UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SUMMER 2000 BOARD MEETING

Piñon Plaza Resort
2171 Highway 50 East
Carson City, Nevada 89701

August 1, 2000

Scientific and Technical Issues and
Total System Performance Assessment

NWTRB BOARD MEMBERS PRESENT

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

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Dr. William Melson, Smithsonian Institute
Dr. John Kessler, EPRI

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P R O C E E D I N G S

8:30 a.m.

COHON: My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board. It's my pleasure to welcome you to this summer meeting of our Board.

We meet as a full Board three or four times a year, usually in Nevada, and most often in Las Vegas or in one of the communities in Nye County, the county in which Yucca Mountain is located.

Today, however, we are very pleased to be here in the State Capitol, and I believe it's the first time the Board has met in Carson City, and we're very happy to be here.

It's my pleasure to extend a special welcome on behalf of the entire Board to all those from the State of Nevada who either live here or travelled to be here for the meeting, and to everyone else who's with us today.

As you may know, Congress enacted the Nuclear Waste Policy Act in 1982. The Act, among other things, created the office of Civilian Radioactive Waste Management, or OCRWM, within the U.S. Department of Energy, and charged it, in part, with development repositories for the final disposal of the nation's spent nuclear fuel and high-level radioactive wastes from reprocessing. Five years later, in 1987, Congress amended that law to focus OCRWM's activities on the characterization of a single candidate site for final disposal, Yucca Mountain, on the western edge of the Nevada Test Site, about 100 miles north of Las Vegas.

In those same amendments in 1987, Congress created the Nuclear Waste Technical Review Board, this Board, as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. The Board is required to periodically furnish its findings, as well as its conclusions and recommendations, to Congress and to the Secretary of DOE. We do this in Congressional testimony and through our reports.

Now, this is a complicated project, as you know, and in order to cover the many aspects of the project, the Board created five panels, each focused on a set of issues. The panels, which are comprised of four to six Board members, meet from time to time in public settings like this.

As specified by the 1987 Act, the President of the United States appoints our Board members from a list of nominees submitted by the National Academy of Sciences. The Act requires the Board to be a highly multi-disciplinary group with areas of expertise covering all aspects of nuclear waste management. I want to introduce to you now the members of the Board, and in doing so, let me remind you that we are all members who serve on the Board in a part-time capacity. In my case, I am president of Carnegie-Mellon University in Pittsburgh, my day job, as it were. My technical expertise is in environmental and water resource systems analysis.

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

Daniel Bullen is an associate professor of Mechanical Engineering at Iowa State University, where he also coordinates the nuclear engineering program. Dan's areas of expertise include nuclear waste management, performance assessment modeling, and materials science. Dan chairs two of our panels, the Panel On Performance Assessment and the Panel on the Repository.

Norman Christensen is Dean of the Nicholas School of Environment at Duke University. His areas of expertise include biology and ecology.

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

Debra Knopman is director of the Center for Innovation and the Environment at the Progressive Policy Institute in Washington, D.C. She's a former Deputy Assistant Secretary in the Department of Interior, and before that, she was a scientist at the U.S. Geological Survey. Her area of expertise is groundwater hydrology, and she chairs the Board's Panel on Site Characterization.

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

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

Donald Runnells is professor emeritus in the Department of Geological Sciences at the University of Colorado at Boulder. He's also now vice-president at Shepherd Miller, Inc. His expertise is in geochemistry.

Alberto Sagüés, who's still working his way to Carson City from Florida, just one of many airline excitements that the Board experienced yesterday, should be joining us in about an hour and a half. Alberto is Distinguished Professor of materials engineering in the

Department of Civil Engineering at the University of South Florida in Tampa. He's an expert in materials engineering and corrosion, with particular emphasis on concrete and its behavior under extreme conditions.

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

That's our Board. I'm delighted that almost all of us could be here, and we'll look forward to Alberto joining us momentarily.

Many of you know and have worked with our staff, who are seated at the side of the room, impressively displayed there along the wall. Bill Barnard--Bill, will you raise your hand-- is the executive director of the Board, and unlike the members who are part-time, the staff serve in a full-time capacity, and I must add on behalf of the Board, they are terrific.

We have asked several individuals to assist the Board in its discussions at this particular meeting. William Melson--Bill, if you'd raise your hand--has been the Board consultant on volcanism since the Board's inception in 1989.

Bill has been a senior scientist in the Division of Petrology and Volcanology at the Smithsonian Institution in Washington since 1963. His principal areas of research include the dynamics and petrology of explosive volcanic eruptions and the impact of these eruptions on climate and on the composition of oceans and atmospheres. He has extensive field and consulting experience in issues related to volcanic activity. We're very pleased he could be with us again.

Rod Ewing is a professor in the Department of Nuclear Engineering and Radiological Sciences at the University of Michigan. He is also responsible for the university's program in nuclear waste management, and he holds appointments there in the Departments of Geological Sciences and Materials Sciences and Engineering. Rod is a mineralogist by training and has conducted extensive research on the effects of radiation on complex minerals, and the application of natural analogs to the long-term durability of radioactive waste forms. Rod has served on several National Academy of Sciences Panels on the Waste Isolation Pilot Plant in New Mexico, and on nuclear facilities in Washington and Idaho.

Of particular interest for us and for this meeting is Rod's experience as a member of a DOE-commissioned panel that reviewed the total system performance assessment of the proposed Yucca Mountain repository, which was conducted for the viability assessment.

John Kessler also has extensive experience in TSPA. John is project manager for the Spent Fuel and High-Level Waste Disposal Programs at the Electric Power Research Institute. John has managed several iterations of EPRI's performance assessment of the proposed Yucca Mountain repository. His background and education are in nuclear engineering, materials science, and hydrogeology. We're very pleased that they could be with us today.

I'd also like to acknowledge the presence of Ivan Itkin, who I will introduce more formally later on. Also, I see George Dials in the audience. George, raise your hand. Where did you go? You can't hide, George. And also Russ Dyer we're very pleased could be with us. Thanks for coming.

Let me turn now to the significance of this meeting for the Board. The DOE is preparing a recommendation on whether to proceed with the development of Yucca Mountain as the site of a radioactive waste repository. This is the culmination of many years of work for the DOE.

The first iteration of this recommendation, which will be called the Site Recommendation Consideration Report, or SRCR, is due for release at the end of this calendar year. The report will address many issues that bear on DOE's most important decision. Of particular importance is the TSPA, the technical core of the evaluation of whether this site is suitable for further development.

In the past, this Board has expressed much interest, and some concern, in the manner in which the DOE in general, and the TSPA, in particular, address uncertainty. Uncertainty is a critical consideration in any projection of performance of anything over thousands of years. Most, but not all, of the discussion over the next two days will be about the proposed TSPA for the site recommendation, which is identified by DOE by the acronym TSPA-SR, for site recommendation.

Let me summarize very briefly the agenda for the next two days. First, we are taking advantage of being here in Carson City, and we'll be hearing a few remarks from Senator Lawrence Jacobsen, who I'll also introduce more formally in a moment, who's Chairman of the Committee on High-Level Radioactive Waste of the Nevada State Legislature. He will be followed by two overview presentations, one from Ivan Itkin, who's Director of OCRWM, who will update us on OCRWM's program and the Yucca Mountain project in general. Bob Loux, Executive Director of the State of Nevada's Agency for Nuclear Projects, will then comment on the Yucca Mountain project from the perspective of his agency.

Following Bob's presentation, we'll move on to our first technical session, which is devoted to scientific and technical developments. Board member Richard Parizek will chair this session. Aaron Barkatt and Jeff Gorman will brief us on some ongoing research on Alloy 22, which has been funded by the State of Nevada. Mark Peters will tell us about the results of recent scientific studies carried out by the Yucca Mountain project. Frank Perry and Kevin Coppersmith will provide us with a recap on volcanism and volcanic hazard estimates at Yucca Mountain.

We have asked for this recap because our understanding is that TSPA-SR predicts that volcanism will be the only contributor to dose during the first 10,000 years of repository operation. Just before lunch, we will have a public comment period, and I'll say more about public comment later on. After lunch, we will hear from Bill Glassley about an independent effort at Lawrence Livermore National Laboratory to model coupled processes.

The rest of the day and most of tomorrow will be devoted to TSPA-SR. Board member Dan Bullen will chair today's session. We will start off with a tag team presentation by Abe Van Luik and Bob Andrews describing the structure, results and overall uncertainty associated with the TSPA-SR. We will then begin a series of presentations on the individual components that make up the TSPA-SR. The series will continue into tomorrow's session. Today, we will hear from Bo Bodvarsson on unsaturated zone flow and transport, from Ernie Hardin on the engineered barrier system environment, and from Pasu Pasupathi on the waste package and drip shield.

Tomorrow morning, Board member Paul Craig will chair the session on the TSPA-SR. And in a continuation of presentations on TSPA components, we will hear from Christine Stockman on the waste form, from Bruce Robinson on saturated zone flow and transport, from John Schmitt on the biosphere, and from Cathy Gaither on disruptive events. We will round out tomorrow morning's session with a presentation by Abe Van Luik on a recent DOE effort to better describe the uncertainties associated with TSPA. And as I noted earlier, the Board has placed a high priority on this issue.

Following lunch tomorrow, Board member Norm Christensen will chair a session that will include a panel discussion on TSPA. Dennis Richardson will provide us with an update of the repository safety strategy, and Abe Van Luik will summarize the presentations on the TSPA-SR. Then we'll have our second opportunity for public comment at this Board meeting.

Now let me say a few things about the opportunities we've provided for public comment and interaction during the meetings. This is something that's extremely important to the Board. We try to give the public as many opportunities as possible to participate in our meetings. For today's and tomorrow's public comment periods, those wanting to comment should sign the Public Comment Register at the check-in table where Linda Hiatt and Linda Country are sitting. They'll be glad to help you in signing up and being prepared to comment publicly when the time arises. Let me point out, and I'll remind you again later, that depending on the number of people signing up, we may have to set a time limit on individual remarks.

As an additional opportunity for questions and continuing something we've tried out successfully before, you can submit written questions to either Linda Hiatt or Linda Country during the meeting. We'll make every effort to ask these questions. That is, the chair of the meeting at the time we ask the question during the meeting itself rather than waiting for the public comment period. We'll do that, however, only if time allows. We have a very tight agenda, as is probably obvious from my recounting of the agenda. It may very well be that time will not allow us to do this. If that's the case, that is, if we don't have the time to ask the question during the meeting itself, we will ask those questions during the public comment period.

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

Finally, I need to offer our usual disclaimer so that everybody is clear on the conduct of our meeting, what you're hearing, and its significance. Our meetings are spontaneous by design. Those of you of course who are especially perceptive have noticed that I've been reading from this script. Otherwise, though, this is not a scripted meeting. It's completely spontaneous.

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

And by the way, we will follow the usual pecking order when it comes to questioning. That is, Board members, then consultants, and other people who have joined us here at the table, then staff. On occasion, we will also entertain questions from the audience if there is sufficient time.

Before I introduce the first speaker, I would like, on behalf of the Board, to say a few words about last month's untimely death of Nick Stellavato of the Nye County Nuclear Waste Repository Office. To many of us, Nick was the heart and soul of the Nye County technical program. He was largely responsible for bringing into being, and leading, the ongoing Nye County Early Warning Drilling Project, which is providing invaluable assistance in shaping our views on flow and transport in the saturated zone.

His many interactions with the Board at meetings and on field trips, he personified sound and responsible science. His dedication to and love of his work were always evident. He made the occasionally painstaking work interesting and enjoyable, and through it all, demonstrated a good sense of humor and great kindness. In short, Nick was a wonderful person, and he will be deeply missed.

Please join me in a moment of silence for Nick Stellavato.

(Pause for moment of silence.)

COHON: Thank you.

Now it's time to begin our program. Our first speaker will be Senator Lawrence E. Jacobsen. As I mentioned earlier, Senator Jacobsen is chair of the Committee on High-Level Radioactive Waste of the Nevada State Legislature. I'd like to point out that among his many distinguished accomplishments, which are too numerous to mention, we are particularly impressed that he's been an active member of the Douglas County Engine Company for 52 years. That's civic mindedness.

It's always been a pleasure to meet with Senator Jacobsen, who has generously travelled to our meetings around the state, and we welcome the opportunity to visit him in Carson City on his own turf.

Senator Jacobsen, welcome.

JACOBSEN: Thank you for that introduction.

Good morning each and every one of you. What a joy to see all of you here today. I'm impressed. It's the first time you've been in Carson City, and everybody thinks that Nevada consists of Las Vegas. There's a lot more to Nevada than Las Vegas, and part of it is right here at the Capitol. I'm sorry that we didn't have you in the legislative building, because I think we're missing an opportunity to kind of show off a little. But let me say what a joy it is to be here this morning.

I live about 15 miles down the road. I'm a native Nevadan, born and raised where I live. I see one of my colleagues in the audience, Bob Price. Bob, stand up. He's a former chairman, and led us a long way. You can't imagine what it is to chair a committee like this when the state is not too kindly in favor, and trying to keep the committee together and be productive is somewhat of a chore.

