, just briefly parent-daughter decay transport. This is important for some of them. This doesn't show up very well for some reason. This happens to be americium. This should be 241. It goes into neptunium and uranium and thorium. This is the source term. Then neptunium concentration, then uranium and thorium concentrations. So, you see we have to take those into account in our modeling of radionuclides.

This is just colloids, and very briefly, looking at colloids, different sizes of colloids. The bigger the colloids the worse for performance because they can't diffuse into the matrix. And, therefore, the small colloids, like the 6 nanometer $PuO_2$ rock colloids have no problem at all. We have very low concentrations of those.

For larger sized colloids, you have significantly higher concentrations, and I'm sure this is included in the TSPA as the size filtering.

This, again, just looks at the declogging models. Declogging meaning that if you have a colloid that diffuses into the matrix, if there is no declogging, it means it just sits there and cannot move away. If it is declogging, then it means it can also mobilize and go back into the fractures and move as a colloid. So, this is an important mechanism of colloids. What we find out, however, with and without declogging doesn't make any difference in the UZ for these parameters.

So, we are next to the end. Uncertainties in transport models. There are certainly uncertainties in the flow conceptualization and parameters. Climate uncertainties we talked about. Matrix diffusion, sorption and filtration of colloids.

This just shows one model uncertainty in the active fracture parameters that have a significant effect. This is the number of fractures that actually participate in transport.

So, after all of this, and I'm sorry it took so long, but conclusions, and these are the conclusions I see for UZ flow and UZ transport. I think the available data provide significant constraints on the model. I think that it's well calibrated using pneumatic saturation/moisture tension, perched water, total chloride, strontium, calcite and temperature data, and that each one of these can't do it alone, that the multiplicity of all these factors really helps us gain confidence in the model.

I think the validation against the cross-drift ECRB has been very useful. The global water flow is well represented, but the details are much less understood. We don't understand which fractures flow, how far, and that also could be important for performance. Other major uncertainties, van Genuchten, fault properties and chloride, especially below the repository horizon.

Transport. I also think ongoing and completed tests provide input data that constrain the UZ transport model. We still have Alcove 8-Niche 3 tests ongoing that are going to give us useful information. And it allows the project to take substantial credit for this important barrier.

Tracer tests using Alcove 1 and Alcove 8-Niche 3 provide clear evidence of matrix diffusion. Tracer tests in Busted Butte have confirmed the porous medium nature of this unit. Colloidal transport is significantly affected by colloid size, but not much by kinetic declogging.

Daughter products must be taken into account. Greatest uncertainties are detailed characteristics of flow, active fracture model, efficiency of matrix diffusion. It's expected that significant additional benefits could be achieved by shadow zone and other means if needed.

That's it. Did I make it?

CERLING: Okay, I'm going to start where we left off last time, which actually Dick was next, and then Dan, and Ron. So, you guys are all on the docket. I see you, Dave. We've got a good lineup, so we'll try to do this in about 15 or so minutes, and then take a break. Dick?

PARIZEK: Parizek, Board.

First, I want to compliment the organization and the presentation in terms of the logic that you present to us. If this is more or less how all of these future reports are going to be organized, it will be possible to actually read it and maybe comprehend it, and know where to go to find the support for it. In the past, it sometimes has been hard, outsiders have said they've spent a whole year and still couldn't figure out what was going on. But, I think this process and what we heard at the last meeting is leading us in that direction to be able to comprehend it. And, although that tree of yours looks a little artistic, more so than scientific, but that's all right.

BODVARSSON: You call it a tree?

PARIZEK: Yeah, it's got green leaves, got birds, and so on, but that's another matter.

A couple points. First of all, the nuclide sizes that might allow diffusion of a conserved species, I didn't get to think about that or to check them out. But, of the ones that don't want to decay, are they the type that would go into matrix locations because of their small size?

BODVARSSON: Some of them are, yeah.

PARIZEK: Some are, some aren't, but would some of them go in there that we're worried about, the long life ones? And, maybe you don't know.

BODVARSSON: I can't think of it offhand.

PARIZEK: Offhand. But, somebody can look that up later and see if you get credit for some of the small ones.

BODVARSSON: But, similarly, I think that's all taken into account in the TSPA in terms of what is allowed to diffuse, because we know the pore sizes very well, and we know the molecule sizes very well.

PARIZEK: So, that ought to be something we can see that you might get some extra credit for the long lived ones.

As far as the colloids, I come back again, having raised this question before, it's really hard to see the evidence of colloid movement in the unsaturated zone, whether they're large or small. And, the large ones are less likely to be tied up than the small, but the evidence for them being tied up, or even existing, in the unsaturated zone still is wanting. I don't see the data for it, and I'm not sure the program has found data for it.

BODVARSSON: Do you want me to answer now?

PARIZEK: Well, if you can.

BODVARSSON: I think your point is absolutely well taken, and that is we don't have much information about colloids in the unsaturated zone. One thing I want to point out, and I think it's important with respect to this, for drifts where you do no have seepage, and you actually have a shadow zone, if we can verify that concept, and all believing it, and et cetera, colloids cannot move, because they cannot diffuse.

PARIZEK: That brings up the question of just documenting somewhere a drift shadow. Now, has the ECRB been open long enough to have one? I mean, could we look in the floor of our present--

BODVARSSON: No.

PARIZEK: It's not old enough; right?

BODVARSSON: It's because you have to look for chemicals that have been in there for a long time. You can look into a house, though.

PARIZEK: So, you're still looking for a candidate place where that can be demonstrated or documented?

BODVARSSON: A possibility. The project has to weigh if they want to go in this direction, or other directions. We will have to figure that out.

PARIZEK: You had a slide, I think it was the Illustration 22, that showed the chloride differences on top of the PTn versus below.

BODVARSSON: Yes.

PARIZEK: And, I was looking at that earlier and thinking I wonder how to explain that. You then suggested it might have been the glacial climate change versus the present climate where ET losses would build up the chloride up above. That would suggest then that the waters down below are all Pleiscecein or Pluvial?

BODVARSSON: Yes.

PARIZEK: And is that true with the age dates that's known for that water? I mean, is that consistent?

BODVARSSON: Well, my indication, at least from what I know, is that we have done a rather detailed modeling of the total chloride using much less infiltration, 10,000 to today, from 10,000 or 20,000 years ago when we had a wetter climate, and we match this chloride very well. That suggests also that the deeper water is older than, of course, 10,000 years.

The ages that I know, and Zell or somebody else can do much more details on this, is perched waters are 10,000 years plus or minus a few thousand. Pore waters are thousands of years old, unless you believe the Chlorine-36 Livermore/USGS 300,000, 200,000 year old water.

PARIZEK: I mean, we would have gone through a number of pluvials and glaciers, and as a result, you could really have signatures of chloride with depth, would not surprise you to find stratification in that regard. I didn't happen to see it there. Now, is that consistent with the nitrate again being sort of higher down low where the chloride is lower?

And then I was thinking, well, is that then glacial waters, and if so, what was going on at that climate time to make more nitrate?

BODVARSSON: Why don't we have Zell tell us about nitrate so we get it from the horse's mouth. I thought the nitrate was here.

PARIZEK: Nitrate is good for performance in the right combinations. I just wanted to understand the origins of it at depth.

PETERMAN: Zell Peterman, USGS.

You're not going to get much from this horse. This is pretty new data, for one thing, and we haven't had a whole lot of time to really think about it. One possibility is that there's some sort of de-nitrification occurring there due to microbial activity. That's a possibility. We didn't show the bicarbonate. The bicarbonate generally I believe increases with depth. So, if it's a temporal thing, then, yes, you can try some sort of other model.

PARIZEK: So, it's still new enough and at least elevated--

PETERMAN: Basically, they're still working on this profile in Denver, trying to fill it out.

PARIZEK: Okay. Perhaps we'll get more information later on that one.

The other thing is you showed us the perched water body. The U.S. Geological Survey did pumping tests on perched water bodies to estimate volume. And, then, having created a draw down cone, was it observed whether the cone resaturated or refilled up, rather, when you shut the pumps off? And, if so, can you use that as evidence of what the percolation rate might be, and as a result, recharge to the perched lenses?

BODVARSSON: Can you put up the perched water one?

PARIZEK: Do you see where I'm going? I'm just trying to see whether your deep percolation is reasonable in view of what the draw down consequences might have been. Oh, so you didn't pump that much water?

BODVARSSON: No. The perched water in the north close to UZ 1 and UZ 14 was pumped for a long, long time, and there were no boundary effects. So, we don't know how big it is, but we know the minimum size on it was fairly large in volume. The perched water flows through ST 7, was very, very small. This is a tiny water body, you know, it's only like 20 meters away from the rock.

PARIZEK: Yeah. But, as you sucked it dry, then did it reappear? And, how long did it take to reappear and, therefore, what inflow rate would it take to rebuild it?

BODVARSSON: It reappeared. The UZ 14, UZ 1 is almost like an infinite amount of water because we didn't see any boundary as it came back. That's my understanding of that one.

PARIZEK: Okay.

BODVARSSON: But, the other one did not, if I remember correctly.

PARIZEK: But that would be calibration, again, against the infiltration rates that made it to the perched body.

BODVARSSON: Right.

PARIZEK: So, I didn't know whether you have a data set that would work that way.

The pneumatic tests on the faults show high permeability on the fault zones in the unsaturated zone. What kind of contrasts in values between the faults and the non-faulted rock? Because, you know where I'm going with this. If we went then to the water table, would similar contrasts also be expected there? That's Bob talk coming up. Maybe we can defer it until then. But, I just wanted to know what kind of contrasts you found in the unsaturated zone.

BODVARSSON: Let me give you an answer to that, and then maybe Gary Le Cain can help with that.

I actually find the opposite. I find very little, surprisingly little difference between core permeability and fracture permeability around it based on Gary's result and our results. I expected it to be much larger than it is, personally. So, we don't see a lot of permeability difference from the pneumatic data that I have seen. And, again, Gary can correct me if I'm wrong.

However, the temperature data is very revealing in terms of pulse, and we haven't sorted that out as well as I would like to sort it out. And I think the temperature analysis of all the boreholes, and we have 30 boreholes with temperatures, and I strongly believe the temperature data is a strong indication of percolation flux. A lot of the boreholes with the highest percolation flux were close to faults.

So, those are the true data I've seen. Was my statement wrong, Gary?

LE CAIN: Gary Le Cain, USGS.

About an order of magnitude, I'd say, Dick, we generally found in the very limited UZ fault testing that we have done.

PARIZEK: I guess the gridding was so refined in the vicinity of faults, it was almost as if you anticipated a big--

BODVARSSON: Yeah, actually I should have told you this before. Our previous models had like 100 meter fault representation, but now we are down to 20 meters, or so, much more realistic fault models for all the major faults. That's why you see those very narrow lines in these faults when we see things going toward them.

CERLING: Okay, with the first question we have used up 15 minutes. So, if each of us behaved in this fashion, we're going to be in big trouble. So, if the remaining people could restrict their things to one very concise good question? Dan is next.

BULLEN: Bullen, Board.

I guess I can be concise. I'm not sure it's going to be good. But, when you put your Mark Peters hat on, you always open up the vaults to all the other types of scientific investigation underway. And, this actually harkens back to seepage, and I just wondered if you could give us your comments or opinions on the seepage that was noted in Alcove 7? And that's all I have to ask, Mr. Chairman.