But I want to indicate to each and every one of you that our welcome is sincere. We're pleased to have you in Carson City, and anything that I can do, we're ready, willing and able.

As I look over the audience, I'm sorry to say I know very few. Of course, I know Bob Loux, we deal with him on a constant basis, usually budget-wise, and I serve on the money committees. I'm the senior member of the Nevada legislature, and that only means one thing. You're getting too old to survive. But it's been a joy, and with 38 years of service, I've really enjoyed it.

I want to indicate to each and every one of you that our committee is very active, thanks to Bob Price as a former committee chairman, and in Nevada, we change every year, change chairmanships and change politics, too, try to keep it non-political, and with the Republican Convention starting, I think this is a good indication of the interest that there is not only in something that's near and dear to our hearts, but also politically, that it's part of the process.

I indicated to you that I was a committee. We've been everywhere and seen almost everything, thanks to DOE and NCSL out of Denver, Sharon Runyon (phonetic), Linda Sekeema (phonetic). We even went on the aircraft carrier, George Washington, if you can believe that, off the coast of

Norfolk, to see what a nuclear powered machine, how it runs and what makes it go. I'm an old Navy man, and so that was kind of like going home for me, and I invite more trips like that. But we've been to Hanford. We've been to Three-Mile Island, Love Canal, I don't know how many power plants we've been to, and I can't begin to tell you how many times we've been x-rayed, and you can see I'm really not green or yellow, I'm a little bit on the tan side. So it's really nothing to be afraid of.

Let me indicate to you that we look at your board, and I'm going to use Nevada terms, as kind of the "ace in the hole" for us, to make sure that studies are proper and productive and scientifically sound. It's very difficult for us, and admittedly, I probably shouldn't tell you this, I've been at Yucca Mountain 15 times, and some of my colleagues have never been there. I think we're honored that our new governor, Governor Gwynn, is one of the first governors to visit the site. And I think, Ladies and Gentlemen, that's one of the answers today, is for each and every one that has some kind of an interest or something that's burning in your craw, to go and take a look, and then draw your own conclusions. I think that's what it's all about today.

Nevada is kind of a unique place, mostly because of our public lands. 87 per cent of Nevada belongs to all of you, and so we're really not directly in charge. I think one of the features that I come from, and mostly that's in First Response, I'm an EMT and been with the Fire Department for 55 years, ambulance driver and all those kind of things, and in the rural areas, we survive by guess and by golly. I think the 137 volunteer fire departments, my main concern is to make sure that whatever happens, whether it happens or not, I think transportation is probably the second issue, to make sure that our responders are properly trained.

We have a fire marshal's office and we have a hard time maintaining that, mostly because the large communities like Las Vegas and Washoe have their in-house training centers. In my county of Douglas, we have 14 fire departments, 11 of them are volunteer, and that means about 30 members, Tom, Dick, Harrys and Marys, and we survive very well, and let me indicate to you, and I probably shouldn't say this, but we lit the fire for you this morning, and it's burning all over Nevada. We don't like that. But mostly in times like that, we're not in charge. The good Lord sends the lightening and we become the survivors, you might say.

I'd indicate to you that legislative-wise, and I started to tell you this, I was a little bit upset when one of my colleagues told me one day in Las Vegas when we were attending one of your meetings down there, that she said, "Jake, I've never been to Yucca Mountain." I said, "Why not? You live right here." She said, "Nobody ever invited me."

So we put out an invitation just to the legislators. Guess how many takers we had? One. One taker that wanted to go. The reason for that is is the political sideline comes in there, and having to run for office, I found that Yucca Mountain is a real detriment. It's safer not even to talk about it, in fact. But as I said earlier, I think it behooves everyone to take a look.

I think we have a responsibility, Ladies and Gentlemen, not only to ourselves and the generations that come, but especially to the military. Every ship at sea today in the Navy is nuclear powered. And I couldn't get over the USS George Washington, 5,000 people on there, and just coming in from Boznia, this was about a couple months ago, they had just completed their 1 millionth mile at sea on a semi load of uranium pellets. And this room would probably hold about five semi loads, which is 20 tons.

I don't know how many of you have seen uranium pellets, but it looks like dog food. And I asked the sailors on the ship, I said, "Are you afraid?" This thing is nuclear powered, and I stood next to the reactor, and I didn't feel anything or see anything. "They said, "Heck no, Senator, we're not afraid. But if this sucker stops, we want to get off." Of course I don't know whether that's safe or not. I think I'd rather stay aboard as long as we're still afloat, because I've had the experience of spending 12 hours in the water at Bougainville when our cruiser was sunk.

I would certainly hope and pray that you have a successful conference, and I think it behooves each and every one of us to keep our ears open, learn what we can, ask the questions that we have, and when you go back home, don't just talk to yourself. If you're like me, I guess as soon as I walk in the door, the Mrs. wants to know what happened, good, bad and indifferent, and occasionally we don't agree. I guess that's normal, but I think that's the process today, to learn to agree and disagree, and not just stick your head in the sand and say go away. That's not the issue, Ladies and Gentlemen. It's not going to go away. It's a universal problem and it's up to you and me and our colleagues, because it's in everybody's interest, everybody's, whether you're here as a native or whether you're here as a tourist, or whether you just like the good old USA, we've got a problem and it's up to us to solve it.

Ladies and Gentlemen, just let me say welcome to Carson City. Anything that I can do for you, I'm ready, willing and able. I'm pretty good at mouth to mouth. I'm a little choosy, though. I noticed one other person, and I saw her earlier, there's a representative from the Lieutenant Governor's office here. Are you still here? Oh, yeah, she is. Stand up so they can see you.

We have wonderful looking ladies in Nevada, and especially in the legislative building. And years ago it kind of tickled me. I was Speaker in the House for 16 years, and I was amazed that a lot of times the staff people got in on the conversation. They wanted to say something and make a decision, and oh what a tough time we had. So we finally had, and in those days, we had very few women, but now that the women have come on, they're more vocal than the men are, and so they insisted, and so we finally decided, well, we'd kind of separate the men and the boys and the girls and try to make some decisions on our own.

But today, we do it all together and the Lieutenant Governor is president of the Senate, does an excellent job. And I guess we missed the boat. We should have had her and her husband here this morning to do a little entertainment for us. But that's one reason for you to come back.

Welcome to Carson City. Nice to have you here. Thanks.

COHON: Thank you very much, Senator. I was remiss in not asking if there are other elected officials or representatives of elected officials here with us today, other than the Lieutenant Governor's representative. Are there?

(No response.)

COHON: Okay. Well, Alberto Sagüés, in a display of exquisite timing, arrived right after the end of my remarks.

And I think all Board members are going to try to figure out how he did that so he could miss my opening remarks. But we're very glad that Dr. Sagüés could be with us.

Thanks for getting here, Alberto, despite American Airlines' and Delta Airlines' best efforts to keep you away.

It's now my pleasure to introduce the director of OCRWM, Dr. Ivan Itkin. Ivan is a fellow Pittsburgher, which makes me especially pleased to introduce him. He came to the program last December after a long career of public service in the state legislature in Pennsylvania. Before that, he worked on the Naval Nuclear Propulsion Program at Bettis Atomic Power Laboratory in Pittsburgh.

Dr. Itkin has a doctoral degree in mathematics from the University of Pittsburgh, a master's degree in Nuclear Engineering from New York University, and a bachelor's degree in Chemical Engineering from the Polytechnic Institute of Brooklyn.

It's my pleasure to welcome back to the Board, Dr. Ivan Itkin.

ITKIN: Thank you very much, Chairman Cohon. It's a pleasure to meet again with the members of the Board. Also, it's a pleasure for me, being a long-tenured state legislator from Pennsylvania, to meet my fellow colleagues, Senator Jacobsen and Representative Price, again. Wish them a good day and a pleasant journey to be with you today.

I would, before I begin my prepared remarks, like to just to follow up on Dr. Cohon's brief memorial of Nick Stellavato. We knew him well in the program. I personally had only met him a couple of times before his passing, but I was very much impressed in my meetings with him about his competency, his knowledge, his dedication, and I guess most of all, being a straight talker. He was very down to earth. He let you know what he thought, and he said it in very succinct terms.

Also, he was a good scientist. He did his job well. He was the driving force for the Nye County Drilling Program, which was not only very helpful to providing security to the residents of Nye County in terms of a monitoring system, but also through the drilling program, we in the Agency and the Department learned an awful lot about the geologic and hydrologic properties in that particular region of the test area.

So we very much appreciate his work. We sorely miss him, and we'd like to extend my sympathies and our sympathies to his friends, many friends, and family.

But I do very much welcome this opportunity to come again and to update the Board on our recent progress and near-term plans. I will use my time to discuss the broader issues that affect the program, along with the issues raised in the Board's recent reports and letter. After my talk, there will be detailed discussions on the technical topics you requested.

In June, the full House passed the Energy and Water Development Appropriations Act, which included $413 million for our Program. This amount is a reduction of $24.5 million from our request of $437.5 million. In recognition of the importance of state oversight, the House included $2.5 million for oversight activities. Although this amount is $2.1 million less than the Administration's request, it is significantly larger than in the past several years. I understand that the State will discuss its program later this morning.

The House Committee on Appropriations requested that we prepare two reports for Congress next year. The first is an updated report on alternative means of financing and managing the program. This report, completed in response to a provision in the Nuclear Waste Policy Act, included the feasibility of evaluating various management structures. Second, the Department must submit a plan for the timely development and deployment of waste acceptance capabilities. This requirement reflects the Committee's concern about the limited funding for activities associated with waste acceptance and transportation functions over the past several years.

The Senate Appropriations Committee included just

$351 million for the program, with a substantial share coming from the defense contribution. Because of this, I remain very concerned that if we do not receive adequate funding for Fiscal Year 2001, we may be forced to delay critical program milestones, such as the site recommendation and license application. This is certainly the case should the Senate Committee budget mark prevail in conference. On July 21, the Administration expressed its strong objection to the mark in a Statement of Administration Policy.

We, in OCRWM, appreciate the Board's timely and constructive feedback. I believe the Board's recommendations have led to substantial improvements in our program, especially towards influencing the evolutionary design process.

Our recent efforts to enhance our repository design and better address the uncertainties in repository performance analyses reflect the input of the Board.

Your April report and June letter raise several issues that I would like to briefly address. I see the Board's broad concerns as three-fold. First, understanding uncertainties. Second, increasing the level of confidence in performance assessment. And, third, describing the technical decision-making process, including the ability to accommodate new information into plans.

A central issue has been the notion of uncertainty and its consequences for decisions on the suitability of the site. Level of confidence has always been an important factor in reaching a decision on a repository. As the Board, the Department, EPA, NRC, and the National Academy of Sciences have recognized, uncertainty about long-term repository performance cannot be totally eliminated.

To address the quantification of uncertainty, the Department is developing and documenting a consistent and defensible method of treating uncertainty in our program. We are examining how uncertainties are currently treated in the process model reports, the analysis and model reports, the total system performance assessment, and the Site Recommendation Consideration Report. The goal is to describe associated uncertainties and make the treatment of uncertainty in performance assessment and other program areas technically defensible and understandable to all interested parties. Our intent is that this process will help to gain the confidence of stakeholders and provide a better scientific basis for decision making. We expect that this will lead to continuous improvements in understanding uncertainties as we proceed through the site recommendation process and, if the site is recommended, to license application.

There is recognition that unquantified uncertainties will remain due to the limits of characterizing any site, and to the present limits in our knowledge of natural and engineering processes over thousands of years. As the NRC's Advisory Committee on Nuclear Waste recently noted, the defense-in-depth philosophy is a strategy to mitigate such unquantified uncertainties. Similarly, the Department expects that the analysis of repository performance, together with the safety margin and defense-in-depth provided by the current repository design, will provide a sufficient technical basis to judge whether the site should be recommended as a repository.

A primary objective of the program's engineering and scientific work continues to be to increase the level of confidence in our analysis of repository performance. Our repository design has evolved to better manage thermal loads and reduce uncertainty. Our current design is both robust and flexible. The design can be operated to manage thermal loads by varying parameters, including the period of ventilation, fuel staging, and waste package spacing. We are continuing to evaluate other operational parameters that also could be varied to manage temperature and reduce uncertainties. A repository that is flexible to accommodate technical advances or future changes in priority is one way to address increasing the level of confidence. This approach will permit future generations to learn from operations and monitoring, and to close the facility when appropriate.

We are also evaluating additional technical work to increase the level of confidence for licensing decisions. The work will provide additional assurance that relevant issues are evaluated in the context necessary for decision-making on issues.

In addition to reducing uncertainty through engineering design and scientific studies, we are increasing our confidence in performance assessment by stressing supplementary lines of evidence as suggested by the Board. These other elements of the safety case, such as the analysis of natural analogues and performance confirmation, are also addressed in the repository safety strategy. This fall, we are completing the fourth revision of this strategy, which will support the site recommendation process.

We are committed to making our technical decision-making process transparent. In many cases, relevant criteria emerge and evolve during the course of investigation as the significance of various parameters, processes and the associated uncertainty are evaluated.