BODVARSSON: That was a very concise question. The seepage in Alcove 7, I was just talking to Dave Hudson about this, and I'm not sure if we know it's seepage yet, because I don't have the chemistry of that. Maybe somebody else has the chemistry of that. And, I'm not sure it's seepage or condensation in Alcove 7, nor am I sure if it is in the ECRB. I personally believe the ECRB, the water we see there is condensation, and I think it can be explained simply by the temperature gradient from actually doing that tunnel.

Alcove 7 has certainly been there longer, and if somebody, and I was asking Dave Hudson, and I didn't think he said it was seepage. But, I may have been wrong. Is Dave here? Yes, am I right or wrong?

HUDSON: David Hudson, USGS.

I think that Alcove 7 and the bulkheads are very different. Alcove 7 is only 200 meters. Relative humidities don't get up into the 100 per cent range. They get up to the 95 sort of range. So, you don't really have a lot of chance for condensation. The relative humidities aren't so high.

What we see in Alcove 7 is more looking like percolation, wet on the ceiling, almost coming around the walls, the walls appearing wet, fractures also appearing wet.

Was there a lot of seepage? There was drip marks on sheets. There wasn't enough to collect those, though. If you're going to answer it in chemistry, you can't answer it that way.

BULLEN: Thanks.

CERLING: Ron?

LATANISION: Well, this may be something of a corollary on that last question.

Is it your position that the water in the bulkheads is essentially an artifact of the processing of the drift and is not of consequence? Or, is there some message there that's important from your point of view?

BODVARSSON: My personal opinion is it's a very important issue. And I'll tell you several reasons.

Certainly, if there is a lot of condensation in different parts of the tunnels, I mean, the chemical environment, the humidity on the waste package, water being there, shadow zone concept, all of those are issues that we have to answer by what is the origin of this water, and how much of this water is going to be there in the future.

LATANISION: Right.

BODVARSSON: Now, my answer to you, and I believe in regard to this, is that it is condensate, like I said before, and I think we can demonstrate it with models that take into account the amount of water we have seen there by modeling air flow in the tunnel coming from hot areas to colder areas.

LATANISION: Right.

BODVARSSON: Those temperature gradients are developed by the construction activities, tunnel boring machine, the electronic and wires in place now, I believe.

LATANISION: So, that simply was an artifact?

BODVARSSON: Well, it needs to be modelled to explain it, because if you don't explain it, we can't quantify it in the future because even if it is an artifact, then the repository drifts are put in place, they're not along an isoberm exactly. They're almost like an isoberm, but not quite along an isoberm. There is going to be a few degrees difference in centigrade from one area of the tunnel into another area of the tunnel. How much that will cause condensation depends on how much air flow would flow in and out, and what the temperature gradient is, and stuff like that. So, that could be an implication for performance on the rock.

LATANISION: Okay. Let me then turn to your Slide Number 53, if you'd put that up? This is the slide which you and Mike had a conversation about, whether it's conservative or realistic, or correct.

How can you feel so confident that, you know, there isn't--we know enough today to be absolutely certain that this is incorrect, if that's the right language, or there isn't some potential for condensation?

BODVARSSON: No, let me go back. If I mean to sound confident that there isn't condensation, I was incorrect in saying that.

LATANISION: That's the implication I got.

BODVARSSON: Okay, sorry about that. Because it's my belief all along, I'm concerned about the condensate in the cross-drift, and we have to explain it if it is condensate or seepage, and if the condensate is actually due to the construction activities. And, then, I would feel a lot better about shadow zone and the environment around the waste package, and all of those. No, I totally agree with you. I think it's a very important concept. I really do.

LATANISION: Thank you.

CERLING: Dave?

DIODATO: Diodato. I'd be willing to pass for the present, but I'd like maybe 90 seconds at the end of today's session.

CERLING: Oh, sure, yeah. We can come back. Mark?

ABKOWITZ: Abkowitz, Board.

If we could go to Slide 64, please? Bo, you mentioned a number of modeling uncertainties, and my question to you is which of these is of greatest concern to you, and is it a potential show stopper from the TSPA standpoint?

BODVARSSON: That's a big question, too. The biggest concern to me? Personally, with respect to transport uncertainties, this one is of the most concern to me, because if we don't know the spacing of leaps, if you will, flow path down through the mountain, it is hard for us to justify the flow focus and concept that we have and the active fracture model concepts that we have, because we don't have real good data to be on a sound foundation with respect to that. Because, this could conceivably be optimistic rather than pessimistic, because of matrix diffusion, sorption, and other aspects of the rock. So, this is of the most concern to me.

Personally, I think we are conservative with respect to climate, very conservative is my personal view with regard to that.

LATANISION: Going back then to the one that you're most concerned about--

BODVARSSON: But, it's not a show stopper.

LATANISION: Then, why are we continuing to do work in this area? Because, if that's not a show stopper, you've pretty much stated your case; is that correct?

BODVARSSON: Yes.

LATANISION: So, there's no need for there to be any additional work at this point in time on this transport model?

BODVARSSON: Well, it depends on how you look at it. I like to think that all of us in the project are trying to strengthen all of our barriers down the road. And, I'd like to agree with Paul Craig there that it's even more important to strengthen the natural barrier case than the engineered barrier case, because it would be really nice to have the natural system by itself meet those, rather than having to rely on an engineered barrier. So, I totally agree with that.

So, I think, and I agree with what the project tries to do, I think it tries to prioritize where it hopes to get the best bucks for their money, and I've heard Margaret say this is going to continue for more confidence. So, I think this, like any other area, is going to compete for funding. Maybe it will get less, or maybe it will get more than other barriers.

ABKOWITZ: Thank you.

CERLING: Dave Duquette?

DUQUETTE: Duquette, Board.

Bo, I've only heard you speak a couple of times, and I'm always amazed at how you can cram a 15 week graduate course into 75 minutes.

I did have one question, if we could go back to Slide 22, and this is a little bit off what you talked about, but I think it's something that hasn't been, or I don't think it's been considered yet. You brought in the concept of microbial activity reducing the amount of nitrate in the rock, at least. You're going to be introducing a lot of microbes, we already know that, in the drift of various kinds. We've already seen mold and slime and some other things where water has been present.

Has anyone considered the possibility that microbial loading, forgetting about MIC, forgetting about corrosion, but microbial loading will deplete the nitrate? Because if it does, you depend very sensitively on the nitrate for corrosion protection.

BODVARSSON: That's a good question, and I would like to answer that question. I only want answers to be easy ones, because I know it has been considered, and I think Dr. Horn from Livermore has been looking at microbes, and I don't know what he has looked at, the source term for nitrate in the waters at Yucca Mountain?

ANDREWS: Again, we're talking about the effects on chemistry. I think the, and I'd have to verify this, that the physical and chemical environment model, which is the model in representation that evolves the chemistry evolution and the dust/chemistry evolution in the drift, has a component that addressed the microbe's effect on the evolution of that chemistry, but I'd have to verify that.

We did, for the SR, have such a representation using the model that was developed by the Swiss program, and I'm pretty sure that same model is represented in that document, but I'd have to verify that.

DUQUETTE: Well, I just don't know if you've closed the loop with your corrosion people, that's all, and it might be worth doing.

ANDREWS: We'll verify that. Thanks.

CERLING: Priscilla has the last question before the break.

NELSON: He's being really nice to me. Nelson, Board.

Slide Number 34, please. I just have sort of a philosophical question for you, Bo. Look at either the saturation or the water potential plots, and the way the variation goes with depth. For example, just look at saturation. Do you think that this profile of information represents a steady state condition? Or, is it in the process of changing, and, if so, is it from saturated or from a dry side? What's going on in the mountain regarding saturation and how that affects the flow? What does this tell you about steady state?

BODVARSSON: I look upon this as what I would call a quasi steady state. I can't say either steady state or transient. Transient means it's very fluid. It's changing constantly. I think this curve here reflects water flow at some 100 meters around the SD fault, because it's not only controlled by the matrix or the fractures, that reflects infiltration changes over the last 50,000 to 100,000 years, or so, because of the permeabilities of the matrix blocks and of the material at hand.

So, I visualize this, if you will, I visualize leaks of water, or streams of water coming through the mountain that were 20,000 years ago deeper streams of water, because there was more infiltration, and now are smaller streams of water, and I visualize matrix flux next to the streams that are trying to equilibrate in moisture tension and chemical potential with this ever changing stream of water. The changes in the stream of water are on the order of 10,000 years, or so, based on past climate studies and past changes in infiltration.

So, that's how I see this, as a quasi steady state, which we can treat as a steady state pretty much, because it's over thousands and thousands of years.

NELSON: I think that this story that you just said there, the idea of looking at this information, combining it with some of the, like Carbon-14 ages that you've got, to tell the story of stability, for example, or instability, variability within this unsaturated zone is an important story to tell. And I don't think it's been--it's sort of been abstracted before it's been communicated, and I think there's a story to be told by looking at profiles like this and showing how past climate changes may be reflected in distributions, and that there is a gradual change, and it's not sudden.

BODVARSSON: I couldn't agree with you more, and let me tell you why explicitly. I've always been tremendously interested in the chloride, total chloride variability in the matrix blocks, because they're fairly uniform, because, again, they reflect over the last 50,000 to 100,000 years what variability was in the climate, because 20 milligrams per liter of chloride in the matrix block reflects basically 10 millimeters per year of infiltration.

So, using that information that is integrated then over tens of thousands of years is a very good indicator of past climate, which would show much less climate change than what we're assuming in the future, and it could be very useful.

NELSON: So, I think to go for it, because you know-- what was the saturation at the time of deposition?

BODVARSSON: Zero.

NELSON: I think so. We can agree on that point. And, from there, there hangs a tale, and that's a story that can be told, too.

BODVARSSON: I agree.

CERLING: So, we will convene at 4:30, in about ten or twelve minutes.

(Whereupon, a brief break was taken.)

CERLING: We have one more scheduled talk this afternoon, and that's by Bob Andrews, and he will now discuss flow and transport in the saturated zone at Yucca Mountain. And, then, after that, we will have some questions for both Dr. Andrews and Bodvarsson, and then there's some more public comments afterwards.

ANDREWS: Thank you.

We're going to be talking this afternoon about this. And, as a safety topic, those of you who don't live around here, it is drier here even than in Las Vegas. So, don't wait for the headache to hit you. I encourage you to get water. I see the Board has theirs, and I hope the public and other members have theirs. There wasn't any at the break, but maybe it has come back. So, if the Lindas can get some water, that would be, I think, great.

It's an honor I think to be here and be representing the work that I'm going to be representing here for the next hour. And, Thure, please keep me on track and on time.

This represents in 60-something slides, you know, the work of scores, literally scores of researchers, investigators, scientists, USGS, Los Alamos, Sandia principally have been involved, but also Nye County scientists and their consultants, and UNLV scientists have worked on characterization of the saturated zone for the purpose of Yucca Mountain.

There have been other scientists characterizing the saturated zone, both on the regional scale, and more site specific scale, for a lot of other reasons, because the saturated zone is important for the Nevada Test Site. The saturated zone is important for Death Valley National Park, and the saturated zone is important for this. This is where this water came from, and we'll look at the wells in the area, and the bases of the wells in the area, and information and data available from the wells in the area.

These scores of scientists have been studying this thing for decades, not just for Yucca Mountain purposes, but for other purposes. So, I'm a little humbled to be here representing this body of scientific work and analyses and data that have been really the province of the people who collected it, interpreted it, analyzed it, developed the models, did the analyses for it. I am their spokesperson, and in 60 slides, each slide represents not quite careers worth of work, but close to that, for some of these issues and data and interpretations.

As Bo did, we have a poster that tries to capture in one poster, you know, everything that's of general relevance to the saturated zone as it affects the performance of the Yucca Mountain repository.

I organized it ever so slightly differently than Bo. Bo, as you saw, went data, calibration, model, model validation, results. I kind of broke it up because different processes are occurring at different scale and are relevant to different scales. I broke it up a little bit more by scale, with two central larger plots that are the net result of why we talk about the saturated zone. It's not so surprisingly why we talk about the saturated zone as where does the water go, and how much water, how fast is it going, and what are the transport times of any potentially released radionuclides that come into the saturated zone to the point of compliance. Point of compliance is defined in Part 63, and in 197, as a zone about 18 kilometers south of the repository.

This represents the culmination of the flow, and this represents the culmination of the transport, where the culmination of the transport is effectively a mass breakthrough curve, or mass arrival curve, or whatever you want to call it, at that point of compliance.

There's a lot of inputs to it, starting with the regional flow characterization, and we're going to go through essentially most of this stuff in the next 60 slides, starting with the regional flow information, because in the saturated zone, there's some advantages that you are constrained by the boundaries upon which you're basing your representation. And, in this case, we have the whole Death Valley regional flow system that constrains what goes on at the scale of the site. And, so, I want to spend some time on that information and recent interpretations and USGS, principally, work that that represents.

Other people in support of the Nevada Test Site have developed other models which are very similar, but I'm going to focus on the USGS 2002 representation, and the supporting information for that.

We then get into the scale of the site here, and the individual data points and wells and test interpretations associated with the site scale information.

I should back up and say at the regional scale, we also have a large body of geochemical information and interpretations of that geochemical information. Jim Paces and co-workers, and Zell, have done most of that over the last decade or so, and there's some recent interpretations of additional geochemistry, and what that tells about average flow paths and flow distributions in the saturated zone.

From the scale site, we have the detailed wells, and in two particular locations, we have, when you come to the scale of the site, you have to talk about the differences in flow and transport in fractured tuff materials, which are directly beneath the site, and flow and transport in alluvial porous materials which are some distance down gradient from the site.

Right here, where we are, it's virtually all alluvium, several thousands of feet of alluvium before you hit the bedrock, although somebody from Nye County was telling me it's the exact number here, if they had it from geophysics or something.

So, we're going to spend some time talking about the details of two large scale tests, one conducted in the fractured tuffs, and one conducted in the alluvium, for their understanding of flow processes and particularly transport processes.

And, then we come down to radionuclide transport, and we deal with transport.  You have those same two tests, large scale tests, the alluvial testing complex in the alluvium, and the c-wells complex in the fractured tuffs, but you also have, in addition, because you're not putting radionuclides generally in situ in the saturated zone and see how they migrate, you have laboratory test data to support the retardation characteristics of the radionuclides of interest.

So, we're going to spend some time talking a little bit about the data associated with it, but I want to impress upon you it's a snapshot of one data set for one radionuclide, and the LANL scientists for the last twelve years have been collecting radionuclide sorptive characteristics in a range of different tests, in a range of different chemical environments, in a range of different geologic media for a range of different radionuclides, and those data are all in the controlled data sets.

So, that's what we'll do. We'll walk through starting with regional, regional and local being flow. Then go into transport, and the basis for the transport, and ultimately to a characterization of the uncertainty of that transport.

I'm also going to compare the results that the Department currently has with the results of some analyses conducted by another interested party here, and that's the Nuclear Regulatory Commission. So, I think it's useful to compare when available, you know, our models, interpretations, analyses, with those of others.

So, with that as an introduction, let's go onto the next one. I kind of talked about the first organization. This one just resituates you to the saturated zone. It is providing those pathways for potential released radionuclides. I use that term reasonably maximally exposed individual, because that's the term in the requirements from Part 63 and 197.

It's important to point out that last bullet, that whether the disruptive event or it's nominal performance in the absence of a disruptive event, the saturated zone plays a role. It does not play a role for the volcanic ash, and ash redistribution, but once radionuclides that are potentially released, however they are released, with whatever probability they are released from the unsaturated zone, enter the saturated zone, that is the stuff I'm going to talk about for the next now 55 minutes.

So, the first part is just the flow, and it's going to define essentially the flow paths, where the water goes, and how much water is going in where the water goes from beneath Yucca Mountain or from the points where it might be potentially released from the base of Yucca Mountain, and there we're kind of at the unsaturated zone/saturated zone contact, releasing mass to the saturated zone, and where it goes through that 18 kilometer compliance point.

And, the transport defines that advective dispersives, or how fast it's moving, how it reacts with the rock mass, any dissolved radionuclides or colloidal radionuclides, how it diffuses into the rock matrix, and how it is sorbed on the rock matrix, or in the rock matrix.

The performance measure of interest, looking solely at the saturated zone, is that mass arrival, mass breakthrough curve, essentially the time at which mass or activity is released at that 18 kilometer point.

And I think the last bullet I've talked about. There's been scores of people looking for a couple of decades at this thing, and, in fact, more.

Okay, I think I hit this when I touched this, but let's talk about these breakthrough curves a little bit. What's shown there is for a singular point value, a mean input parameter, no uncertainty, realization, first off, a non-retarded species represented by Technician or carbon or Iodine-129, and a slightly sorbed radionuclide in this case represented by neptunium. And, you can see that I've broken the curves for neptunium into three different components. One is sorption on the fractures and in the matrix, has us going through the fractured tuff materials. One where it's only sorption in the alluvium for that travel path length that's in the alluvium. Remember, this is a singular point value and I'm going to talk about uncertainty later. And, the third is the combined effect. So, it's the combined effect that we think is the ultimate performance.

So, for unretarded species, we see in the range of hundreds to thousands of years. You know, the median is there at whatever it is, 600 or so years. And, for retarded species, we see something that's a little bit less than 10,000 to something that's significantly greater than 10,000.

So, you could argue that the saturated zone by itself for things like neptunium and plutonium and other sorbing radionuclides gave all the performance you needed. And, I don't think anybody is proposing putting the waste in the saturated zone, but that's the results, and we'll look at the uncertainty later.

Bo had a nice little pictorial of the analyses models supporting the unsaturated zone characterization and the models and uncertainty, et cetera. I just kind of listed them. A lot of the results that we're going to be showing here are in some published USGS work. I've just captured three of them on here, and a lot of the other results are preliminary. They're in varying stages of check, review and approval conducted by Los Alamos, USGS and Sandia scientists principally.

Bob Roback, who you heard earlier, is a principal contributor on the geochemical constraints on the flow directions, and we'll talk about those results here in a little bit.

Okay, this shows a conceptual representation, radionuclides that are potentially released. I've conceptually just shown this as vertical flow. As Bo showed you, there is some lateral flow components to that. For conceptualization, that's relatively unimportant, but the details are incorporated in the TSPA of where the actual radionuclides come, if they are released from the waste packages and the EBS and from the UZ because of the UZ transport paths.

And, then, essentially lateral flow and migration through both fractured tuffs, shown here schematically, and then the alluvium. It's a little bit not to scale. The depth to water of alluvium at the point of compliance is on the order of 100 feet roughly. The depth to water at the repository 1000 feet from the repository to the water table. So, it's a little bit out of kilter scale-wise there.

But this I think shows that you can contact different rock types as you're going along that flow path, and that distribution between flow in the tuff and flow in the alluvium, there's about two or three KTI agreements on just that question, where is the alluvium/tuff contact, and what's the uncertainty in the flow path with respect to that alluvium/tuff contact.

Okay, now I want to talk about regional groundwater flow system. Most of this stuff, as I say, is in USGS publications, most of them completed last year. I should say that the USGS is continuing this work and developing a transient model as we speak. I think the release date of that transient flow model, regional flow model, is sometime next fiscal year, maybe at the end of next fiscal year. But, it's in development as we speak.

Okay, get out your magnifying glasses now. But, the actual publications are full size pictures. I want to set the stage of where we are, about there, right now. The principal features I'll be talking about through this talk are Fortymile Wash. This little black box is the site scale representation. Various regional scale models and representations have been developed for different purposes, some of them for EM purposes, NTS purposes, and then for Yucca Mountain purposes. So, different regional boundaries are shown on here. They are slightly off by a few kilometers, but that's, you know, unimportant for how we're actually using that information.

Death Valley you see here, Amargosa Valley here, Rainier Mesa, Spring Mountains, et cetera. We're talking about the whole region. You can see the scale here of zero to 80 miles.

One unique feature of this whole hydrogeologic basin is it's an enclosed basin. There's no net water flux out of this basin as water. There is water flux out as evapotranspiration, but not as water. So, it's kind of a unique hydrogeologic basin in that regard.

Okay, the first aspect, once you understand the regional physiography, is understanding something about how the water got in and how the water gets out. So, let's talk about water getting in first.

Bo talked about the USGS infiltration work at Yucca Mountain. This is an extrapolation of that. It's been modified several times over the last decade, but it's an extrapolation of that work at Yucca Mountain over the entire region. So, this is done by Joe Hevesi and co-workers at the USGS. This is the current, at least 2002, published representation of how much and where the net infiltration is occurring. So, again, you see large amounts in the Spring Mountains, you see lower amounts in the Timber Mountains north of the site, Piute Mesa, et cetera.

Let's keep going. These are the actual volumetric recharge rates from those estimates from Hevesi, et al. The recharge estimate is kind of a function over what time period you're looking, and what kind of approximation method you use for estimating net infiltration. So, they looked at two different time periods, and looked at several different methods of quantifying the average net recharge infiltration over the entire basin.

One thing of note is the total volumetric recharge is in the range of between, based on these interpretations, between 100 and 300 million cubic meters per year. So, for as large of an area, reasonably well constrained. You know, 100 to 300, a factor of three is fairly incredible for the different estimation methods.

They have also used, and I think these are published in a paper by Alan Flint and his co-workers some years ago, maybe even referenced in the blue book that Bo passed around, compared these average infiltration rates to infiltration rates in arid regions around the world, the Negev Desert, Arizona, et cetera, and you nominally get these sorts of percentages of percent of total precipitation as net infiltration in those other arid climates, you know, in the range of 1 to 5 per cent. So, again, confirming or supporting the average volumetric influx into the entire region.

Okay, a lot of us don't think in millions of meters cubed per year, so I put a little time out for a little discussion of volumetric flow rates for a couple reasons. One, we are concerned about water use, and water appropriations and water availability. And, two, it's directly written in the regulation, water uses, water demand is directly written in the regulation.

So, that 100 to 300 million meter cubed, I give some conversion factors up here. This is a point of information. The average household in Las Vegas, and those of us who live in Las Vegas all know this because the water district told us this when they told us to ration our water September 1st, so we know exactly what the average water use is in Las Vegas, is about 20,000 gallons a month per household, or about a 1000 meters cubed per year, a little less than an acre foot per year.

But, the other key one is the regulation requirement of when you're doing your calculation of dose, and you're looking at the water demand of that reasonably maximally exposed individual, put it in 3000 acre feet per year. So, 3.7 million meters cubed per year. So, that's the requirement, and it's good to have that in the back of your mind. That's both for individual protection and groundwater protection, same number in EPA and in NRC.

Okay, that was a little time out for volumetric flow. Now I'm talking about discharge. USGS, D'Agnese and co-workers, and a lot of supporting references, they were kind of the assimilator of this information. As I say, I'm kind of a slide per major piece of work, a lot of information behind these, a lot of publications behind these, and I pick the one slide to represent some piece of information that was germane to understanding flow.