As a further means of increasing the level of confidence in the understanding of long-term repository behavior in support of an eventual decision on repository closure, the NRC requires that a performance confirmation program be put in place. It would evaluate whether new information obtained during licensing, construction, operation, and monitoring of the repository confirms the assumptions and bases for the postclosure compliance evaluation. The 50-year retrievability period was established as a reasonable estimate of the time that might be needed to permit repository closure. However, our design would permit future generations to keep the repository open significantly longer, and use their own evaluation about repository closure. We have developed a preliminary performance confirmation plan to support site recommendation, and will refine it to support licensing.

The NRC, EPA, and the Department are working to complete the site-specific regulatory framework for Yucca Mountain. Finalizing this regulatory framework is central to the site recommendation process. Since I addressed the Board in May, both the NRC and the EPA have continued work to finalize their respective regulations. On May 4, we submitted our draft final regulation to NRC for its review and concurrence. That concurrence process continues.

We continue to analyze and develop responses to the public comments on our Draft EIS, and to prepare the Final EIS. Our responses will be documented in a Comment Response Document as part of the Final EIS. As the Nuclear Waste Policy Act requires, the Final EIS will accompany a site recommendation to the President if the Secretary decides to recommend the site for development as a repository.

The emphasis of our work this year has been the developing of the SRCR and supporting documentation. Although the SRCR is not specifically required by the Nuclear Waste Policy Act, it will help support the statutory site recommendation process by assembling information in a format more amenable to widespread public review. We are planning to issue the SRCR late this year. Consistent with our open and transparent policy, we have already begun the process of providing the supporting documentation on the internet, which will include the nine process model reports, the 121 analysis and model reports, and other supporting documentation.

To date, more than 153,000 pages of information are available. The SRCR will consist of two volumes, one containing repository and waste package design, site date and total system performance assessment, and the other containing a preliminary site suitability evaluation. After the issuance of the SRCR, we plan to hold public hearings in the vicinity of Yucca Mountain to inform the public and receive their comments.

As you may be aware, Secretary Bill Richardson recently announced the signing of an agreement with PECO Energy Company to address the Department's delay in accepting spent fuel from utilities. The agreement, which applies only to PECO's Peach Bottom Plant in Pennsylvania, allows PECO Energy to reduce the projected charges paid into the Nuclear

Waste Fund. This would reflect costs reasonably incurred by PECO Energy due to the Department's delay. It is intended to be a framework that can be applied to other nuclear power plants. Negotiations with other plant owners will be conducted on a contract by contract basis.

For this agreement, we estimate that PECO's adjustments could reach $80 million through 2010. The agreement demonstrates that the Department and the utilities can reach a resolution regarding the delay without resorting to costly and protracted litigation. During our negotiations, we were careful to ensure that this agreement would not have adverse impacts on the Nuclear Waste Fund and jeopardize the viability of the repository program. In fact, we believe that if all the other utilities entered into an agreement of this type, there would be no impact on our current activities at Yucca Mountain.

In addition to our work here in the United States, the Department recently signed four agreements to conduct collaborative work with the Russian Academy of Sciences. The Department and the Russian Academy will collaborate on studying geochemistry of actinides, modeling transport in heterogeneous environments, and developing a Russian plan for a repository. This work will increase the understanding of radionuclide thermochemical properties and contribute to the international database development effort, and may support future use of more technically defensible models for radionuclide behavior. The geologic repository plan calls for the development of a coordinated approach between the Russian Academy and the Russian Ministry of Atomic Energy that will help prioritize future collaborative work between the Department and Russia.

I would like to update the Board on the recompetition for our Management and Operating contract, which expires in February 2001. We are evaluating submittals that we received in June from three teams. We expect to award a follow-on performance-based contract in late summer or early fall. Upon awarding the contract, we plan an orderly transition to ensure that we continue to meet the challenging tasks and milestones we have set for ourselves. To that end, we have already established a federal transition team both at Headquarters and at the Yucca Mountain Project.

In conclusion, we are nearing a point where we expect that the scientific information will be adequate to support a determination on whether the Yucca Mountain site should be designated as a repository site, and to prepare a license application if the site is found suitable, and subsequent, to complete the process outlined in the Nuclear Waste Policy Act.

We are now completing the documentation to present the technical basis for a possible site recommendation. My goal is to ensure that the technical basis is explained in such a way that it provides the information necessary to answer the key technical questions and provides a sound scientific basis for decision-making.

Thank you for the opportunity to share my views with you today, and I will be happy, within the time remaining, to address your questions.

COHON: Thank you, Dr. Itkin. Questions from the Board? Ban Bullen?

BULLEN: Bullen, Board. Dr. Itkin, you made your comments about the repository design being robust and flexible, with variations in periods of ventilation, fuel staging and waste package spacing. Could you tell us what the current repository design is? Is it the base case design?

I mean, as we approach the SRCR stage, we really need to know what the design is, and so could you comment on that?

ITKIN: Well, I'm glad you asked that question, because that's a question that we continually are asked to comment on. We see this design as an evolving design. We see that we are responsible to have to document the design that we have established periodically. But you have to understand, as most will, that by the time you see this in public print, it's usually obsolete. The design has changed as we continue to make use of more recent data and more recent decisions.

And so if we look at this design in a way that, say, the Wright Brothers laid it out in 1903, and said is this the design for the airplane, can you imagine a hundred years from today what we'd be flying in. So I raise that as an issue, that as we go through today's technical discussions, some of our more technical staff will give you more insight as to how that design is evolving. But I want you to make it emphatic that this design is not stagnant, that this design will continue to change as we have the capacity to change within the time remaining, all the way probably through licensing application, and perhaps even beyond that.

BULLEN: Just one more quick question. With respect to the PECO agreement and not having an impact on your funding, I guess in the short-term, that's probably true because the payments paid into the Nuclear Waste Fund are never completely allocated each year to you. But as you move into construction and license application and the resource requirements increase, how could agreements with the utilities not have an impact and not be detrimental?

ITKIN: Because we have examined what we believe to be the total cost impact of these settlements will be in view of using the PECO settlement as an example. We have measured it in comparison to what our anticipations or expectation is in terms of the revenues into the Nuclear Waste Fund, and we believe we have sufficient monies to be able to carry out the program as we intend to.

BULLEN: Do you think that Congress will ever allocate more money than is paid in each year to the Nuclear Waste Fund to you?

ITKIN: Do I think they will give us more money?

BULLEN: Well, when you get to the point where actually you require more money to build the repository than comes in each year, do you think Congress will allocate some of the back funds?

ITKIN: Well, let me just say we're going to work with the Congress to minimize the burden to them, and to maximize our productivity.

BULLEN: Thank you.

ITKIN: Obviously, you know, there's a smoothing out. We're working on modular designs as a way of not having peaks and valleys in our revenue requirements as great as they would be without that type of consideration. So we are trying to plan a balanced request from the Congress in a way that they can be able to tolerate it with their accounting processes now in place.

COHON: Last question from Don Runnells.

RUNNELLS: Dan Bullen asked my question about clarification of no impact from the PECO agreements.

ITKIN: Thank you very much.

COHON: Thank you, Dr. Itkin.

It's now my pleasure to introduce Bob Loux, the Executive Director of the State of Nevada's Agency for Nuclear Projects, Nuclear Waste Project Office. Bob has been Director since the inception of the office in 1983, and has worked under six governors on high-level radioactive waste management, and other related issues.

Bob holds a master's degree from the University of Nevada at Reno, which he received in 1972, and has been a State employee since 1976. He has appeared before the Board many times in the past. We're pleased to welcome him back.

Bob?

LOUX: Good morning. On behalf of the Governor Gwynn, I'd like to also add my welcome to you to Carson City for your meeting. If there's any way that we or any of our staff can make your visit here more enjoyable in any way, please let us know. We certainly appreciate the opportunity to make a few remarks to you this morning. I note that we're now into the program and slightly off schedule. I'll try to be brief and make this up for you.

Also, in a few moments, you'll hear from Drs. Barkatt and Gorman on a presentation that we sponsored a couple years ago regarding C-22 that's being proposed by the Department relative to containers for disposal. And, of course, the basis for this research really is rooted in the DOE's allocation and performance at the Yucca Mountain, probably captured in the viability assessment with the bulk, if not the majority, of performance attributable to container, we felt it was pretty critical that we began looking at that issue, and I hope that you'll find this presentation informative and helpful to you.

We appreciate the Board's ongoing willingness to hear our views and the high-level waste program, and on the Yucca Mountain site characterization project. We applaud the Board's availability to hear comments from the interested public, especially during its meetings here in Nevada where the Yucca Mountain project, both from a policy and a programmatic standpoint, has been at the forefront of public concern for better than two decades now.

Now that the national high-level waste program is nearing the point where the Secretary will be making a decision about the suitability of the Yucca Mountain site for the development of a repository, the Board's role is even of greater importance to Nevadans. We at the Agency, and I know all Nevadans really have come to depend on the Board's rigor, objectivity and openness in these evaluations.

With a growing institutional momentum towards the Secretary's suitability decision, the Board really may be the only entity in our opinion left that is not driven by the political pressures on Capitol Hill to move this program along, and may be the only one who is somewhat insulated from the political process. We're depending even more now on the Board's continuing to be the ear and the voice of reason in this otherwise politically charged climate.

Over the years, the growing body of technical evidence from the site characterization has only served to reinforce our view that the site is not suitable for development as a repository. The current eleventh hour flurry to change all of the regulations that affect site suitability and licenseability decisions cannot change the natural inability of the site to isolate waste for long periods of time. It would only serve I guess to reinforce our view about site suitability.

I guess I would note parenthetically one of the major concerns that we've got with the regulatory process is that TSPA in the proposed rule would be the only measure of site suitability, and that causes us a great deal of concern, especially with the large uncertainties associated with those calculations.

The current efforts to try and make the site work through the application of multiple engineered barriers does nothing to improve the safety of the site. The barriers possibly delay releases of radionuclides into the environment, but their projected contribution to long-term repository performance is highly uncertain. At some uncertain time in the future, the repository will perform as if there were no engineered barriers, and that performance, based on available DOE calculations, will not be acceptable compared to any reasonable regulatory standard.

The current goal of making a Yucca Mountain repository work appears to be rooted in adding more engineered barriers. The hope, of course, is to improve the probability solely for regulatory purposes that significant releases will not occur during the first 10,000 years after closure, not withstanding the scientific knowledge gained through site characterization that at some point after the engineered barriers, there likely will be significant releases of radionuclides to the environment. The scientific and technical validity of this conclusion is reasonably well understood and established.

The critical uncertainty then is much less a question if repository performance is unacceptable than it is one of when that condition will actually occur. Attempts to reduce the uncertainty we believe are imprudent and fruitless, especially if the ultimate goal is to achieve the permanent safe isolation of wastes.

Let me turn briefly, if I can, in closing to the site recommendation process, as Dr. Itkin alluded to a moment ago. The first step towards the Secretary's suitability determination is the site characterization considerations report, as you heard, expected to be released later this year. DOE intends this report, which is not required by law, to be the subject of required public hearings in the vicinity of the Yucca Mountain site. It's also been announced that the information contained in the report will be changed and updated in the Secretary's required site recommendation report, which is currently scheduled for release in mid 2001.

It's been announced also that the final environmental impact statement for the Yucca Mountain repository won't be issued until the time the Secretary makes the site recommendation.

Because of these schedule pressures that you heard of earlier, DOE expects the incomplete and outdated considerations report to be the technical basis of the public comment on the Secretary's consideration to recommend the Yucca Mountain site.

Furthermore, DOE expects that the considerations report, without the benefit of a final environmental impact statement, or the actual decision, will be the basis of the governor and the legislature's comments on the Secretary's site recommendation decision, which the Secretary must by law respond in his report to the President.

In this process, commenters, including the Board, if it so chooses, will not have the ability to review and comment on the same material that the Secretary will use as the basis for the site recommendation decision. Obviously, this process will limit the potential of effectiveness of any comments which will be on a report that's essentially outdated, including those of the Board, on the most important decision made in the program to date.

Just yesterday, the Attorney General for the state, Frankie Sue Del Papa, met with Dr. Itkin and we exchanged letters again relative to this issue. It's certainly the Attorney General's view that this process as described by the Department of Energy is not in conformance with the Act, is unreasonable, and really subverts the purpose of the gubernatorial and legislative input into the site recommendation process. It's an issue that we hope to continue to work with the Department of Energy on. I expect that the governor will be talking to the Secretary about this matter in the near term, and hopefully we can get this resolved so that people's comments on the site recommendation report will actually have some meaning and weight.

With that, I'd just like to thank you again for the opportunity to be here and sharing some of our concerns. I again hope that the presentation that is going to be coming up on the C-22, you'll find informative and helpful in your deliberations. And with that, Mr. Chairman, if I can answer any questions, I'd be happy to. Thank you.

COHON: Thank you, Bob. I'd actually like to pursue this latter matter a bit more that you just raised about SRCR. The Board received a copy of your letter and Dr. Itkin's reply. Now, is there another pair of letters, or was that--

LOUX: There's my letter and Dr. Itkin's letter. Now there's another letter from the Attorney General to Dr. Itkin.

COHON: So that suggests that Dr. Itkin's response didn't resolve the matter in the views of the state.

LOUX: From our view, that's correct.