So, this happens to capture the major naturally occurring discharges in the Yucca Mountain region. A lot of it's occurring in Death Valley, Ash Meadows, et cetera, Oasis

Valley toward the north.

Okay, what are those volumetric discharge estimates turning out to be? Well, I presented the actual presentation of D'Agnese's in thousands of meters cubed per day, and, so, I didn't want you to have to calculate it yourself. I converted it to millions of meters cubed per year so we have an apples to apples comparison with the recharge estimates.

So, you see the total naturally occurring discharge estimate in the 2002 report by D'Agnese, et al, is 100 million cubic meters per year. Pretty amazing that totally independent estimates of volumetric flow, one being recharge, one being discharge, come up with ostensibly the same number. For the amount of uncertainty that exists in that vast area, it is amazing they're within a factor of three. And, in fact, one of the recharge estimates was 100 million meters cubed per year.

You're saying okay, that's only part of the story. I want to know about the wells that gave me this, and I want to know about the wells that the other people in the whole regional basin are pumping. The most current information is presented in this plot, which I think is from Belcher, et al. I might have the wrong reference, published last year or the year before, I think the year before. And, each pumping center is located by a dot.

I want to draw your attention to the fact that they averaged--or they didn't average, they did the cumulative pumping rate between '87 and '98, so that twelve year time period. You can see some of the major pumping centers here in the Amargosa Valley area. J-12 and J-13 sit right there. So, you can see DOE's water extraction permit, and water extraction over that twelve year time period, and Bo is familiar with Pahrump, having stayed there last night, unfortunately. So, Las Vegas, for those of you not familiar, is just maybe here, just off these regional maps.

PARIZEK: Bob would that circle be ten times in a cluster, 50 times, if you are doing a circle based on withdrawal per use?

ANDREWS: I'd have to look at the actual data source.

PARIZEK: It's huge.

ANDREWS: I'd have to look at the data source. I think they tried to capture individual water uses to the best of their ability, not just what was permitted, but their understanding of use.

Another way of plotting use--well, I should point out that in this area, most of the water is used for irrigation purposes, roughly 75 per cent is used for irrigation purposes. Another 10 per cent is used for mining purposes, and the remainder is generally domestic water use, especially in the Amargosa Valley area, it might be different ratios in Pahrump.

This sub-basin boundary from this particular report is just slightly different than the regional sub-basis boundary I showed you earlier, but just how they characterized it.

Let's go on to the next slide. It shows the temporal evolution over the last 15 years, or so, of water extraction in just the Amargosa Valley area, so just in those cluster of wells representing where we are right now. And, you see the data that we have that are published. There's no current data on irrigation withdrawals for '99 and 2000, so that's why, it wasn't that there were zero, it's just that there is no data available. So, it's on the order of 12,000 to 14,000 acre feet per year pumped from the wells right around where we are.

NELSON: What happened in 1989?

ANDREWS: Ii don't know. I'd have to look at the report. It might have been a really wet year, I don't know. Maybe they didn't need to do as much. Maybe they didn't get all the information reported. I'd have to look, to be honest with you.

Okay, in addition to understanding volumetrically recharge and discharge, where it is and how much it is, water levels have been observed and inferred and interpolated from well logs, from well interpretations, from springs, from surface features, and from geologic interpretation. So, this particular map of potentiometric surface is based on all of the above. It's not just well measurements of how deep is the water, et cetera, but there's some geologic and spring interpolation, or interpretation, excuse me, on this potentiometric surface. But, again, it's not so surprising that it indicates major areas of recharge and major areas of discharge, simply based on potentiometric surface contours.

Okay, the USGS, based on geologic information and inferences from potentiometric information, simply inferred some regional flow directions in the report by D'Agnese, et all. Those inferences, this is a sub-basin of the total basin. Yucca Mountain here, Fortymile Wash. Fortymile Canyon, Fortymile Wash is here, so in the vicinity of Yucca Mountain, the general flow direction inferred from this research is southerly, essentially.

There's some inference from recharge in the Specter Range to the southwest of carbonate flow potentially discharging to the Death Valley region, and to Ash Meadows.

Okay, in addition to potentiometric information and recharge, discharge, general relationships, there's a wealth of interpretations, well, first data, and then interpretations of those data associated with basic geochemistry in the wells in and around the whole Death Valley region, and in particular around Yucca Mountain. These data have been collected over the last decade, and interpreted by USGS scientists and LANL scientists.

Now, the geochemistry interpretation of regional flow systems, there's some good news and bad news. The good news is the geochemistry integrates and averages over a relatively wide volume and time. So, that point measurement that you make of geochemistry kind of represents an average over space and time that is hard to capture otherwise.

The bad news is that an individual point value can be locally affected by local heterogeneity, local discontinuities in flow, local discontinuities in geology, that affect locally the geochemistry that might be observed. So, it's not a single straightforward, ah ha, the chloride always looks like this, the sulfate always looks like this, the Carbon-14 always looks like this. There's a very detailed interpretation that almost has to go on well by well, or cluster of wells by cluster of wells.

This represents a figure in the Amar. that I mentioned to you earlier that Ed Kwickless and Bob Roback from LANL are in the process of putting together. So, it is definitely preliminary, even though I didn't put preliminary down here. Even this label down here probably should not say NRC. It probably should say NWTRB. But, don't worry about that.

This indicates the general flow directions looking at clustered similar types of water, and where types of water were dissimilar, where there was probably mixing of different water types. So, they characterize in this case seven flow--no, I'm sorry--nine flow paths, potential flow paths. Yucca Mountain is right--I was a little bit confused. Potentially, it's here where all these data points are. Again, the interpretation is generally the flows are southerly. This Path 9 is kind of an underflow interpretation in the deeper carbonate aquifer system. Significant uncertainty with that because of very limited data on the carbonate geochemistry, especially in this particular region.

The next slide shows the chloride part. What these researchers have done is they have looked at sulfate, chloride. Other researchers from the USGS, Zell and his co-workers, have looked at strontium and strontium isotopes. Sulfate has been looked at. Uranium-234, 238 ratios and bulk uranium concentrations, I think I have those on the next slide. But, these generally indicate an increasing chloride concentration along the travel path. And, you might quibble with the exact numbers, and can you detail the interpretation, the details of the flow path based on a point measurement, and the answer is no, but you can get general inferences from the flow paths from these average geochemical observations.

Just a point of clarification. These mixing zones, Mix A, Mix B, and somewhere I have a Mix C, points where it appears from the geochemistry that different water chemistries are effectively being combined in a point of mixing. So, the flow paths, if you will, are sort of converging on the Amargosa Valley discharge area, potential discharge area. These are not showing discharge. This is groundwater flow systems, not discharging yet. Ash Meadows is just off this map down to the south.

NELSON: Bob, can I just ask for a clarification?

ANDREWS: Yes.

NELSON: As I recall, when we discussed this data, there was really no control on depth, particularly for the samples, but with the constraint that all of these data, except for the blue, Path 9, were above the paleozoic limestone.

ANDREWS: That's correct. So, they are in different geologic units, but they're generally open hole. So, what exact unit and what exact depth that water may have been coming from, in some cases, we know because we have done fluid logging and other types of surveys, but in many cases, we don't know exactly the depth and exactly the lithologic unit from which they're coming.

Okay, this is a similar plot. This is in a 2002 paper by Jim Paces and a number of USGS co-workers looking at bulk uranium, and then uranium 234, 238 ratios in the saturated zone. Bo talked a little bit earlier today about U-234, U-238 ratios in the UZ, and uranium in the UZ and interpretations of that in the unsaturated zone. Here, in the saturated zone, basically the perched waters have a very high U-234, U-238 ratio. It's up around seven or eight. These are not the UZ waters. These are saturated zone waters. So, you see these high activity ratios here, and the activity ratios generally decrease as you go southerly, as you would expect them to as they mix with other uranium bearing waters.

This is a more detailed interpretation that Jim and co-workers have of the J-12, J-13, U-234, U-238 ratios, but I kind of pointed to their paper for that interpretation.

So, we put all this stuff together, I should say the USGS put all this information together, understanding of hydraulic properties, understanding of recharge, discharge, location, potentiometric surface, and like any good hydrogeologist, they developed a model because they wanted to understand where the water is going, and how much water is going where it's going.

This is the 2002 regional USGS model showing essentially recharge here, flow towards the south, and ultimate discharge at those discharge locations that I identified earlier. Shown also, this is the simulated potentiometric surface, and the residual heads shown with the little symbols as they fit between the observed and simulated. And, you can see most of the larger head residuals are in an area of low gradient, especially in the area where we are right now. In the area to the north and the area near the compliance point, the matches are fairly reasonable, within, you know, a few meters, or tens of meters.

When you get into steeper hydraulic gradient areas, or areas of very sparse data, you know, such as over here, you know, the errors can be, or the residuals, simulated versus observed, can be 100 meters. That does not demonstrably affect the average flow paths.

Here's the comparison of the model simulated discharge against the observed discharge. This is shown in graphical form. Each one of those major areas were the total discharge that we showed in the previous slide as a table. And, again, there's significant uncertainty with some of these individual estimates of discharge, but the regional model does an extremely good job of capturing the expected naturally occurring discharge.

So, having done the regional, and the regional is what I'm using and what the project is using, I'm not doing any of this, as I said earlier, you know, it's LANL and USGS and Sandia, what they've used as the boundary conditions for now coming into the scale of the site. And the scale of the site now is several tens of kilometers north and south, and several tens of kilometers east and west. So, it's still a fairly large area of doing groundwater flow and transport representations.

This site scale has much greater detail of geology. There is a geologic representation at the regional scale. I'm sure you've seen the regional geologic maps of NTS and the surrounding areas used as a starting point for that regional geologic characterization. Claudia Faunt and her co-workers within the USGS have refined the geologic representation of the whole regional area, and all modelers have been using that geologic interpretation.

And, the site scale model, just so I have the scale issue and what details I'm trying to characterize, and I'm also trying to at the site scale build on the details of the hydraulic heads, permeability, geochemistry, that have been observed in those wells, and in particular, in some of the larger scale tests that have been conducted in the saturated zones. And, those tests are generally in the C-wells complex, in the tuff, and in the alluvial testing complex, and the alluvium.

This is the current representation of the boreholes used to characterize the hydraulic properties and flow properties and potentiometric surface for the saturated zone. Some of the more recent Nye County wells are shown on here, and although they have just finished completing drilling Phase 4, I don't think the heads have been observed in all of those recently completed wells, although there's maybe somebody here who can elucidate how far they are on the testing of those wells now that they have completed the Phase 4 drilling of the Nye County wells.

One point here is the previous wells, I'll call them, prior to about four or five years ago when I think the Nye County-DOE cooperation really kicked into high gear, were all much closer to the site. I mean, we were characterizing the saturated zone in the vicinity of the site. Four or five years ago, we did not have a final EPA regulation. We did not have a final NRC regulation. We, nor they, knew where the compliance point was going to be. Where are we going to determine the safety of this, the post-closure safety of this facility.

As you are aware, there's a lot of discussions maybe it would be at 5 kilometers. Who knows where it would be. But, when the final rule came out, it was here essentially. If I could see the fence, I'd know exactly where it was. But, the 18 kilometer boundary from Yucca Mountain is essentially along that line.

As a result of that, and trying to characterize both the geology and the hydrology and the transport characteristics now of an area much further south, DOE, in cooperation with Nye County, instituted, you know, a series of drilling, testing campaign, principally starting along

Highway 95, and then going further north and east and west along that boundary with the objective of characterizing the alluvium, both hydrologically and geologically and geometrically, where is the alluvium and where is that contact between the alluvium and the tuff.

This is a geologic scale of the site. This is a geologic representation of the surface of the site, a large amount of alluvium, Highway 95 is not on here, but essentially, it goes through here along the Highway 95 fault. We are essentially down here a little bit.

This is a geologic representation of the site. Geology goes to a depth of 3 kilometers. A lot of this is based on geophysics in addition to boreholes to characterize the distribution of geologic layers.

One of the purposes of the Nye County wells, as I said, was to characterize where is the alluvium. These two cross-sections are straight off of a Nye County web page, and show the interpretation. Here's Highway 95, here's Fortymile Wash, so we're doing one cross-section AA prime up Fortymile Wash, and the other BB prime going east--well, northwest, southeast into Fortymile Wash. And, I hoped that it printed off a little better so that I can read on the screen, but there are several hundreds of feet of alluvium at that point before they get into tuff, rock units.

I encourage you, for those of you on the Board, when you log onto nyecounty.com, a lot of information there on the wells, the completion, the heads, the testing, and things like this, lithologic logs and, as appropriate, cross-sections, so all of their data essentially are posted on that web page. It's very nicely laid out, easy to click through, a little advertisement. Okay.

This is an interpretation both from geophysics and from those boreholes by Rick Spengler and his co-workers at the USGS of the alluvium thickness south of Yucca Mountain. So, here you see the ESF, and here is the map of the total alluvium thickness. The saturated alluvium thickness is a little less than this. There's a map of this that we presented in the Technical Basis document, but I think this gives you a sense of the massive alluvium thickness that exists both east and south of Yucca Mountain itself.

Okay, this is I think the 2001 USGS potentiometric surface. Now it's a scale of the site. So, I've come in from that regional potentiometric surface and am presenting the individual head values here. This is from Pat Tucci and his co-workers of the Survey, and the inferred contours of that potentiometric surface. This particular potentiometric surface is assumed that the observed heads at the two wells a few kilometers north of Yucca Mountain represent locally perched conditions, and are not characteristic of the regional or local flow system itself.

So, in addition to having site specific information and site specific geology and site specific hydraulic estimates, we essentially take as a starting point the regional model fluxes. I mean, that's why it was so important to begin with to get that regional model and get it reasonably constrained and reasonably validated, because those boundary fluxes are used as a starting point for understanding what are the fluxes at the scale of the site. And, fluxes are ultimately going to drive not only flow paths, but they're also going to drive flow rates, and flow rates are going to drive velocities. So, getting the fluxes reasonably constrained from the regional model is an important deal.

These values represent the inputs, you know, taking it straight off of the regional model. These exact values in fact represent the earlier version of the regional model, In the analysis model report, we're presenting both the earlier version and the most recent version, and the site scale model is, as it's been calibrated, what those fluxes moved up or down to be. So, those are targets, if you will, and those are the net results. So, it's not that dissimilar really.

Okay, now we come into the details associated with our understanding of the flow characteristics and transport characteristics of the tuff and of the alluvium. And, what I've shown here is kind of a blow-up of the C-wells test complex, and a blow-up of the alluvial testing complex done at NC EWDP 19. EWDP stands for Early Warning Drilling Program, and NC stands for Nye County.

In both cases, longer duration tests have been conducted in the alluvial testing complex. As was alluded to earlier, we have not been able to conduct the larger scale tracer tests, larger scale pump tests, but there have been some single well injection and withdrawal tests that I will talk about, because they do help us confirm some of the average fluxes that we've determined from the models.

Okay, this is C-wells, the three wells, two logs, one being the matrix porosity on the left, and the other one being fractures per meter on the right. And of most significance, why there's significance on this is it finds the geology, which C-well it was tested in. But of importance for us are where, from fluid logging, the flow actually comes from. Those of you who have wells, maybe many of you have wells, should realize that not all the water comes from one particular--from the hole test interval, it comes from a zone, and that's not dissimilar in these fractured rocks either. It's coming from discrete zones when they're pumped, do a fluid log, and determine what fraction of the water comes from what zone.

It's this spacing between these zones, so if that's where the water is going, and we have other tests, fluid logging type tests, to confirm in other areas of the scale of the site the spacing of these what we've called flowing intervals. You can see this is units of meters. The spacing is in the order of tens of meters. If that's where the water is going, that's where the potential radionuclides would also go. So, it's based on the order of tens of meters. There's actually uncertainty in that, but you can see from this distribution that's kind of an average value.

Okay, one of the big advantages of the C-wells, there was a little longer than a year pump test conducted from C-wells from May of '96 to November '97. It pumped out almost a half a million meters cubed. The result of that was to engender some draw-downs in a number of neighboring wells, and some of those wells, as you can see, are kilometers away, up to 5 kilometers away from a one and a half year test. These draw-downs in the observation wells around the C-wells have been interpreted to determine average aquifer characteristics, and the effects of faults on those average aquifer characteristics. So, it's these data, and other single hole test data, that have been used to assist in constraining the hydraulic characteristics of the site scale flow representation.

And, the other thing they have been used for is to get a global, an estimate of anisotropy and the uncertainty associated with that anisotropy. We had a couple, or at least one KTI agreement that talks about the anisotropy and the interpretation of anisotropy and the uncertainty in propagation of that anisotropy through the saturated zone that we address in the appendix of this Technical Basis document.

I didn't tell you at the beginning, I'm essentially walking through what's in the Technical Basis document, not everything, but a large fraction.

Okay, at the site scale, we also have a model based on those boundary conditions, both vertical and lateral boundary conditions, based on the hydraulic properties that we have observed, and the potentiometric surface that I pointed to earlier. And, this is the fit or the representation of that potentiometric surface as embodied in that site scale saturated zone flow model.

Again, I have a lot more data in the vicinity of Yucca Mountain itself, and where I have a lot of data, or where the gradients are low, the matches are very good, you know, within a few meters. As I move away, or get into areas of steeper gradient, which might represent areas of larger heterogeneity or, you know, places where we haven't characterized the spatial variability of the geology adequately in the model, the residual heads can be larger. But, our interest, again, is the flow system essentially from here to here.

This is the match, or one of the matches of the observed versus predicted permeabilities from the saturated zone site scale model. Most of them are reasonable, with the exception of this one in the tram, which was from the C-wells area, and that difference, that large difference has been attributed to the fault that was intersected just near the c-wells test itself. So, it's an enormously high permeability estimate, if you will, from its average characteristics over the model domain. And, that is accommodated with the anisotropy, essentially. So, the anisotropy is accommodating the characteristics of small and medium sized faults.

Okay, the net result of this for a singular realization is this. It's the flow paths from Yucca Mountain trending in a generally southeasterly direction until you get to Fortymile Wash, and then underneath Fortymile Wash, it's more or less paralleling the axis of Fortymile Wash. Fortymile Wash is shown here.

We're going to show, not to whet your appetite, but in about 30 slides, we're going to compare this to NRC's results. So, let's keep going.

This is a comparison of the geochemistry with those flow paths. So, this is the same essentially seven Tuff related flow paths that derive from geochemistry, superimposed on a few of the flow paths from the model. So, it's a fairly good correlation here between the two.

There is uncertainty. That uncertainty is principally a reflection of the anisotropy and uncertainty in the average anisotropy over the scale of the site, and also shown on here is, although it's been reduced, the uncertainty, current uncertainty associated with where exactly the alluvium tuff contact is. It's clearly a function of depth, a function of location, and, so, there is still remaining uncertainty in where that alluvium tuff contact is, and there's uncertainty associated with the flow direction. It is possible to get a more southerly flow direction for certain anisotropies and possible to get a much more easterly flow direction for other anisotropies.

And, now I really encourage you to look at the NRC slides, because they have essentially the same picture.

NELSON: Could you explain the 0.05 up through 20?

ANDREWS: Yeah, I think we're looking at--which way is--green is .05, so it's 20 times more transmissive or permeable in the east-west direction than in the north-south direction. So, we're looking at ratios east, west, north, south, and 20 would be 20 times more in the north-south direction than it is in the east-west direction.

The data from C-wells, which are the principal means of constraining the anisotropy ratio, indicate it's somewhere in the generally 1 to 5, perhaps 1 to 10 range. But, there's some low possibility that it's less than one.

So, we ran a case with it less than one. So, this is trying to capture the overall range.

NELSON: Nelson, Board.

Do you have any evidence about this from anyplace else other than in the C-well complex.

ANDREWS: I'd have to check with you, to be honest with you. I think they used other data sources besides C-wells, but C-wells was the principal constraint for the middle point of that anisotropy ratio.

Okay, just as a point of information, with that uncertainty, the flow path length to that point of compliance within the alluvium ranges from about 1 to 10 kilometers, just to give you a frame of reference.

Okay, I'm going to switch to transport. There's a number of processes going on within transport, different processes going on within a fractured tuff and going on within an alluvium. Both of them have sorption capabilities, but in the fractured tuff, there's matrix diffusion and the effective porosities, i.e. where the water really goes is much smaller than what it is in the porous media in the alluvium. So, these are the processes we're going to describe.

So, this transport model is going to give us those velocities, not just fluxes, but now I'm in velocities, how fast any dissolved constituent or global constituent might move within the saturated zone to the point where they were drawn by that hypothetical individual, the reasonably maximally exposed individual.  And, although this last bullet I think is of importance, one could say a transport model generally is calculating concentrations, and generally a transport model is modeling concentrations.

However, because of the way the regulation was written by EPA and NRC, they said go to the point of maximum concentration.  Find the point of maximum concentration and put that hypothetical individual, that reasonably maximally exposed individual, at that point in 2-D aerial space, and then give the water demand of 3000 acre feet per years.  So, put it where the maximum concentration is.

So, because we had that requirement of put it where the maximum concentration is, there's no need to directly calculate the concentration.  You just calculate mass, and put that mass, which comes out in curies per year, or activity, I should say, not mass, in curies per year, and put it in the 3000 acre feet per year, or 3.7 million cubic meters per year.

So, you end up with a concentration, you know, at that point.  You might say is that conservative or is that optimistic.  In one case, it's conservative because you capture the entire mass.  Whatever that mass is going across that boundary, you've gotten all of it, not some partial set, not some of it went around and missed the well. You've gotten it all. So, in that case, the regulation is conservative.

On the other case, you might say, well, it could have been some small fingers, you know, and those fingers only hold whatever they hold, you know, 1000 acre feet per year, or 100 acre feet per year, in which case the concentration locally could be different. It could be higher. But, the requirement is concentration in 3000 acre feet per year.

So, I won't talk about concentrations in the saturated zone. I'm going to talk about fluxes and activity fluxes, and velocities that relate to those activity fluxes.

Okay, C-wells has been a phenomenal source of transport data in the saturated zone, in situ in the tuffs. And similarly to what Bo is illustrating with respect to Busted Butte and what he had indicated I think it was in Alcove 7, was this understanding of general conceptual model in terms of the role of matrix diffusion on transport by looking at different tracers, and in this case, we have PFBA, we have bromide tracers, and different colloid representations. In this case, we have microspheres that are included in this test, that funny blue. And, some constituents that have been shown in the lab to have sorptive capacity, i.e. in lab tests, both batch tests and cone tests and other sorts of tests, lithium has been shown to be a sorbing constituent on Yucca Mountain tuff type materials.