COHON: And just to make sure that I understand, is the concern that by commenting on SRCR, that somehow the State's impact, or rights, under the site recommendation process have been preempted or affected somehow?

LOUX: Not by merely commenting on the considerations report, but the inability to have comments on the final recommendation report with the final environmental impact statement attached to it, and having the Secretary's views on those comments is the process that we believe the law requires, and one that allows for the legislature and the governor to have some substantive input into the actual recommendation decision, not the considerations report.

COHON: So your concern is that the SRCR might have the effect of replacing part of what the process would have been had there not been an SRCR, if I said that correctly?

LOUX: My reading of Dr. Itkin's letter is that they only intend that the SRCR will be available for review and comment prior to the recommendation going to the president, not the actual recommendation report itself.

COHON: Let me see if Dr. Itkin or anybody else wants to comment. Ivan, do you want to comment on this?

ITKIN: It's our belief that we will follow the law. We will follow the law as Congress intended us to do. The SRCR, which is non-statutorily required, is being produced by us. The law makes it very clear that prior to a site recommendation report, that we hold public hearings, and we will do that in the wake of the SRCR report.

The law is also very clear, it says that the Secretary of Energy, if he makes a recommendation that the Yucca Mountain is suitable as a repository site, he must notify the State of Nevada and its legislature within 30 days of informing the President, and that we will do. If the Secretary decides to recommend this report, we'll follow the law, and the Secretary will advise the State that he intends to recommend to the President.

At the end of 30 days, it's our understanding, following the law, within that 30 days, after 30 days, the Secretary will send the SR and the FEIS, as required by law, to the President. The President will make a judgment on all available information, the NRC sufficiency comments, by law, any comments that the State of Nevada chooses to provide at that time to the Secretary will be done.

We are in a position at this point in time to move forward and to get to the point where the Secretary of Energy can make that decision.

If the President should make a decision to go forward, obviously there is a Congressional action that needs to be taken, and the State of Nevada can take such action. If the State of Nevada chooses to object to the program, it may do so after the Presidential decision. They also may make their views, if they feel compelled to do so, to the Congress, and the State's position will hold, that is, the State of Nevada has the right to veto, and if the State vetoes it and Congress does not override that veto, it holds. It will not go forward.

On the other hand, if the State is not able to convince the Congress of their position, and the Congress chooses to override, then of course the program will go forward.

And so there is redress. I just want to say this very clearly, that it is our intention to explicitly follow the Nuclear Waste Policy Act and everything that it requires us to do.

COHON: Thanks. Other questions for Bob? Dan?

BULLEN: Bullen, Board. We heard about the House Appropriation of 2.5 million for oversight, and I don't know what the Senate gave. But do you have sufficient resources at this critical point in your review from the legislature to do that? I know there's never enough money, but is it an improvement? Can you bring us up to date on what your budget fund is?

LOUX: Well, I think the situation is improving. The Senate did agree with the House number, so it's not a conference issue at this point, at least my understanding. That would certainly help us to go a long way in continuing some of the work, for example, that you're going to hear about earlier today.

We're able to keep things going and focus very sharply on those key issues. We're obviously not able to do the kinds of things that it would be nice to do, but really not critical, so it's really caused us to have to focus dramatically. In some sense, DOE's performance assessment and allocation of performance has in some ways helped that situation a lot with their view that the site provides essentially very little in the way of performance. It doesn't seem to us that that's an area that we're probably going to spend a heck of a lot more resources, especially if according to at least one calculation, 95 per cent of the performance is going to be attributable to the container, it seems to us that's the place to put money. And I think we'll have sufficient resources in that regard.

COHON: Thank you very much, Bob.

LOUX: Thank you.

COHON: Richard Parizek, a member of the Board, will now serve as Chair of our first technical session.

PARIZEK: Good morning. My name is Richard Parizek, and I will be chairing the session on scientific and technical issues. Because of the time constraints we're under, I'm going to kind of cut my remarks brief in terms of the introduction I was going to give you. But first, I wanted to add my remarks with those of others on Nick's passing. It was a great sadness and shock that the Board learned of his passing. His wife, Sandra, as I understand it, has lost not only her husband, but mother and father, all within the last few months, and she has a very heavy burden to bear.

Those of you who had the pleasure to know and work with Nick will remember him as a doer, an up-front production man. He had a passion for his work in Nye County on the early warning drilling program, and he had a passion for Nye County. The early warning drilling program would provide valuable information on available water resources in the Amargosa Desert region that might be put to use by local residents. It would provide important data on the water quality related issues concerning the Nevada Test Site activities, and the Yucca Mountain project. He knew his work, and that of his associates would add confidence to site scale and regional groundwater flow models of importance to the DOE systems performance and biosphere issues. 23 new groundwater geochemical and geological control points would add significantly to the understanding of the regional groundwater flow at the interface between fractured volcanic rocks and alluvium.

I've said publicly Nick's program was the best show in town in providing important new geological, hydrological and geochemical data at a critical time in the Yucca Mountain project. A lot of people associated with the Yucca Mountain project continue to stretch the envelope as highly dedicated public officials, responsible, concerned individuals in the face of criticism. Nick was such a person.

Thank you, Nick, for your warmth, respect and friendship. Your shoes will be hard to fill.

Now, we all know that DOE is working very hard to formulate a site recommendation. Following this session, we'll hear a great deal about the total system performance assessment. It will constitute a very important element of that site recommendation.

However, we should not forget the important ongoing and plan scientific and technical work. These studies have a bearing on many of the technical conclusions lying at the heart of determining whether Yucca Mountain is a suitable site for a repository. At the heart of determining the engineered aspects of the repository and the behavior under repository conditions for thousands of years, past experience indicates that we would indeed be foolish if we assume that we know everything that needs to be known about the site and the proposed engineering system. The Board looks with great interest at ongoing and planned investigations.

We should then begin, I think, with the first presentation from the State of Nevada, Aaron Barkatt and Jeff Gorman, concerning their research on Alloy 22. I will not delay the beginning of their presentations any further with my remarks.

BARKATT: Good morning. The next presentation on Alloy 22 will be given by Dr. Jeff Gorman from Dominion Engineering.

COHON: I'm sorry to interrupt. If you'd just tell us your name, because there are two people, and our reporter needs to know who is who.

BARKATT: I was going to say Dr. Jeff Gorman of Dominion Engineering and myself. My name is Aaron Barkatt, and I'm from the board of the oxide chemistry group at the Catholic University of America in Washington, D.C.

However, we have a third member of our team, who we are fortunate to have her with us, Dr. Staehle who is Dean of

Sciences and Engineering at the University of Minnesota, and is a leading member of the corrosion science and engineering community.

GORMAN: Excuse me just a minute. Is it possible for us to use this as a hand held mike and both of us to be here?

BARKATT: We started working for the program of the State of Nevada several years ago in the area of glass durability, and we were the first to come up with a comprehensive model for the effects of solutes in aqueous media on glass dissolution, and we came up with an extensive database, as well as a model. And at the same time, for the past twelve years or so, we have worked in the nuclear industry on corrosion issues. And during that work, we came to form a close cooperation with Dominion Engineering. That's a firm based in Virginia, that for the past 20 years, had a key role in addressing corrosion issues in the nuclear power industry.

So when a few months ago we were asked by the State to refocus our efforts from glass to C-22, we of course immediately invoked this cooperation with Dominion Engineering, with Dr. Gorman and his colleagues in trying to address these issues, and to plan a series of initial studies, both literature surveys and experimental studies, which have been going on now for only a few months.

The studies which we are planning are based on the experience of both our groups with materials issues in the nuclear industry. For instance, and this is an example which we are going to talk about today, and to exhibit the results, we have started looking into an issue which has been bothering the industry for the past few years, and that is the role of potentially corrosive trace species such as lead, mercury, arsenic, and so on, the interaction with those high nickel alloys used in nuclear components. And we started looking again at lead and some of the others.

The experiments which we have been doing so far have been preliminary in nature. The intent of this program is not to compete with the extensive and high quality experiments carried out by the national labs and universities for the DOE. The intent is strictly to try to point out issues which have not been completely covered, and try to suggest these for future studies whenever necessary.

So based on this concept, we have not started the experiments which we are going to talk about now, are not in any way meant to describe the interaction of lead or mercury with C-22 under service conditions. They are meant, rather, to ask the preliminary question, does that interaction occur and does it merit future attention. Again, what we are describing right is not meant to answer the question how is lead going to affect C-22 under service conditions. We are still far from being able to answer that question.

But, again, preliminary to talking about service conditions, there are two questions which come to mine. Is it possible that we will have lead, for instance, or mercury, or other potentially active ingredients, in the repository environment? And the second preliminary question is under any condition, accelerated, if you wish, to a great extent, is it possible that C-22 interacts with such species?

Now, with respect to the first question, it is not possible I think at the present time to exclude the possibility that lead may be in the repository environment, both as a result of natural causes, the fact that there is lead in the rock, and some of it may solutize, and as a result of man-made operations at Yucca Mountain, the presence of components such as shielding or solder which contain lead, or as impurities in other materials which may go into the repository. So because this cannot be a priori excluded, comes the question can lead at all interact with C-22.

So, again, the tests which have been carried out over the past two or three months, which is all we had so far, was to try to take a look at possible interaction. We used two types of samples. We used U-bends, stress U-bends, both welded and non-welded, and we used static disks. The U-bends were explored to a temperature of 250 degrees centigrade, the disks at 160 degrees centigrade. We used J-13 water concentrated by a factor of 1000. Both of these were of type alkaline, pH of 12, 13, and certified to a low pH, and we tried to look at the results of introducing lead to the system. Again, the J-13 concentrate which we used, based on studies by Rosenberg and co-workers at Lawrence Livermore, and based on the water composition, the temperatures and the samples which we mentioned, we built up to a preliminary matrix of experiments, and Dr. Gorman will go into the detailed results of these tests.

GORMAN: I'm Jeff Gorman, principal engineer at Dominion Engineering. This is the matrix of these U-bend tests that have been rather short tests, maximum time of 32 days, and most done at 250, one at room temperature and a couple at 200, or a couple at room temperature. And with the range of pHs, as Dr. Barkatt was saying, and here's the room temperature pH, and here's the calculated high temperature pH. And the matrix indicates what additives were added. The most important ones to note are for the lead case where we had, as we'll show in some examples in just a minute, some cracking starting after about a week of testing, had about a half a percent of lead in this case here. So we think there's actually very little additional sulfide actually added. So this is sort of a base case without any significant additives.

The main results of these tests were one was lead cracked. We got some significant pitting, and this we'll show you, in a couple of other cases. All of these three cases are cases using hydrochloric acid, which seemed to have more severe effects than sulfuric acid. We also got some effects on the surfaces, and we'll show some weight loss and chemical results in just a minute.

This is the same matrix of tests now with the measured main elements in the post test solution and the percent weight loss. You see these three here are the same that had the crack and the pitting, had the most weight loss, but there was some weight loss in other cases, and some dissolution of the base alloys in these other solutions also.

BULLEN: Bullen, Board. Just excuse me for a second on that figure. The NM's are?

GORMAN: Not measured.

BULLEN: Not measured or not measurable?

GORMAN: Not measured.

BULLEN: Okay.

GORMAN: And we started with a half a percent lead, and the engineer who was doing it just thought it wasn't worth testing.

These specimens, I forgot to say the size were about 3/4 inch wide, 1/8 inch thick, and about two inches long to start with, bent around a 1/4 inch rod, and then held with a C-276, and not with Teflon washers isolating it from the U-bend, with calculated 25 per cent strain at the outer surface. This is after two weeks of testing. The crack was detectable after one week of testing, but we kept it in to see whether it would continue to grow, which it did.

This is more detail of it. This is the ID, this is the OD. This is the crack. And as we'll show on another figure, starting transgranular on the first half, and then apparently as stresses decreased, it started following an intergranular path.

This is just an overview showing the width of the U-bend. This is about 3/4 inch, and as you can see, that's the crack. This is that same specimen now with the teflon washer removed, and you see some pitting kind of attack also occurred on this specimen, as well as the cracking.

This is the detail. This is a cross-section. I think this is the OD, with the more transgranular propagation, turning to intergranular, sort of typical intergranular branch, showing a pretty strong corrosive attack as opposed to just a straight break cracking kind of attack. And transgranular, this is the SEM view showing transgranular, and then intergranular attack.

In looking at another part, on the OD surface, high stress surface, you can see that there are many other incipient cracks starting, so it wasn't just one single place.

Switching to the sample with mercury, mercury also seemed to cause pretty severe pitting kind of attack, as you can see here, and as shown by the dissolution of elements and the weight loss also. This is the sulfur added to the acid, and as we note, a lot of the sulfur was not present in the actual test. It's sort of removed during the heat-up process.

Okay, these are the disk tests here.

BARKATT: The tests on the U-bends, the U-bends were stressed, they were studied at a temperature of 250 degrees Centigrade, and with pHs as low as about .6. The tests on the disks, on the other hand, were carried out in unstressed samples at a pH of, again, either 12, 13 or mildly acidic to .5 to .6. The most important thing, 160 degrees Centigrade instead of 250 degrees Centigrade. And as you can see, we tried, this is with a very scoping type of experiment with the disks. We had no additives with the base case. And as you can see, you need to compare these results against the base case. At the same pH, you can see that chromium is about 50 per cent higher in terms of dissolution, and molybdenum is about double.