So, the data from C-wells illustrated here, on the one particular test, and there were multiple tests, multiple intervals, multiple cross-hole studies, all of which are summarized in some USGS work, and work from LANL scientists, indicate that we do see the effects of matrix diffusion because different ionic radices diffuse at different rates, and we see that difference, and we see the effect of retardation.

Now, these cross-hole tracer tests at C-wells on the order of tens of meters, I think these two wells I think are 80 meters apart, something like that, I'm looking at the scale of 80 meters even though the test was conducted over, and the hydrology was affected over a much larger area. So, the matrix diffusion model more or less confirmed, and Bo showed results in the UZ that confirmed, and the sorptive capacity of lithium in this case was confirmed, which is a very positive finding that in situ and in the lab, we get similar amount of sorptive capacities.

In fact, the lab sorptions are lower than what actually resulted in situ in the field for lithium, and what we've used going forward is always the lab values. A, we have a ton more lab data. But, B, the lab results, at least for lithium, show that that's reasonable, and a little bit on the conservative side. Paul left. They use the word conservative, Paul.

Okay, here's the matrix diffusion tests. These are generally lab data. We compared it from a lot of different sources. It shows the uncertainty that we've incorporated in the matrix diffusion in our model. They are different for different sized radionuclides, but they show similar trends.

This is more or less what I was alluding to earlier on the lithium. This is lithium transport in the lab, and this is lithium transport in situ in the field at C-wells. That difference between the lab and field, and you can see the numbers here, the field is ranging from .6 to 4 milliliters per gram, and the lab from .1 to .3. The difference might be in sample preparation. I hope it's not the same difference as Chlorine-36. I would say it's sample preparation and interpretation and lab test versus field test, not LANL versus somewhere else.

Okay, a similar, not cross-hole tracer tests, but single hole tracer tests because we couldn't do cross-hole tracer tests in the alluvial testing complex, have been done in the alluvium. It's been very interesting. These results are presented in that AMR I was talking about earlier. These are the tests, interpretation of the tests from M.J. Mowry from the Survey, and what we have done here is you inject a tracer, but you don't really--you try not to over inject it.

So, you inject the tracer and it sits for some period of time, let the natural system, it's called the natural gradient dilution test by many, some people call it a pumpback test, depending on what kind of over-pressure was applied, let the natural gradient do its thing, and take that tracer away, and then pump it back in, and then interpret it.

It's not a singular interpretation, so a range of interpretations have been made, but from those interpretations, a range of fluxes have been inferred because of uncertainty in the effective porosity of between 1 and 9 meters per year. Now, this is flux, not velocity. So, I'm sure we're clear there. For a range of effective porosities between 5 per cent and 30 per cent.

Of note, this is in the alluvium at 19D, I believe, at the alluvial testing complex. The site scale model, the site scale model that we talked about earlier, gives a flux for the central tendency case of 2.3 meters per years. So, I don't want to oversell it, but it's confirming that the range of observed fluxes and the range of predicted fluxes are close to, for all practical purposes, they're the same with the uncertainty we have in the test and the uncertainty we have in the model itself.

Okay, in addition to the site specific or test specific information on hydrology and transport characteristics, it would be nice to confirm over a little larger scale, and something that integrates over a larger spatial scale, something that tells you something about transport times. We've tried to do that in the unsaturated zone, with things like Chlorine-36 and Carbon-14 and tritium, and we're trying to do that in the saturated zone, principally with Carbon-14. There's other isotopes that have been looked at to try to understand something about the average travel times, if you will, or advective transport times between two points, but the Carbon-14 information in the saturated zone is a way, not the only way, but a way of integrating over a larger space and over a larger time.

And, I should say the project is not the only ones who are interested in advective transport times in the saturated tuff aquifers of Southern Nevada. The EM program is also concerned, and NTS is also concerned with understanding average advective travel times, or velocities, particularly up in Oasis Valley, and going from Piute Mesa to Oasis Valley, and also west of Yucca Mountain. So, other investigators, in addition to the LANL and Survey investigators, have been looking at Carbon-14 and trying to interpret it to the best of their ability on what the average transport times may be.

And, for the range in the vicinity of Yucca Mountain and going south along that flow path that we've already talked about, the average groundwater velocity estimates, and there's large uncertainty associated with this, presented in the draft AMR are in the range of 5 to 40 meters per year. Now, this is velocities, not fluxes. So, flux divided by porosity is velocity.

If you looked at that over a range of 18 kilometers, you would say average advective transport times of a non-retarded species are in the order of, you know, you can do the math yourselves, but several hundred to several thousand years if the velocity range is 5 to 40 meters per years.

These velocity estimates are not dissimilar from the velocity estimate that Oasis Valley, between Piute Mesa and Oasis Valley, or the other velocity estimates from some researchers that I forgot the name of, we referenced them in the Technical Basis document, but I forgot, just west of Yucca Mountain.

The next slide captures some of the open symbols are UZ, these are data that I think Zell, or they might have been taken from Zell and replotted, I'm not sure where the original source was, whether it was in the AMR or one of Zell's reports, where we combined UZ data for Carbon-14 and Delta Carbon-13, because the Delta Carbon-13 is going more positive, or less negative, if you will, going to the right and might be an indicator of carbonate waters and mixing with carbonate minerals or carbonate mineral dissolution.

But, we generally see that the unsaturated zone waters which generally are on the ages of thousands of years, and the per cent carbon decreasing, so becoming older as you go into the saturated zone waters directly beneath Yucca Mountain, and onto the south.

You have some anomalous values here in Timber Mountain, which is further north, which are described in the analysis model report.

Okay, having done flow and velocity, now it's time to bring in the retardation characteristics. We talked about retardation of lithium. Well, lithium isn't a big problem for the repository, but radionuclides are. So, what I've illustrated here are the neptunium sorption data from lab tests at LANL, all combined on one plot as a function of experiment duration.

There's a little nomenclature here, new versus old, this is pre-QA and post-QA, and also pre-1990 and post-1990. But, it shows that, as Bo said, you know, neptunium Kd in the range of between .1 and 10. Most of them are around 1. For the vitrified tuff, there's additional data on zeolitic tuff, et cetera.

This is more recent data from LANL incorporated in that the analysis model report. There are, as I think I said earlier, there's three KTI agreements associated with alluvium sorption, and the project with the rest of those KTI agreements, did a lot of laboratory testing, principally at LANL on neptunium, uranium, plutonium, iodine and technetium. Plutonium I think was in colloidal form, not plutonium in dissolved form.

For the iodine and technetium, the lab data indicated zero sorption, essentially, so an Rd of one or a Kd of zero. There were some who believed, and I think even some peer reviewers of ours believed that there was potential sorption of iodine in alluvial materials. The laboratory data did not support that assertion that some peer reviewers had, so we are not taking any credit for sorption of iodine on the alluvial materials.

This shows for different wells, these are all Nye County wells, so, you know, for well indicator, put NC-EWDP, and it shows the well location, different depths, and for different size grain materials in there. So, the different sample preparation led to different sorption characteristics. Not surprisingly, the finer grains, which include higher clay mineral contents proportionately and the iron oxides gave higher sorption of neptunium, and there's a wealth of literature out there, our project, other projects, of sorption of neptunium on iron oxides and clay minerals as mineral species, not as rock, combined rock species. So, these data, and the uncertainty in these data are represented in the Kd distributions for neptunium.

All of these things ultimately lead into what we're after, which is not only where the water goes and how much water is going where it's going, but how fast is it going, and how fast are the radionuclides going that are in that water. So, I talked about this earlier. Show here is the case of a non-sorbing species for the mean of the flow characteristics, if you will, just looking at a breakthrough curve of, in this case, technetium.

And, as we talked about earlier, from Carbon-14, Carbon-14 with the velocities of 5 to 40 meters per year, was giving an average, if you converted that to 18 kilometers, giving several hundred to several thousand years of transport time for non-retarded species. So, that's where we are from the several hundred to several thousand years. There is a long, a little bit longer tail in there, but 70, 80 per cent of the mass is coming in that several hundred to several thousand years.

For sorbing radionuclide, in this case neptunium, you see it's out close to 10,000 years. Plutonium is even further to the right. I haven't shown it on here.

I think I'm now going to talk about the uncertainty. So, there's uncertainty in these, and although I haven't listed them parameter by parameter, as Bo did, but essentially there's uncertainty in matrix diffusion, uncertainty in flow interval spacing, uncertainty in flux, uncertainty in Kd, in this case, it's zero Kd, because it's a non-sorbing radionuclide, uncertainty in dispersion, which I didn't talk much about, didn't talk at all about, but there's uncertainty there, uncertainty in flow path lengths in the alluvium versus in the tuff, uncertainty in the effective porosity, both in the fractured matrix and in the porous matrix. So, I get a distribution. So, this is the uncertainty, if you will, in technetium arrival times at that 18 kilometer fence line. We're going to compare this to somebody else's work in a little bit.

This is putting all the uncertainty in neptunium. These top curves represent I think it's--I'm not sure if it's 300 realizations or 100 realizations, to be honest with you--but, it represents the individual breakthrough curves incorporated in that uncertainty, and then this is just looking at the PDF associated with those. And, again, you can see there is some possibility of it occurring before 10,000 years, but well over 50 per cent of the mass arrival in neptunium is after 10,000 years.

Here's plutonium. So, it's further to the right. So, plutonium is an even higher, more sorbing radionuclide under the geochemical and geologic conditions we have at Yucca Mountain.

This is colloids. This is just the singular realization. We have the horse tail plots similar to the other ones, but this shows the singular realization of colloids. And, there is some very small fraction that can occur early, but the bulk of it is out at several thousands of years, and this is based on, although I haven't talked about it in here, the colloid transport parameters derive in part from C-wells and in part from laboratory testing, the column and everything done at Los Alamos.

Okay, we're not the only ones looking at the saturated zone. NRC has had the Center, Jim Winterle and his co-workers look at the saturated zone for their understanding of the saturated zone model, saturated zone flow path, and saturated zone transport. They are, of course in any footnote, you will see these publications that I've cited here, say that they are doing these analyses so they can interpret DOE's analyses, so they understand the uncertainty associated with DOE's analyses, and their analyses are not to be construed as a regulatory basis. It's up to DOE to present the regulatory basis, the scientific basis for their understanding of the saturated zone, not NRC. But, I think these are illustrative.

First, is there flow paths? Okay, let's go back. This is one representation of NRC's flow path distribution from their model of repository down to the 18 kilometer point. Although I have not presented it here, they have other representations that have an easterly, more easterly flow component before it goes southerly. There have been two more recent publications than that in 2002 that this represents. One was presented at the High-Level Waste Conference in May, and one was a Center internal paper that Jim just finished this spring. So, there are other flow paths.

Let's go on to the next one and show you for non-sorbing radionuclide, two different representations of that breakthrough time distribution. He's presented it in log years here. So, $10^{2.75}$ to $10^{4.75}$ or $10^{2.6}$ to $10^{3.4}$, but if you would have applied those at a distribution on either a log or linear axis, you would have seen that these are not dissimilar from the hundreds to thousands of years that we also projected for a non-retarding radionuclide.

NELSON: Nelson, Board.

Those times are all from--they're not from time zero. They're from time when the radionuclides hit--

ANDREWS: Excellent point. All of the times that I've been showing, and that just represented the NRC plots, are from the time the radionuclide hits the saturated zone, whenever that might be, to the time it's at that 18 kilometer compliance point. Excellent point. It's the time in the saturated zone, not the total time from package failure to EBS transport, et cetera.