But when we go to mercury, we can see a very pronounced rise in the extent of dissolution of both chromium and molybdenum, and in addition, in a mildly acidic environment. Of course, you have much more dissolution of all the C-22 components. And in agreement with the wet analysis results, there was one sample that stood out, the sample which was present, again at 160 degrees, mildly acid conditions after two weeks, showed no cracking, but extensive, extensive pitting, and in some cases, the pitting was overgrown by a deposition of corrosion products.

So as you can see, we already started reducing the acceleration factors, and we were still able to see in the presence of lead a very significant amount of attack.

The main findings first for U-bend.

GORMAN: Okay. The main findings of the U-bend tests were that we saw corrosive attack where those exposed to acidic environment. Without additives the corrosion is mild, and some shallow general corrosion and mild pitting, but possibly with some deposition. When you add mercury, you get strong general corrosion, some pitting, and some deposition of corrosion products. We didn't see any accumulation of mercury on the corroded surface.

When you use lead as the additive to the acid, cracking occurs first in a transgranular mode. When the stress gets relieved at about the halfway point, the crack grows in an intergranular mode. There's a lot of secondary cracks, mostly intergranular.

There was corrosion product deposition observed, and enriched in silicon and depleted with respect to nickel and tungsten. There may have been some pitting preceding the cracking, and a lot of lead concentrates on the crack surface. This is contrast to mercury, where mercury doesn't seem to attach to the metal surfaces, lead does. So it seems to be a different mechanism than the effect of mercury.

BARKATT: With regard to the unstressed disks, again, these were done under milder conditions, and we still observed a corrosive attack, extensive pitting. We observed some deposition, and we observed by EDX about 11 per cent of lead on the surface, the pitted surface. It appears as if a very large fraction of the lead available in the environment ends up on the surface of the C-22 samples.

With regard to the chemical analysis of the dissolved species, what we can see here is that in the presence of both lead and mercury, we have extensive dissolution of these ingredients, in particular nickel. And it appears the severity of the attack as measured by wet analysis as well as by observations of the sample agree pretty well.

In the basic concentrated J-13, J-13 without acidification, mercury still gives significant enhancement of the dissolution of chromium and molybdenum from the C-22. So we were able to point out both mercury and lead as candidates for further studies on these interactions.

GORMAN: So our main conclusions from these preliminary tests are that in some environments, small amounts of aggressive species that could be present in the repository water, lead, mercury, arsenic, species such as that, can strongly aggravate pitting, crevice corrosion and SCC. And we conclude that any qualification program for the alloy needs to consider the possible presence of these species, any that are in the environment that could be transported to the metal surface.

BARKATT: Let's return for a moment to the initial question of can you have lead and/or mercury in the repository environment? And here with support from Dr. Morganstein and Shettle, Geoscientist Management, they were able to go through the literature, and to go through studies by the USGS, and under the sponsorship of the Department of Energy for the hydrology and geology of these sites. And what we can see when we look at the water itself, the water itself in these environments contains between a few tenths of ppm and a few ppms of lead, and mercury values are not available.

But, again, it's important to point out that it's not only the concentrations that are important, but also the total amounts of lead, if so much lead ends up on the C-22 surface itself. So since the total availability of lead is important, we should look at the rocks surrounding the repository site, and when we do that, we end up with values ranging from a few ppm to 100 to 200 ppm of lead in the whole rock formations around. And with respect to mercury, somewhere between values of sub ppms to a few ppms of mercury, which are available and could be solubilized during repository operations.

GORMAN: We're now switching to another aspect of this preliminary work, which is sort of looking at what has the experience been with similar kinds of alloys in the nuclear power plant industry, and saying from that experience, what do we learn as to how a qualification program for new material for a new application ought to be qualified.

There have been, unfortunately, lots of--well, unfortunately for the industry, lots of failures of materials, fortunate for those of us who have made a living trying to understand them and try and fix things based on them. The ones that seem to have the biggest impacts that we've listed here are the austenitic stainless steel for BWR structural materials, piping and core shrouds; inconel 600 for PWR steam generator tubes and for nozzles like control rock drive mechanism nozzles and instrument nozzles and pressurizers and the like; X750, which is a high strength precipitation hardened nickel alloy used for bolting and similar kinds of applications, which has had extensive failures in both BWRs and PWRs; A286, which is a precipitation hardened stainless steel used in one class of PWRs and experienced extensive failures; 17-4 PH, a

Martensitic precipitation hardening stainless steel, which has been used, was widely used and still is widely used for valve parts and bolting, and has had many failures; Martensitic stainless steel, similar kind of applications and similar types of failures; zircaloy fuel rod cladding, which was selected for its good corrosion resistance and low neutron cross-section, but then was found to be susceptible in both BWR and PWR to stress corrosion and cracking from the ID of the pipe.

We went through and looked at each of these in some depth trying to decide what sort of reason, why did we get into the problem and what lessons to learn. In the interest of time, I would like to leave those and go to the summary of lessons learned. What it seems that the--how we got into all those problems which have cost hundreds of millions of dollars and long delays at many plants for repairs and changes of materials, the reasons seem to be there wasn't a full range of realistic service environments considered, potential pH, aggressive species. And potential, for example, apparently the effective small amounts of oxygen and BWR, coolant wasn't adequate, its effect on sensitized stainless steel was not adequately investigated back at the time of the selection of the stainless steel, and so we've had this long continuing problem with cracking of the weld zones in austenitic stainless steels and BWRs.

With regard to pH, an aggressive species, the best examples of those are in BWR steam generators, alloy-600 steam generators where the pH can go either very low or very high due to concentration occurring in boiling zones, compounded by effects of small aggressive species, such as lead, which is in the feed water typically in the 20 ppt range seems to have an ability to collect on cracked surfaces, and under oxides, we measured them sometimes in some recent failure examinations on some steam generator tubes in the 3 or 4 per cent on crack surfaces, despite it being in the 20 ppt in the water. So nickel alloys have an ability to sort of concentrate some aggressive species.

So that's the first category of how we seem to have gotten into these problems. Another aspect is the realistic range of material conditions and compositions wasn't evaluated. The effects of centralization from long range is an obvious one. The possibility of trace boron in alloy-600, Inconel 600, leading to increased susceptibility to caustic attack is another, where it's not a controlled measured interstitial species, but it's there and it can effect its performance.

Another aspect is the range of total stresses, and in particular, including residual stresses and applied strains not adequately considered. The residual stresses often resulting from things such as insertion gouges, parts falling on things, surface effects. In many cases, the testing didn't adequately use a staged planned program of accelerated testing so that you could extrapolate for the full life, 40, 60 year life. The tests were shorter term and really only focusing on the expected behavior in the first couple years of service.

I've already mentioned the aggravating effects of surface damage, not only residual stress, but local cold work often accelerates the cracking process, from polishing of the OD of Inconel, the grinding of stainless steel in BWRs is found to be a big accelerator, for example.

Last one on the list is long term material aging effects were not adequately addressed in some cases, 17-5 pH being probably the best example of where if it's up at high temperature for a long time, it embrittles and becomes more susceptible to stress corrosion.

The main lesson is you've got to consider all those factors when qualifying a material, and from our reviews of the literature that we've been able to get ahold of, it seems that all of them have not been adequately reviewed for C-22 at this stage.

Lots of tests have been and are being performed on C-22. Some aspects that seem to us warrant some more attention, they haven't addressed the possible effects of trace aggressive impurities such as the ones we list, and several others that are in the environment that could possibly get to the metal surfaces.

We haven't seen that they've addressed the full range of water chemistries and concentrations that could occur, such as with boiling on a super-heated surface where in the PWR steam generator world, we find that the concentrations that you can develop with some super-heat, 10, 20 degrees super-heat can cause very rapid corrosion of similar nickel alloys.

We haven't seen tests that address the full range of base material compositions, including trace deleterious impurities. Now, that's trace deleterious impurities in the metal, not in the environment. And then the conditions, welding and cold work, we know that some work has started on the welding aspect.

So the intention is to cover those insufficiently addressed aspects, and with the assistance of Dr. Staehle and Dr. Barkatt and ourselves and Dr. Morgenstein and Shettle, they're going to try and develop a test program that addresses those aspects that haven't had sufficient attention so far.

PARIZEK: Thank you for your presentation. We are on a tight time schedule, but I think it's a very important presentation that we ought to allow time, and the coffee break can suffer, and those that need to go out for a moment can do so, but I think questions generally are very important because Alloy-22 is critical to the performance of the repository, and if there's some inconsistencies with what the program and what you folks are finding, it's important. Obviously, your results are not published. You've gone through the peer review process, I believe. I assume you intend to make those kind of publications somewhere in the future.

But I would be wanting to know more about the study plan, your last page, as to how you would lay this out and what time frame would be required to adequately address these concerns, what kind of dollars are required, and does the State of Nevada have that kind of money to support your program available to you at this time?

GORMAN: This is a subject that's under discussion with them at this time, and I guess it depends parts on the political process here over the next few months. So I think I'm not in a position to address that question. We've done some preliminary thinking of what sort of resources would be required. We've had some preliminary discussions with the geosciences management and with the state, but these are very preliminary, and so I don't think I can answer your question at this time.

BARKATT: I think in retrospect, one very important aspect and a lesson that I think was learned in other stages of our programs is that a contingent fee is made for longer term tests so that the validation of predictions made from short-term tests can take place over longer periods of time that will allow us to gain more confidence in extrapolating from accelerated tests to service conditions.

PARIZEK: Alberto Sagüés?

SAGÜÉS: Were these tests performed like in autoclaves?

GORMAN: Yes, they were performed in small stainless steel autoclaves.

SAGÜÉS: I see.

GORMAN: With teflon liners.

SAGÜÉS: With teflon liners. What kind of pressures would you build in?

GORMAN: It would be the saturation pressure for 250, and at 288, it's 1000 psi, so it's probably 600 psi, something in that order.

SAGÜÉS: Right. And how much of the volume of liquid, about 100 cc, something like that?

GORMAN: About 150 cc.

SAGÜÉS: 150 cc. So when one looks at the concentrations at the end, one can get an idea of the total amount of that.

GORMAN: Yes.

SAGÜÉS: And those were single tests, like for example, the specimen that showed the crack, is that one test?

GORMAN: That was one specimen. And in some cases, we had two specimens, but I think in that case, it was only the one specimen. These are not duplicated, and of course in a full scale program, we would intend to go to statistically significant numbers to be able to get some statistics, 10, 100, or something in that order, under each environment, and then staged at different temperatures and staged with stresses to try and get ability to predict to service conditions, or a variety of service conditions.

SAGÜÉS: And lead was in the form of lead acetate or some such sort?

BARKATT: Lead was lead acetate.

SAGÜÉS: Acetate. And that was all soluble, in other words, you ended up with salts at the bottom of the autoclave?

BARKATT: This is again, I apologize for reporting very preliminary data, we put in half a per cent, and in one of these test what we ended up with was .14 per cent of lead in the solution. Now, some of it may--we didn't notice a voluminous precipitate, but our feeling is that a lot of the lead ended up on the surface of the corroded metal.

PARIZEK: Okay, thank you. Dan Bullen?

BULLEN: Bullen, Board. You may know that the Board is interested in cooler repositories for reduction of uncertainty, but the question that now arises is I saw that the room temperature data had very limited impact on it. And as we go to cooler repository designs, and perhaps not boiling surfaces on the waste package, would you expect there to be an acceleration or a deceleration in the impact of the trace impurities on the corrosion?

GORMAN: Oh, I think most of the corrosion processes are thermally activated so that the lower the temperature, the better. There are a few hydrogen cracking mechanisms that are worse at lower temperatures in these high nickel alloys, but almost all of them, most of the ones that we would be worried about are thermally driven, so the lower the temperatures, the better.

BULLEN: Do you think your data would be able to identify a threshold temperature that would be of critical importance for the trace elements, or is that going to be sort of beyond the scope of what you have planned?

GORMAN: If the program is funded and carried out the way we envision and we get the kind of results we expect, the intent would be to allow--there's no threshold where it actually stops, but you get--the time scale gets stretched out the lower the temperature. And so you get results at different temperatures, and then extrapolate to service conditions. So the answer is is you try and get results at a sufficient number of temperatures, and at sufficiently low temperatures, so you have pretty high assurance of predicting to service conditions.

BULLEN: Right. But the key there for service conditions was already identified by Dr. Sagüés, in that if your atmospheric conditions, you're kind of limited to how high you can go in temperature because of the fact that you're at saturation and you can't get beyond that.

GORMAN: Well, if you get deposits on surfaces and you have a heat source that can get above 100 C., it depends on what sort of deposits you get on it, how much insulation that is, you can then, in that deposit, get temperatures above 100 C. and you can also get boiling point elevations and concentrations up to the 10, 20, 30 per cent of dissolved solids in those deposits. So there may be some possibility of that kind of environment developing.

BULLEN: But the critical part there is trying to figure out where that is and how to do the extrapolation then?