Okay, so, in summary, I've tried to do in an hour and 60 slides what, you know, represents a large body of work. And if I have screwed up, you know, it's my fault, but we can point you to the original source, where the original conclusions are made, and the original data are presented. But, we developed both regional and site-scale fractures and flow models. Those flow models generally indicate a southerly flow direction from Yucca Mountain to the point of compliance.

The flow fluxes along that flow path are generally in the 1 to 2 meters per year. It increases slightly as you go southerly as the water starts combining, if you will, or the flow paths converge. Permeabilities are also higher to the south when you enter the alluvium.

These fluxes are reasonably constrained by the Carbon-14 estimates, and they're also reasonably constrained by the single point value at the ATC, which gave that 1 to 9 meters per year flux range.

There is still uncertainty associated with where that flux or that path enters the alluvium, and the travel path length in the alluvium. For the SR, and I might get my numbers a little bit wrong, but I believe the distribution was between 0 and 6 kilometers. It's now between 1 and 10 kilometers based on the new interpretations of where the alluvium is, based on the most recent Nye County wells and the interpretations of those, and geophysics from the Survey.

On the transport side, we have the effective flow porosities and flow interval spacings, both in the fractured tuff and effective porosities in the alluvium. All those things combine to give average transport times in the hundreds to thousands of years for non-retarded species. Those are at least consistent with the Carbon-14 interpretations, both here and the Oasis Valley area.

The tests have been done to confirm the processes that we have, matrix diffusion processes, the retardation processes, and based on a large amount, and I can't do it justice in one slide, clearly, a large amount of laboratory data on sorptive characteristics, we have sorption of the key radionuclides.

Okay. So, if I was talking about this as a barrier or as a feature important to performance, for some radionuclides, like iodine and technetium and carbon, the saturated zone by itself is buying in the hundreds to thousands of years of delay. Barriers defined in Part 63 talks about delay of radionuclides.

For a lot of other radionuclides, like neptunium, it's essentially buying thousands to tens of thousands of years of delay. And for many other radionuclides, and I haven't gone into the details of the individual radionuclides of interest, but take plutonium, especially dissolved plutonium, it's out past the regulatory time of interest.

So, the saturated zone, if you will, by itself buys more than 10,000 years of transport time, which means it would be a barrier in and of itself for those radionuclides.

I think we've adequately captured--I haven't talked about the details of the uncertainty of each parameter, but that uncertainty is characterized and represented by those breakthrough curves, and the uncertainty in those breakthrough curves. And, as a final note, I think I already talked about the USGS work, regional work continuing. The Nye County drilling, I think they have, as I said, completed Phase 4, pumping and testing and analysis of those Phase 4 wells is ongoing.

The Department and LANL and USGS scientists are out there taking specimens every few weeks as they pump those wells. The future plans associated with the Nye County testing program, whether additional transport tests, I think probably we would be best asking Nye County tomorrow morning. But, that work continues.

So, with that, I will stop and entertain any questions.

CERLING: Questions from the Board? Priscilla?

NELSON: Bob, thanks. It's I think 6 o'clock, although I'm not sure.

ANDREWS: You were supposed to be keeping time.

NELSON: You did a great job. I want to ask you a question, though.

I recall some thinking about a couple of things regarding I guess the site-scale model that had to do with-- you can't hear me? Okay. At the site-scale model having a high hydraulic head, which I'm not sure that I yet understand what is going on there. The fact that in Slides 32 and 35, I think it confirms that we still have not very good control due south of the site on what's going on with the saturated zone. And, remember Linda Lehman talking about her temperature measurements, and the plots of flow that she was showing based on those, which showed a more southerly flow.

All these things can be important because they may have something to do with how long the water stays in the tuff versus the alluvium, and I have a feeling that there's a difference in performance of tuff versus alluvium in terms of time, delay, whatever. So, is it significant that if the flow were more southerly and it stayed in the tuff, that there could be a significant difference? And, what is that high hydraulic head now? What is that?

ANDREWS: Okay, let's start with Linda's thermal stuff. We did thermal analyses, too, in the saturated zone to determine whether the thermal data can assist in constraining the saturated zone flow representation. It worked very well in the unsaturated zone. I think it's given Bo a lot of extra confidence of being able to interpret match, if you will, of the thermal profiles.

In the saturated zone, it's not as clear. We have some results that we present. We show them in one of these appendices. But, to use those thermal data to assist in constraining the flow system is not very easy because of uncertainties associated with thermal properties and thermal fluxes and elevation variation that ties into the amount of radiated heat loss through the UZ.

So, although we presented it, I didn't present it here because it doesn't help constraining really that site-scale flow model.

The alluvium tuff contact is still of significance. We, as I've shown here, we do have a lot more information, a lot more weld points and control on those weld points and additional geophysics, that have been interpreted to better constrain, to the best of our ability, where that contact is. But, is the uncertainty zero? No.

NELSON: Well, the contact where the path is, where that exit point is, in terms of going south--

ANDREWS: Yes, both issues are there, the geometry part and the path part. So, the path part, as I said, can be sometimes a little bit further west. That's one realization there, those red lines. So, it is possible to have a more southerly flow component within the uncertainty of where the flow goes.

NELSON:  And, it's faster.
ANDREWS:  Yes, velocities would be faster.
NELSON:  If they stay in the tuff.
ANDREWS:  Yes.
NELSON:  And, so, breakthrough is faster?
ANDREWS:  Yes.
NELSON:  Okay.
ANDREWS:  But, that uncertainty is represented in those curves.
NELSON:  So, it can be important.
ANDREWS:  Yes.  I mean, for me to directly answer your question, I probably should look at the output files of the distribution and say, okay, which ones of these are the one kilometer, two kilometer, you know, travel path lengths in the alluvium, and which ones of these are the eight kilometer, nine kilometer, ten kilometer travel path lengths, just to see, you know, how much is it alluvium transport uncertainty that's driving this distribution, and how much is it, you know, matrix diffusion, effective porosity, flow and interval spacing, et cetera.
NELSON:  I think that would help for me, and also plans to determine better.
ANDREWS:  Yes, I think that would be a useful way of--
NELSON:  And, can you just give me a final parting insight into the high hydraulic head?

ANDREWS: No. It's still very--interpreted, you know, as is it perched and represents a local condition, or does it represent a regional--you know, I think we had the peer review, looked at that some, what, three, four years ago. They said it could be either interpretation. We agreed that it could be either interpretation, and they concluded that either interpretation doesn't make any difference, you know, whether it's a confining layer or a local perched layer that's causing that discontinuity, if you will. It doesn't make any difference to the flow paths, flow directions, and flow rates from the repository down gradient.

So, additional testing could be proposed.

CERLING: Richard?

PARIZEK: Parizek, Board.

Again, Bob, compliments for the organization and the same sort of remarks apply to Bo's presentation. We see where you're going with all of this. I have a number of points, and so maybe you'll have to cut me off before I get to all of them. It's late in the day I know.

But, with regard to this particular graph 35, you have a contour line, a 775 meter line. That's the first time I think I've seen such a hook. It's on the--do you see that 775 finger that comes down? If you look at that and then compare the chemistry in terms of the flow paths implied from the chemistry, you wouldn't necessarily have some of those chemical pathways come out right. Or, you would get a strong easterly direction of the flow. Is there something inconsistent about that, with such a long finger to the south, and I wanted to know what the basis is for that. There's some wells down in that southern Crater Flat areas, and is that the true water table, or is there something weird going on down there?

ANDREWS: I'd have to verify it, and maybe somebody knows exactly where 7-S is. Nye County EWDP 7-S has an anomalously high head. It's like the first 20 meters above sea level, and I believe it might be represented in there.

PARIZEK: Yes, let's assume it isn't high, it's a question of whether that's an upflow portion of a regional flow system where you may really have an upward gradient. So, then we have to go back to nested piezometer data, that's Nye County's area of expertise, to sort of see if that's the explanation for it. And, then you say okay, well, I shouldn't really put that in. I want the true water table, not the elevated head because of an upflow component.

ANDREWS: Excellent point. If this is 7-S, and somebody can say it's not 7-S, the individual point when they were drilling and testing showed a significant decrease as you went down the borehole, when they kept the whole hole open, which the whole hole is open right now, I don't think it was cased off at any depth, the average head is driven by the most permeable unit in that section, which I think is the top of the section, which is darned close to land surface.

So, I think although there is some uncertainty associated with the interpretation of 7-S, I believe it's been interpreted as local recharge, and you're essentially looking at a local, maybe not perched, it may be, you know, actually confined by a low permeability layer.

PARIZEK: Another way to do that is to have a fault controlled high permeability with water rushing down the high permeability finger, and creating this ridge.

ANDREWS: It could be Solitario Canyon.

PARIZEK: I mean, it's kind of important, and I think really you don't have an answer for it right now, but just to draw attention to that inconsistency there.

ANDREWS: Yes, I don't think we have an answer for it.

PARIZEK: On Page 39, I guess it was the red flow path that must give us that one kilometer, because I've never seen a one kilometer alluvial compliance distance of travel. That's scary. It's almost like you came straight through the volcanics and came out straight south and never did go east.

ANDREWS: I'm pretty sure--

PARIZEK: It must be that red one then.

ANDREWS: It's somewhere in here. So, the red would give you that.

PARIZEK: Yeah. So, that's again pretty close to where there's been bedrock outcrops. So, I guess some more work could be done, maybe drilling, I suppose, or maybe is it done, all the drilling is going to be done in that immediate area.

I'll go back to another point on the anisotropic permeability contrast that the C-well testing complex gave us. That's I guess one of the reasons why the southeasterly pattern of flow comes in.

ANDREWS: Yes.

PARIZEK: Because it seems like that's sort of special. All the other faults, many of the other faults have a north-south kind of orientation, so you'd kind of think the contrast might be different.

ANDREWS: That southeast trending fault right there.

PARIZEK: Yes. If we had a C-well testing complex somewhere south of the footprint, maybe we would get another orientation.

ANDREWS: Yeah.

PARIZEK: So, I would hope that somewhere along the line, the program will have a C-well testing complex in some other fault complex there. I guess Nye County is opposing that, or talked about that, and I think we have heard the need for that somewhere along the line. I hope that doesn't die in the future.

ANDREWS: S&T is here to listen to your comment.

PARIZEK: There's a Page 3, you had yellow for zero recharge. I guess this is the recharge input. That's not 3. It's the recharge, it's the boundary condition figure that also you had the flux.

ANDREWS: Oh, for the local site-scale model?

PARIZEK: For the local site-scale model. There was yellow, which was zero recharge, and then there was a lot of white. So, what's white if yellow is zero?

ANDREWS: It's zero.

PARIZEK: It's zero, too? Okay. I just wanted to make sure that's clear. That would be coming way down on Fortymile Wash. It's a white zero. But, the yellow way down is also zero? It can't be zero.

ANDREWS: No, what they're trying to do is capture the potential for recharge along Fortymile Wash, potentially. So, it's like on the order of a half millimeter per years, I think. I'd have to verify the exact number that they use.

PARIZEK: And, then there's some other observations about the spreading of the plume. The plume is narrow, and it got narrow compared to TSPA in '98 when it was too wide and too dilute. I mean, everybody took flack because of that one. But, there are mechanisms to cause spreading, assuming that there is climate change or recharge changes causing some spatial spreading. And, I guess that's the transient part of the model. It may come in some day in the future. You said the USGS is now doing the transient model. They may deliver results sometime in the near future. But, then there's an opportunity to build transients in for calibrating steady state models, another way to calibrate them. Right?