GORMAN: Yes. Well, there's two parts of it. There's doing the tests for the extrapolation of the data, but there's also the definition of the environment from a realistic set of scenarios, as to can you get deposits on the surface and can the temperatures be raised by those deposits.

BULLEN: Thank you.

PARIZEK: Debra Knopman?

KNOPMAN: Knopman, Board. Just a real quick followup to Dan Bullen's question. Would there be any particular significance based on your preliminary work with temperatures between 96 degrees C. and 100 degrees C., for example, at boiling, is there anything in particular that changes in that range? Would you expect some change in the temperature/corrosion curve, corrosion rate curve?

GORMAN: For the kind of mechanisms we were looking at in these very preliminary tests, no. There are some degradation modes that affect high nickel alloys that are worse at the lower temperatures, and they seem to be hydrogen driven processes, so the hydrogen doesn't diffuse out of the metal so fast, and you get faster cracking at sort of the just below boiling point conditions.

So, in general, the answer is that there's no particular threshold, and it gets better the lower temperature you get, but there are a few low temperature processes that possibly need to be considered.

PARIZEK: Chairman Cohon?

COHON: I know that you're very early in the process of your studies, which are very interesting, so my question is probably unfair, but it's a very key question. And that's extending the results, and even later results after you continue to do your work, to the very long times that these waste packages have to perform, you mentioned in your review of some of the instances of failures in nuclear power plants, that one of the failings in terms of design was not thinking through or not designing for the full lifetime. And you mentioned 40 to 60 years. In this case, though, we have to extrapolate to hundreds of thousands of years. Thoughts about this and, you know, what should one do in order to get from where we are to where we need to be in terms of making reasonable predictions over such long periods of time?

GORMAN: The standard approach for addressing that question is to do a series of tests at different temperatures, with longer times required at lower temperature, and then extrapolate to the times, to the service conditions. We, of course, looked at that on an exploratory basis, if it cracks in one week, in 250, what does that mean in terms of service condition. That all hinges on a parameter called the activation energy.

If the activation energy is in the low range for this kind of process, 25 kilocalories per mole, then the one week occurs within the design lifetime of these parts. If it's at the high end, 50 kilocalories per mole, then the one week, 250 C., it doesn't indicate a problem in 10,000 years. But the activation energies that you get for these cracking and crack processes range sort of from 25 to 50 kilocalories per mole. So at the lower end, the cracking in one week would indicate a problem in the design lifetime of these barriers.

Does that address your question?

COHON: Yes.

GORMAN: Could we let Roger Staehle add a comment on this?

PARIZEK: Yes, and let him introduce himself for the record.

STAEHLE: I'm Roger Staehle. I'm an independent consultant. I'm also an adjunct professor at the University of Minnesota.

There are sort of two main ideas here in responding to this question. First is C-22 is basically an acid alloy. It has a molybdenum concentration, chromium concentration which are basically acid oriented. You probably understand the thermodynamics of the problem. C-22 is not basically a neutral or an alkaline alloy because the high solubility of molybdenum and tungsten and chromium as you move up in pH suggest that it should begin to dissolve and corrode in those cases. So you have to kind of think about that general pattern.

The second general pattern about prediction is Jeff made a very important point that I assume all of you picked up, and that is that the lessons learned from the nuclear industry pertain directly here. And some of you know that the Alloy 600 was picked by somebody who would sit at the right hand of God, some of you know the story here and some of you don't, but Admiral Richover sort of unilaterally decided that Alloy 600 was the right material no matter what. That alloy turned out to be among the most corrosion prone alloys that ever existed. And that alloy is in the same family as your C-22. It's not exactly the same, but it's very close. And the reason that the problems of failure occurred, as Jeff pointed out, were basically people wanting to define the environments very well, and second, didn't define the limits.

And so what Jeff has done is very nice work, is actually begin to define some of the limits of the performance of this material. So if you would have asked in 1960 what is the expected life of Alloy 600, he would have said, well, something semi-infinite. Today, actually through a lot of Jeff's work, the life is not zero, but it's not all that very good either, and to have a one year experience base for the application of C-22 of this application in that framework is an almost laughable kind of a basis for making a prediction.

So I think that it needs a very serious thoughtful consideration of the site chemistry, the reality of the site chemistry, and the reality of the boundaries, and then you can begin to make some predictions.

COHON: If I could just follow up real quickly, Richard, because it would get to a key point? Suppose the environment in which the material would have to operate were specified, though that's a big issue, I have a specific question with regard to the kinds of experiments you would conduct if you had to answer the question how long will C-22 likely perform. It sounded from your answer like if you had a year and enough money, you could come up with--whatever answer you could produce in that year would probably be as good as you could produce in two years, or three. Of course, you'd love to have 9,000 years probably, or 9,999, but given like one, two, three years, am I coming to the right conclusion, based on what you said?

GORMAN: I think a little optimistic. To get sufficiently long tests to get--use a low enough temperature so that you have higher assurance in your extrapolation to service temperatures, we've estimated that you need about three years to get the length of tests that you need to do it. So you'd get some useful information in one year, but your uncertainty would be quite high trying to extrapolate down to service temperatures.

COHON: But five years would not provide a very large gain over three years?

GORMAN: That's my preliminary assessment. Long tests always take longer and are more difficult than you anticipate.

STAEHLE: But that's not really the total answer to that problem. It's not that you can run a test in a week, and

Jeff's done this, or two weeks, whatever. The activation energies for the systems are well known. I mean, you can just say it's either between this or this, and make an extrapolation, and so why do the experiments at lower temperatures. The bigger issue I think is not the question of how they extrapolate. The bigger issue is what is the set of chemical species that is of concern. And the Alloy-22 I don't think is sufficiently well understood from the point of how it interacts with the environmental species, nor how the environmental species would build up in time, and I think it would take some time to not do the experiment in a sense, but to figure out the right set of things, that is, the respectable engineering, and that's what would actually take more time. And, of course, you need some extrapolation base to be legitimate, but as a first cut, I think you can kind of almost assume the extrapolation.

PARIZEK: Can you identify yourself at the end?

STAEHLE: I'm sorry, excuse me. Roger Staehle, consultant.

PARIZEK: Priscilla Nelson, Board.

BARKATT: May I just add a quick comment to that? We have to be careful when we use the term extrapolation. When one thinks about extrapolation, first cut is it's a linear extension of observed results to service conditions, assuming that you can linearly plot something and continue in a straight line, which we know from metal corrosion that has been first discovered in the 1920s, as well as in the corrosion of ceramics, composites, glasses, is that in many cases, you may have non-linear effects where your initial degradation or corrosion life is low, and then due to some effects, physical, chemical or mechanical effects, all of a sudden, your corrosion will take off.

Now, that means that linear extrapolation by itself is insufficient to come up with confident prediction of what's going to happen in the long term. You must good understanding of the total mechanism, of all the mechanisms that can lead to such an effect, and this is one very important reason why long-term studies are very, very important.

NELSON: Nelson, Board. I realize that it's possible--we have been thinking, at least I have been thinking that, you know, Alloy 22 is the super metal, and we're making it seems more mortal, but I'm struck by Slide 31 when you talk about the reasons for unexpected failures, including most of your points really require some knowledge of realistic environments, or realistic material conditions, realistic ranges of stresses. And I'm wondering, in hindsight, thinking about the nuclear power plant experience that you talk about here, it's possible to understand what those realistic stresses environments are. But evidently it wasn't possible to anticipate what those realistic conditions were before.

I'm wondering how good we're going to be at understanding what realistic means when you try from here to do forecasting. Do you have any comments on that?

GORMAN: My comment is that we ought to do a very serious job of looking at all those factors that have caused us problems in the nuclear power industry, and learn from that experience as best as we possibly can. And that's what we recommend be done to try and define both the environmental conditions to which the barrier will be exposed, and then also the range of material conditions itself, and then all these untoward sort of things that happen to real materials that induce increased stresses and susceptibility to cracking, the scratches, the grinding, all of these kind of things, and address those either in the predictions of life or by design processes that control them.

STAEHLE: Roger Staehle. I'm also working with the Nevada people. Actually, the approach, and you're really right on in terms of saying you can't define all this all that precisely, and there's no question about that. You can't. But what you can do, you can bound the system, and there are certain ways of bounding systems thermally, chemically, from a stress point of view, without making the boundary so broad you can't build anything. And that's a problem with bounding.

And so it's quite within present technology to bound a system like this and work within it and demonstrate whether it works or doesn't work within that bounding system.

NELSON: Nelson, Board. Presumably, that would have been possible in the nuclear power plant industry in the past as well. I guess based on what you understand about the testing that's going on on the project right now, are what you would think as being realistic conditions, realist environments being tested and evaluated in the existing program? And it's fair to say that you don't know, if you don't know.

GORMAN: The answer is that from what I do know, they're not being completely covered, but I don't know the whole program, and I expect to be better educated by the end of these two days.

PARIZEK: Parizek, Board. You spoke about the non-linear nature. You have accelerated rates. You can also have decelerating rates as a result of passification of pits, as an example, you didn't count on them?

BARKATT: Certainly. That is why a complete mechanistic understanding is so important. I think I would not be very wrong in saying that cases where you have perfect linear behavior, super linear or sublinear behavior appears to be the rule.

PARIZEK: Questions from the advisors for the Board? Kessler or Ewing?

EWING: Rod Ewing, an advisor to the Board. Did you identify any of the corrosion products that formed in your experiments?

BARKATT: No, not yet, except for EDS, which Jeff identified on the surface, we haven't come to that yet.

EWING: And did you try to calculate the species that would be in solution, say if it's a lead, mercury and arsenic?

BARKATT: To calculate the speciation? These experiments that we're talking about started two or three months ago, and we are planning to do that in the near future.

EWING: And then finally, the very low pH values, do you envision them as part of the accelerated experiment, or are they relevant to the repository condition?

BARKATT: Well, there are several answers to that. Again, we tried to answer the question of are these interactions possible at all, so we went to highly accelerated conditions. But as you could see, we already went to much milder pH, to .6 only, and we still observed extensive pitting.

In addition to that, the temperatures with the pHs that we are talking about are room temperature pHs, and when you look at a pH at temperature, they're not so much out of realistic range as might appear from looking at the room temperature values.

PARIZEK: John Kessler, Dr. Kessler?

KESSLER: John Kessler, EPRI. Can you give us some kind of idea what you've been funded for in terms of the next round of tests, lower temperatures, whatever, longer time periods; what's next?

BARKATT: Again, this meeting, and the meeting with the State of Nevada people, will already be taking place here, are about priorities. We believe that the most important next stage is, again, what Dr. Staehle referred to, a definition of the expected environments, so we will know what we are supposed to extrapolate to.

Following this definition of expected conditions that we hope to do based on review of DOE documents and other literatures available, we will then be in a position to plan a detailed matrix of experiments which will allow us to do this extrapolated work.

PARIZEK: Questions from staff?

DIODATO: Dave Diodato, staff. I was just wondering, I know that you still have many more experiments you'd like to do, but you presented quite a range of experiments here this morning, both under stressed and unstressed conditions in a variety of environments, and with a variety of accelerants and different concentrations. Would it be appropriate, even now in a preliminary way, to think about a multivariable regression to identify the relative impacts of the different factors, and separate those out in kind of a quantitative fashion, at least in a preliminary way? Or is that not done?

BARKATT: Certainly it would be appropriate. But since we have talked so far about scoping studies, we are still trying to isolate parameters and study them one at a time.

STAEHLE: Maybe I could respond a little bit. Roger Staehle from the Nevada program.

That's a perfectly rational approach to doing this kind of work. The problem here is is a matter of figuring out what the problem actually is. And as an example, Jeff and Ron in their program, have looked at the question of lead. It would be a little bit difficult to explain how undefined the system actually is from the point of view of cracks. And before you do some kind of a serious multivariable kind of experiment, you've got to in fact figure out what the multivariables are, and that's sort of step one. That's a big step.

And the other part of the multivariable is the question of what the environments actually are, or can be, or can be bounded to be. And so then another part of this thing has to do with the fact that you not only have to deal with the multivariable effect, but you've got to deal with the dispersion effect, and you're interested in the first failure and the second failure and the third failure, and that's also something that needs to be thought about. And I'm not so sure it's part of the Nevada program to figure out the dispersion problem, and maybe even the multivariable problem.

I sort of see the effect of the work here as being identifying the bounding conditions, and what it looks like, and maybe someone with more money can do a bigger study.

DIODATO: With one thing being possibly maybe total loss of mass in the system looked at and another time to incipient formation of these first cracks; are these the kind of variables that you look at for response variables?

STAEHLE: Sort of. The problem here is not a mass loss problem. With C-22 and this kind of a system, it's not a matter of losing mass. Like, for example, if you use iron or an iron base material, you're looking at mass loss because you've got this general problem of instability. Whereas, in the C-22 with the alloys you've got, you're really looking at cracking processes, local processes, and so it's not a mass loss problem.

PARIZEK: Paul Craig, Board?

CRAIG: Craig, Board. You mentioned, Dr. Gorman I believe mentioned the importance of scratches and gouges and how these can sometimes accelerate deterioration, and also small impurities in the materials. And, of course, many of these for the DOE design, these materials will have to be manufactured in large quantities over quite a period of time, so quality control will matter. I wonder if you could give us some insight as to how to think about that kind of problem from the perspective of a scoping study.