ANDREWS: Well, they're doing transient flow. I don't think there's any plans of doing transient transport within that regional--

PARIZEK: Well, you're not going to say regional, but if you had that information, you could do that in a site-scale again. I'm just thinking the observation, that if we go into the region, say, and look at just alluvial fan development in the last 10,000 years, we get like four to five to seven periods of fan development. We have four to five lake level stands in the Mojave River Basin showing, hey, 10,000 years has got a hell of lot variability in the climate through that time period, and all of that is capable of causing a recharge variation in a place like this, assuming this is similar, in which case then it could be plume spreading. So, you really could have something wider and more dilute than the pencil thin lines that the results now show.

ANDREWS: Yeah, that's an excellent point. We'd want to probably talk to NRC before we implemented such a representation, because as the rule is written, Part 63 is written, it says go to that point of maximum concentration. It kind of presupposes that that point of maximum concentration is not wildly varying all over the map.

PARIZEK: Yeah, but the geomorphic evidence in the region, you know, depending on water shed to this water shed, shows this complication over the last 10,000 years, and could occur again in the next 10,000 years. So, maybe the NRC needs to be aware of the fact that this could be a moving target and bouncing around, you know, if the future climate states are like that, too.

ANDREWS: Well, we tried to simplify that, obviously, by saying we're going to go to that point and take the mass out, even if it's moving in space.

Now, as you're probably aware, EPA, in developing the rule, had some discussion of this issue, and they went on to say the discussion of where is the farm, and let's move the farm around in time. So, I think both EPA and NRC understood that simplifying that compliance evaluation to constrain it to that well, and that 3000 acre feet per years, removed a lot of regulatory, if you will, uncertainty that was unknown.

PARIZEK: I guess that brings up the question of why the 5 kilometer fence idea might have constrained where all of the regional hydrological data was coming from, and now the need to go to Nye County and add stuff in a hurry seems like even back in those days, you'd need to know what was going on in and around you more regionally than just 5 kilometer compliance boundary to get that story correct.

ANDREWS: Yeah, you'd need to be able to constrain what goes on at a little larger scale to understand what's happening at a smaller scale.

PARIZEK: I mean, that's just a catching up requirement. I've taken too much time.

CERLING: Dave Diodato, do you have a residual question?

DIODATO: I have many, many questions. I guess we have the saturated zone question we could focus on, and then also is there an opportunity for a wrap up, if Bob does a wrap up?

CERLING: No, this is the end. But, you can still ask a question.

DIODATO: Well, all right. Diodato, Staff.

On the saturated zone, it seems like the 500 year median breakthrough for technetium as a conservative species, or however you want to describe it, compare that with the unsaturated zone, which has like a 6000 year number attached to it, and it kind of makes the unsaturated zone almost look like--it makes the saturated zone look like the 98 pound weakling, and the unsaturated zone look like Arnold, or something, you know. I mean, so, what I wonder is if there's processes that are included in the unsaturated zone, like matrix diffusion, that may not be included in that saturated zone calculation. Is there any matrix diffusion in that 500 year number, or not?

ANDREWS: Yeah.

DIODATO: With matrix diffusion through the volcanics?

ANDREWS: I think part of the issue is, and the distinction between unsaturated zone transport and saturated zone transport, and I don't think you'll see 6000 years, but you'll see a distribution around that with some early arrivals and some late arrivals.

The distinction is where the water is flowing. In the saturated zone, when I hit that saturated zone, I'm in these flowing intervals which are nominally the most transmissive units, spaced on the order of tens of meters apart, with distribution. There's uncertainty in the distribution on that. Were they spaced at meters or centimeters apart, the effect of matrix diffusion would be significantly greater. Significantly greater.

We did a sensitivity analysis. I'm not sure we presented it to the Board, but it's in the analysis and model report, that looks at a range of different flow interval spacings, and the impact of that. And, when you're in that tens of meters of flow interval spacing, the amount of matrix diffusion, the amount, it's the same coefficient, but the amount of matrix diffusion you get is in the order of, you know, centimeters, or tens of centimeters around those flow interval spacings, those flowing intervals.

In the unsaturated zone, and, Bo, you're going to have to correct me if I'm wrong, the spacing between flowing fractures in the unsaturated zone is much smaller. So, the effect of matrix diffusion could be larger. Diffusion coefficients are the same. I mean, you're at 80, 90 per cent saturation, and so your net matrix diffusion coefficient is similar in magnitude because of the spacing. The net effect of them on breakthrough curves and breakthrough times could be greater.

Now, you've got to also understand that the flux rate through the UZ is much smaller than the flux rate through the saturated zone. The effective porosities I believe for where the radionuclides are going, the effective porosities are similar, you know, $10^{-3}$, $10^{-4}$ range. I didn't present those in here.

Now, where's Bo?

DIODATO: I don't know where Bo is. I thought we saw him slip out. But, the numbers have changed, because when Bo spoke with us before in this very spot, January of 2001, he had a 1000 year number for the median value for the breakthrough conservative species. So, we see these numbers changing, and we wonder now, are the transport models changing, or there's more information that's coming along that helps to either build your case or to increase confidence in your predictions. So, is it always going to increase upwards, or at times can it decrease downward? But, I'll kind of leave that out there. Have the transport models changed since then?

ANDREWS: The unsaturated zone transport model has changed a little bit. The flux distribution has, at the repository horizon, has changed a little bit. The infiltration flux has stayed virtually unchanged. But, the flux distribution at the repository horizon and how that's distributed between fractures and matrix within the active fracture model has changed a little bit between the time of SR and the time of LA.

I don't look at things as getting worse or getting better, but additional information and characterization and uncertainty representation are being portrayed in the license application models and analyses, and you're seeing some preliminary results of those in these meetings.

DIODATO: Okay. I just had one other comment not related to this, but to the MacKinnon talk earlier today. A lot of his arguments were hinged, you know, based strongly on the multi-scale thermal hydrology, those calculations, and the credibility of that model rests on the assumption that you can represent non-linear dynamics in a complex, spatially varying system with these static lookup tables.

You know, some of the problems we've seen with that in terms of the output also suggests that maybe there's some problems in the implementation of the boundary conditions and assumptions of that in the sub-models, and the one test that we've seen that was published in the White literature didn't have--had some problems in terms of predicting saturations, under predicted saturations, which is a critical parameter in the unsaturated zone.

So, we still look to see some evidence, and the project is aware, you know, of some of these problems I think with the multi-scale models. So, we still look to see some evidence for support of that, or some revisions to that approach to calculating thermal hydrology hopefully in the future.

ANDREWS: That's a good suggestion. I think we weren't talking about thermal hydrology in here, but we'd be happy to--

DIODATO: No, I just wanted to go on the record. There wasn't a time earlier when it was presented.

CERLING: Mark Abkowitz has the last question.

ABKOWITZ: Okay. It's my lucky day. Abkowitz, Board.

In lieu of the technical program summary and discussion, which I guess we're not going to hear, I wanted to kind of make a wrap up observation, and then also make a statement, and I'd like to ask if you and Margaret and the other people who are sort of the executive body would agree with the statement.

I teach a course at Vanderbilt called Risk and

Liability Case Studies, and we offer the course so we can look at a variety of different events that have happened over time to try to sort of work backwards and understand what went wrong, and we looked at ship wrecks and building collapses and chemical spills and space disasters, and a variety of other things, and it's uncanny how some of the common features that come out of that retrospective look focus on the same issues that just keep surfacing over and over again. And, among the ones that are prevalent are issues such as tight schedules, cost control or financial greed, poor design, inattentiveness to troubling signs, lack of effective communication, and arrogance.

And, I'm not implying directly that what I've seen so far is highly correlated with those things, but I think it's important to recognize that we have a long history of dealing with contentious technological problems, and we can certainly learn from them. And, the take-away message that I've gotten from that process is really very simply, which is take the time to do it right.

And, so, my question to you in the performance assessment area, and to the program in general, is is this program committed to taking the time to do it right if it means that license application needs to be delayed past December of 2004?

ANDREWS: I would say yes, but I'll let John and

Margaret answer.

ARTHUR: I agree. I think a similar analysis was done at one time on WIPP. There's another one in there called human error and quality assurance once you finally get all the calculations to make sure things are built in a quality fashion. But, the commitment we've made is there is a schedule in every project, and called a baseline, be it the design and the rest, and we're not going to sacrifice quality to get there. Our current plan is December of '04, and we made commitments to our regulator and others that it will be done with quality. And we're going to watch it.

One of the things I didn't say this morning when you show some of the measures in other key areas we're trying to identify hopefully the successes as well as the issues, and we're going to make sure everything is ready to go and satisfies the criteria before it's submitted. But, you have to have a schedule.

I ask people, well, do you want December of '05, December of '06, or what should it be, and I have not seen issues to date that say we can't make it, but we're going to watch it closely, and about March of next year, I think we'll have better indicators to say is that December of '04 achievable or not. And, to date, I haven't seen anything. I mean, I'm talking to our best specialists and others, but it's going to go in when it's quality.

CERLING: I think that closes this part of the session. And, now, there's a little bit of time for public comments, and I'll turn the meeting over to Mike Corradini.

CORRADINI: Thank you, Thure.

We have two people that are signed up for public comment. The first one is Ms. Sally Devlin.

DEVLIN: Mrs.

CORRADINI: Mrs. Sorry.

DEVLIN: Again, thank you all for coming, and for Nye County and the Commissioners, I hope to see you all tomorrow when you're going to feed us. And that will be even more fun.

But, the most important thing is you know my new middle name, and that is Performance Criteria, and I just love Mr. Arthur, III, because this is what we need.

And I want to thank every presenter, particular those that talked about my bugs and my colloids, and I am so proud of all the new ones you keep finding daily, and I have a new report from Dr. Bond at Livermore, and my bugs are just eating their little hearts out up at Yucca Mountain, and so are the colloids, and I thank NRC for sending me that report.

So, we'll see you all tomorrow, and thank you again for coming. You're all a pleasure as always.

CORRADINI: Thank you. Our second comment is by Grant Hudlow.

HUDLOW: Hi. Again, I want to thank you for putting up with us, coming out to rude, crude Nevada. The Federal Register pointed out that even though Yucca Mountain is going to kill 20,000 people in the Amargosa Valley, it's okay because those people have strange habits.

I wanted to mention there's a new opportunity for you to handle your linchpin component and the cask. We have 20 years of work on the DU sir net. It protects the tanks in Iraq. You can hit it with a missile. It won't penetrate or bother anybody inside of it. It also stops the Gamma, and if you a little--in the inside, it will stop the neutrons, and you can handle the thing. So, that saves you and your Yucca Mountain operation about a billion dollars a year in the remote handling. You don't have to do it.

And, the really nice part about this is that we have 500,000 tons of DU, most of it is sitting around in a field in Paducah, Kentucky as hexafluoride leaking into the water, and those people have a ton of money to get rid of it. So, the cask is free if you play it right, and I just wanted you to know that.

CORRADINI: Thank you.

This wraps up our twelve hour day, at least for some of us. I'd like to thank our speakers from the DOE. It was quite good. I think everything in terms of a discussion and comments were very helpful and productive.

We begin again tomorrow at 7:15, to remind the Board. We'll be here to meet members of the general public, and then start our set of presentations and discussions at 8 o'clock.

Thank you to the staff of the Board, and all the support today. It worked out quite well.

See you tomorrow.

(Whereupon, the meeting was adjourned.)