GORMAN: First, let me refresh myself and you as to why I was saying this. For example, in Inconel 600 tubes, we find that the tubes in some cases when they were inserted had lines and abrasion or scratches along the surfaces. The failure analysis I happen to be working on right now, we find that the cracks tend to occur preferentially along that line of disturbed material, and while the rest of the material that have an attack of only a grain or so, saying 1 mill deep in the regular surface, it will be 15 to 20 mills deep, half the tube wall, along a line of scratching. So the scratch seems to have residual stresses and cold work, both of which seem to aggravate many of these degradation processes.

Grinding on stainless steel puts in residual stresses and also phase change, and both of those processes accelerate cracking of stainless steels. And then in the trace impurities--

CRAIG: Well, we're concerned here about, one, will be the welds, and the second aspect will be rocks that might fall on the canisters.

GORMAN: Right, or other installation damage. And so I think the testing ought to include tests of material with representative surface damage. And so you try and isolate to determine how much effect it has on either pitting or most likely cracking, and the welding, you ought to have welds in this test qualification program.

The trace impurities in the material is a harder one actually to address, because these impurities such as boron and other interstitials are not normally measured in the material, and they're not quantified, yet we have found in Inconel 600, for example, that boron in the material greatly accelerates susceptibility to caustic cracking. And so there has to be some systematic testing done to try and identify what are the trace impurities and what effect do they have, and you have to use an accelerated environment to do that test, and then hope that that tells you it can be safely extrapolated to lower temperatures.

So it would be part of a spring test either done with electrochemical methods or with methods such as constant ascension rate tests to get results in a six month or one year time frame.

PARIZEK: I want to thank you for your contributions this morning, and hope that you'll be available throughout the day, and perhaps in the public comment period. And it's been an important topic, so we've let it run beyond the coffee break time, so can we assemble here at 11:00 sharp?

Okay, so those of you who must leave for a few minutes, do so, because we really have volcanism and we have the whole scientific program update. Mark Peters is our next presenter from the DOE program, a review of all the scientific studies ongoing, which is also a very important topic, and then we'll follow that with the volcanism discussion.

The next presentation is our scientific and engineering testing update, an important presentation, followed by another one on volcanism, so we must get started.

Mark?

PETERS: Thanks very much. Can you hear me okay?

I guess I'll start off with half the room, and then it will fill up as we go.

Thank you very much for having me again today. It's nice to have the opportunity to talk to you all again.

Again, just following through on previous presentations that I've given to the Board, this is just another volume of the scientific and engineering testing update to give you a feel for where we're at in the testing program across the project, cover a lot of ground, as I normally do, try to give you a feel for what we're doing in all the areas, and then hopefully prompt questions. We'll hear a lot more about the details of the modeling in later presentations in the TSPA.

I've already given you the objective, provide the status of the testing program in support of the process models and design. I'll start with update of some of the ESF studies, the drift scale tests, as well as an update on where we're at with Chlorine 36 validation work. You've heard an extensive presentation on that at the June meeting. I'll just give you an update on where we're at with that.

Moving into the cross drift, I'll talk about results from the seepage studies at Niche 5 in the lower lithophysal, the Topopah Spring, and then some additional work that Lawrence Berkeley is conducting in the lower lithophysal, looking at systematic hydrologic properties, a brief update on where we're at with the bulkhead investigations, an update on where we're at with the Phase II testing at Busted Butte, unsaturate zone transport test, some new discussion of where we're at with the alluvial testing complex work in cooperation with Nye County, and then switch gears into the engineered barrier system, an overview of the pilot-scale testing at Atlas in North Las Vegas, where we're headed with ventilation testing also at the Atlas facility, and then a very high level overview of the materials testing, waste package materials testing, and then wrap up with a brief summary.

Starting with the ESF and the cross-drift studies. You've seen this many times before. The ESF here, north ramp, main drift, and south ramp, the potential repository block to the west. This is north in this direction on this diagram. So the potential repository block with the cross-drift in red going out over top of the block.

Today in the ESF section of the discussion, I'll focus on some results from the drift scale test, and also a little bit about Chlorine 36 validation from samples up here near the Drillhole Wash Fault, and also taken down here near the Sundance Fault.

Starting with the drift scale test, I'm sure you're all familiar now with this diagram and layout of the test, the observation drift with the connection drift, and then the heated drift with the boreholes drilled from the observative drift, both above and below the heated drift, as well as boreholes drilled within the heated drift itself, primarily to get temperature and mechanical changes in the rock.

In terms of an update of where we're at with power and temperature, I mentioned at the last meeting that we had gone through one power ramp-down. We were approaching 200 degrees C. at the drift wall, which was our target. So we've ramped down the power now once again, so you can see the changes in power here, time and days versus power in kilowatts on this axis, temperature and degrees C. on the right-hand axis. The temperature of the drift wall, both at the crown on the left rib and the right rib are also plotted here, and show how we've converged on 200 degrees C., and we're not flattening off and we're going to maintain. We're in about two and a half years into the heating phase, plan to heat for the full four years.

I'd like to talk a little bit about some of the gas chemistry and water chemistry work that we've been doing, analyzing gas, collecting gas and water from some of the holes drilled off the observative drift, and also compare that to some of our modeling that we've done with active transport modeling, pretest predictions, and then evolving predictions through the test.

First gas chemistry. What I've got plotted here is time from heating versus CO2 in parts per million by volume. The solid are measurements of CO2 concentration for the borehole, Borehole 75, which is a borehole drilled up from the axis drift above the heated drift. So we've got CO2 measurements versus a model calculation for CO2 concentration in the fractures as a function of time. It's a dual permeability simulation, and again, since it's due to permeability, you have predictions for matrix CO2 contents and fracture CO2 contents.

But you can see we expected to see a rise, and we've talked before about the CO2, I'll call it a halo, the increased CO2 concentrations in advance of the boiling front. And then once the boiling front passes, we would tend to see the CO_2 concentrations reduce. That's predicted by the models, and we're starting to see that behavior in the measurements as well.

CO_2 concentration ties, of course, heavily to water chemistry, particularly controls of pH. This is data on pH measurements on water sampled from three different intervals. Borehole 60 is a down borehole, so this was sampled below the heated drift, or to the side and below the heated drift. And 186, Borehole 186, is also a down borehole, so to the side of the heated drift, below and to the side.

Again, a series of pH measurements. The pH, this is a model prediction for that region of the heated area, showing a decrease in pH, and then a subsequent rise in pH, and we're in fact seeing that kind of systematic behavior in the pH measurements that we're getting from the water sample.

Chlorine 36 validation, I mentioned at the beginning that you heard a lot about that at the June meeting. Mark Caffee from Lawrence Livermore gave a presentation on that, and June Fabryka-Martin was also there, added some during the discussion.

We're all familiar with what we're doing there, but what we're after is validating the occurrence of bomb-pulse Chlorine 36. From the original work that was done by June, she identified bomb-pulse locations in several places in the ESF. We chose two of those, the Sundance Fault and the

Drillhole Wash Fault, and the USGS, in cooperation with Livermore and Los Alamos, has led a study to try to validate those occurrences.

In terms of the path forward on that, you heard a lot about discussion of the data, and there's some interesting differences in the previous data collected versus what Livermore is now collecting. So we've developed a path forward to try to get at understanding why there's those differences. So the USGS has prepared a reference sample. We took some rock from the tunnel. We've crushed it, homogenized it, and we're distributing aliquots to Livermore and Los Alamos. That has been done.

Livermore and Los Alamos are now in the process of documenting how they plan to test the effective different leaching procedures. We don't think it has to do with the sampling. It's more of the laboratory, how they're leaching the chloride from the rock for subsequent analysis. So we're going to go through a process of comparing leaching procedures, doing different leaching techniques, and then swapping samples basically to try to get an inter-laboratory comparison to try to address the differences. So these two steps are really ongoing as we speak.

Once we've agreed as a team on the standard processing method, we'll apply that to a separate aliquot of the reference sample, and then to the additional validation samples. I should also mention the USGS continues to extract water and conduct tritium analyses, and they've got close to 38 analyses right now of tritium from the Sundance fault, and all of them, except for one, have shown no evidence of any tritium, and that one is in the order of 2 1/2 to 3 tritium units. So it's still below bomb-pulse levels. So we still see no evidence of bomb-pulse tritium at the Sundance.

And then at any rate, that's the results. The last bullet here just reminds everybody that once we're done analyzing the validation samples, we'll synthesize the results and prepare a report.

Switching gears to the cross-drift, this is a diagram you've seen before, the cross-drift here cutting across over the top of the potential repository block. Different units within the Topopah Spring are marked, as I've done in previous meetings. The upper lithophysal exposed up to here. The middle non-lithophysal in this section of tunnel. The lower lithophysal through here, and the lower non-lith exposed from this part of the tunnel all the way to the Solitario Canyon Fault.

Color coding again, the stuff in black is existing excavations that I would have tests ongoing, or we're in the process of finishing up test construction. And those in Italics and blue are things that are in the baseline plan for out years.

I'll talk today about work Berkeley has been conducting at Niche 5, seepage testing in the lower lithophysal. I'll also talk about some systematic work that Berkeley is doing within the lower lith in this section of the cross-drift, and then a brief update on the bulkheads, remind you that we have bulkheads constructed at about 1750 meters from the entrance to the cross-drift, another one just before the Solitario Canyon Fault, and as of today, we should be getting ready to close up a third bulkhead that we've stuck down right at the back of the tunnel boring machine to try to minimize the effects of the tunnel boring machine, the heat produced from the tunnel boring machine.

First, Niche 5, again, after drift-scale seepage processes, Niches 1 through 4 in the ESF were in the middle non-lithophysal, the upper part of the repository. Here, we're in the lower lithophysal. In terms of status, excavation of the niche is complete. They've characterized the flow paths using dye released prior to niche excavation, and then looking for the dye as excavation proceeded.

Air permeability tests for the post-excavation boreholes are in progress, very close to finished. We've got a bulkhead installed so that when the seepage tests begin, we'll be at ambient humidity conditions as we're dripping into the opening.

Reminder of what Niche 5 looks like. There's an access drift that was excavated, then a series of boreholes, pre-niche excavation boreholes were drilled. This is where liquid dye was released, and then while we were excavating the niche itself, we looked for where the dye travelled along pathways within the lower lithophysal. And then we've also drilled post-excavation boreholes, and that's where some of the additional air permeability testing is taking place.

The liquid release seepage testing will take place from some of these same boreholes here that are drilled above the niche itself.

I'll talk about comparison, what we've seem preliminarily in Niche 5. Again, we're in the lower lithophysal and we're comparing what we've seen in Niches 1 through 4 in the ESF in the middle non-lithophysal. Pictures on the right are meant to illustrate some of the points that I'm going to be making here in the bullets. The picture on the left, you can see purple dye gathered around the borehole. This is from Niche 5 within the lower lithophysal. The picture on the right, you can see a trace of blue dye. You should be able to pick up a trace of blue dye that travelled along through-going fracture, with the scale here. This is on the order of a meter. This is in the middle non-lithophysal.

So there is a contract in the behavior of the liquid dye travel in the two units. Evidence in the lower lithophysal of stronger capillarity. Also, the air permeability tests that we've done at Niche 5 suggest higher permeability. When you put those two things together and compare them to what we saw in the middle non-lith, it's potentially a higher seepage threshold in the lower lithophysal than in the middle non-lithophysal. That's positive for performance. Remember, the lower lithophysal is on the order of 70 or greater per cent of the potential repository horizon, so it equals less seepage into drifts in the lower lith, based on these preliminary results.

I believe it was two meetings ago, there was also discussion about the evidence that calcite lithophysal cavities, and what that might say about seepage. One of the things that was seen by Lawrence Berkeley as we were excavating the niche after release of the liquid dye, this was a borehole where there was dye released, and you can see that the dye actually by capillary forces actually flowed up and coated the bottom of a lithophysal cavity.

So the strong capillary forces in the lower lithophysal could be possible alternative explanation for calcite in the bottom of lithophysal cavities, at least in the lower lithophysal.

And I should also mention that as we were looking at the lithophysal cavities during excavation, we didn't see spots. We could see just dripping into those cavities. We saw evidence of coating along the bottom of the lithophysal.

Switching gears, Berkeley is also conducting, we're drilling a series of boreholes in the lower lithophysal to look at Niche 5 as one location in the lower lith. We're interested in heterogeneity and the variability of rock properties, fracture properties, throughout the lower lithophysal. It is a heterogeneous unit, so we're drilling a series of boreholes, both off to the side of the drift, as well as up and at low angles in the crown of the drift.

This is just a schematic to kind of show you that systematically, we're drilling these boreholes. We're doing it from the top of the lower lith, all the way down to the first bulkhead right now, and we've conducted some preliminary testing in some of those boreholes, and I'll talk about those in the next slide.

One borehole in particular is a low angle borehole drilled from the crown of the drift, and then it's packed off at different zones, and the second area picked up the distance from those packed zones to the crown of the drift, so on the order of 1 1/2 to a little bit more than 4 meters. We then do liquid release tests. We do air K tests in those holes, and also do liquid release tests, so borehole seepage measurements, both high rate tests as well as low rate tests.

Yvonne Tsang, the principal investigator from Lawrence Berkeley for these tests, shows the set-up in the cross-drift, the gas cylinders for air injection, and the data collection system. And, again, she's working here on a set-up that's set up in a borehole that's drilled from the crown at an angle off in that direction.

I showed part of this diagram at the last meeting. This is an update on the air permeability measurements that we've been getting in Niche 5, and also some preliminary stuff from the systematic work that I'm discussing right now. This is a plot log of permeability, air permeability versus basically location. For Niches 1 through 4 in the ESF, you saw a mean air K for three different boreholes on the order of a little less than 10 to the minus 13 in meters squared, and a range over about two orders of magnitude. The results from Niche 5 in the cross-drift suggest that the air permeabilities are greater by as much as an order of magnitude, which still show a significant range.

Very preliminary results from one of the boreholes in the systematic work suggests again the lower lith seems to exhibit relatively high air permeabilities, relative to what we saw in the middle non-lithophysal in the ESF, a much smaller range, but that's a very limited dataset at this point, bearing out these higher permeabilities that were seen in the lower lith versus the middle non-lith.

Bulkhead. Again, we bulkheaded off the back half of the cross-drift, no ventilation, looking for drips basically, looking for seepage, looking for return of the rock to ambient conditions, rewetting.

If you remember, there was condensation observed back earlier this calendar year, and we attributed a lot of that to the influence of the tunnel boring machine still being under power, and it was producing a thermal gradient. So we've gone in and put a third bulkhead with insulation just behind the tunnel boring machine to try to minimize those impacts. We've also rewired the lights so that we can turn the lights off while we're not in there, which was another contribution of heat. And we've also installed additional instrumentation. The USGS has put in additional temperature sensors. We're looking at wind speed. And also we've got drip cloths installed at certain locations where we think we might see drips. But as of right now, there's still no apparent evidence of any seepage into the back half of the cross-drift.

Moving out of the cross-drift in the repository horizon to the bottom of the Topopah, the Calico Hills, transport in the unsaturated zone below the potential repository. As you recall, we're doing an unsaturated zone transport test at the Busted Butte facility, which is actually southeast of Yucca Mountain, exposed at Busted Butte. We have a shallow excavation portal where we've exposed the bottom of the Topopah Springs and the top of the

Calico Hills formation, and we're doing unsaturated transport tests.

Purpose is to evaluate the influence of heterogeneities, look at fracture/matrix interactions, and interactions, permeability contrasts across lithologic contacts, consider colloid migration in the unsaturated zone. Scale or laboratory sorption measurements to field scale, calibrate and validate the UZ flow and transport model, and again, get at scaling issues.

I'll show another diagram that gives more detail of the test block, so I won't dwell on it in this particular diagram.

Reminder, the test was broken up into two phases. I'll focus today on Phase II. But this just gives you an idea of the suite of tracers that we're using in the transport tests. Phase I, we used a whole series of conservative tracers, included fluorescent dye, also microspheres for colloid analogs, and as well as lithium bromide as a reactive tracer.

For Phase II, we added these tracers as well, some of the transition metals meant to be analoged for some of the radionuclides from our perspective.

I'm going to talk again about Phase II today. This is a more detailed diagram of the test block. The main adit comes down here. The test alcove breaks off to the right-hand side. You're looking at three units, the upper fractured vitrophere, which is the basal part of the Topopah Springs, an unfractured vitrophere at the bottom of the lithologic Topopah Springs, which is part of the hydrologic Calico Hills, and then the true bedded Calico Hills. So we're looking at hydrologic Calico Hills in these two units, and hydrologic Topopah Springs in the upper unit, this being fractured, this being relatively unfractured, and this being a bedded tuff, bedded Calico Hills.

Phase I, you've heard about in previous meetings, so I won't dwell on Phase I. I'm going to give you an update on where we're at with Phase II. Phase II is this larger test block here. We have a series, two planes of injection holes, one upper plane and one lower plane, and some of the basic statistics on the injection rates in the different holes are shown here in this box. Then we also have a plane coming off the main adit of collection boreholes, two planes of those as well, one for the upper injection and one for the lower injection.

We're doing geophysics, so some of the symbols show where some of the--all these collection holes have liner systems where we can pull the liners periodically, collect collection pads and analyze those in the laboratory. Those holes are also used for running radar and neutron logging for looking at movement of the moisture front. Each of the injection holes has on the order of ten injection points per borehole.

What I'm going to do is I'm going to get fancy and go multi-media on you here. Just as I'm going through some of the results, it's hopefully clear, I'll leave this diagram up so that you can refer to the borehole numbers as I'm going through.

Phase II is ongoing. It's been running for almost two years, and we'll run through the fiscal year. So far, we've collected nearly 15,000 pads, well over that by now. Each borehole is harvested for its pads every other week, on the order of every other week. So we're collecting a huge amount of pads. Of those pads we collect, we take a subset of those and do analyses of the conservative tracers, as well as looking for reactive tracers. And then as I mentioned, we're doing multiple geophysical logging runs, and that's to get at the movement of the moisture front, and also helps guide us is where we're pulling pads at different times during the test.

We've seen breakthrough of non-reactive tracers at all of the boreholes except for 10, 11 and 47. So all the boreholes have really seen breakthrough of non-reactive tracers, conservative tracers. Lithium is one of our key reactive tracers. We've seen breakthrough of lithium at several boreholes. But as of right now, we've seen no breakthrough of any of the transition metals.

We're collecting a lot of data on pads, looking for travel of the tracers, and we're also doing some test scale modeling that's used in conjunction with the site scale modeling for confidence building validation exercises.

The test scale modeling that's been done to date for Phase II involves three hydrologic layers, with no faults, and right now, they're really concentrating on trying to match the conservative tracer results, ongoing modeling, which I won't talk about today, but we're starting to look at the reactive tracer results. But, again, we've really only seen lithium breakthrough. We haven't seen breakthrough of the other reactive tracers. In general, the model shows a good match of characteristics, and in some, it's actually a very excellent quantitative match.

We could probably do a better job of having the model match the measurements by simply incorporating even more accurate geology and accounting for dispersion and heterogeneity in the system, more than we have in this relatively simpler modeling that we're doing today.

This is just an example of what we've done in terms of predictions. What you're looking at here is concentration normalized, initial concentration versus measured concentration versus distance along a given borehole. This is for Borehole 16, which is this borehole up here. And what you're seeing is a function of time. These are four different time slices, earliest, progressing to most recent. And you can see in the solid, is the actual measurements, and I believe in--no, excuse me, the solid is actually the simulation, and the dotted line are the actual measurements.

So in terms of accounting for conservative tracer porous flow, we do a nice job of matching the observations for the conservative tracers.

Switching now to Nye County, work we're doing cooperatively with Nye County. Nye County's Early Warning Drilling Program, we're heard that discussed a lot this morning in the context of Nick's passing. This is just a reminder that the project is working cooperatively with Nye County. We are collecting data from the boreholes that Nye County is drilling, and trying to get as much information as we can as well out of this program.

Some of the data that we're using that we're collecting, and also using that Nye County is collecting for the SZ flow and transport model is listed here in bullets. I'm going to focus today on where we're headed with the alluvial testing complex, where Nye County has drilled a well and plans to drill additional wells, where the project will conduct tracer testing in the alluvium in the saturated zones downgradient of Yucca Mountain.

The borehole I'll be referring to is right here, 19D. It sits just off the edge, just southwest of the Nevada Test Site, and that well is completed and there's some single hole testing ongoing that I'll present some preliminary results from.

Again, Nye County has completed drilling 19D/D1. There was alluvium from the ground surface down to just over 800 feet. Static water level was at about 360 feet. The borehole also penetrated volcanic tuffs over about 400 feet, and then was terminated in a section of tertiary sedimentary rocks. That was one of the criteria that we had for this borehole to be the likely borehole to be the start of the multi-hole complex that we call the alluvial tracer complex.

Nye County has done some flow surveys, and also a 48-hour open-hole hydraulic test of the entire borehole. So all three sections. It's not my place to talk about Nye County's results, only in the context of what we found in our pump tests, and I'll get to that.

We've conducted also an open-hole hydraulic test, but here we only isolated the alluvium. So Nye County was looking at the entire borehole. We looked at just the alluvial aquifer. It was seven days of pumping, seven days recovery. It just finished really a couple of weeks ago.

We were monitoring distant and nearby wells. It pumped at 150 gallons per minute. There was 100 feet of drawdown. I made a mistake in the parentheses here. That "less" should be a "more." That's a big mistake. Our tests, there was more drawdown than the Nye County open-hole test. So when we isolated just the alluvial aquifer, this shouldn't be a surprise, because they were seeing contribution from the other sections. But there was more drawdown in our test by almost an order of magnitude.

The plans from here, we have four intervals identified in 19D where we're going to do isolated interval hydraulic testing. That's in the process of being fielded. That's going to take place in this late fiscal year. And current plans for next year would be to take those isolated intervals and also do tracer testing where we would inject push-pull. We would inject the tracer suite, and then immediately pump it back out to look for transport properties of those same alluvial intervals. And this would be with that single hole, and then the Phase III of Nye County has, at least the current plans would have additional holes drilled that would make up the multi-well complex. But that's all planned. I should emphasize that's planned work.

Switching from the natural system to the engineered, the pilot scale testing focusing on the work that's being done at the Atlas facility in North Las Vegas, here you've heard about this before, but we're evaluating various EBS configurations and providing data in support of the EBS process models.

I won't dwell on this slide in the interest of time, but you've heard a lot about the test canisters testing that we've done at the Atlas facility, pilot scale testing, quarter scale testing. Canisters 1 through 3, you've heard results of before. I'd remind you that the first canister was a Richard's Barrier type setup. Second canister was a backfill setup, straight sand backfill, with Canister 3 being a drip shield with a mock waste package heated, with no backfill. Results are kind of summarized in the bottom of each of the slides.

I should say that Canister 3 with the drip shield, the results of that, the drip shield effectively protected the mock waste package from drips. We didn't see any condensation underneath the drip shield.

Now, on to Canister 4. Canister 4 was a drip shield with backfill. Again, similar conditions in terms of the waste package surface, the mock waste package surface was at 80 degree C. The surface of the test cell was maintained at 60 degrees C., same as Canister 3. But here, we added backfill, Overton sand. Similar results in the sense that the drip shield still effectively shields the waste package from drips, the mock waste package from drips, and it creates an environment that is warmer. So we actually saw gradients in relative humidity, and also we saw no drips again, no condensation on the inner surface of the drip shield. That test was completed in early May, and they're in the process of comparing that to model results and preparing a report on that.

Now moving into the planned arena, the ventilation test. This is a test that's being planned for next year. Again, it's being planned to provide data for validating our preclosure ventilation model. As we talk a lot about design and evolution of our design, our design is currently relying on ventilation during preclosure. We have codes, models, and models associated with that that we use to do that ventilation modeling. This test is planned to validate that model.

This is again over at North Las Vegas in the Atlas facility. You'd have a simulated emplacement drift with simulated waste packages, about 25. This would be about .35 kilowatts per meter of power output, and the surface of the mock waste packages would be 200 degrees C. So what you're looking at is a long pipe where we would have mock waste packages, and we would ventilate that, and then compare that to model predictions. This is the intake air velocity, and we would maintain the maximum temperature of the crown at boiling.

This is, again, planned work. The first phase, which is in the process of being fielded, should start, if it hasn't started today, it will start likely this week, would be where we would heat the waste packages and intake ambient air, and ventilate that long pipe with ambient air.

The next phase would be to use conditioned air. We would recirculate the air and continually suck the air into the intake. We would recirculate the air. These other two phases are in the planning stages and may or may not happen next year, depending upon budget constraints. But we would wrap up whatever work we do do this year, and next year would be wrapped up in fiscal year '01 into a nice package. But, again, this is all planned. The next meeting, we should have some preliminary results from Phase I.

Moving to waste package materials, this is going to be very high level. This is just a reminder we have the corrosion testing facility at Lawrence Livermore. There's been long-term tests underway there for longer than two years. We have a range of conditions, immersed G ponds, water line, vapor corrosion. We're looking at general and localized corrosion mechanisms over a wide range of conditions.

We're looking at a lot of different materials, corrosion allowance materials, corrosion resistant materials, different geometries. You heard some discussion of geometries in the previous presentation, U-bend specimens, crevice specimens.

Our test conditions over a wide range of temperature, ionic strength of the water, as well as pH, a very wide range. We feel like we've bounded the conditions that we would expect to see.

The long-term tests are evaluated for weight loss as well as the presence of crevice or any other localized corrosion. For things such as Alloy 22 and the titanium alloys where we're looking at following corrosion processes that tend to occur more slowly, or look for passive film stability, we're using standard microscopic techniques and also using some more detailed microscopy, Atomic Force Microscopy to look at, in particular, passive film stability. This is a program that you've heard about before. It's ongoing and will continue.

So to wrap up, I gave you hopefully a pretty comprehensive but very quick overview of where we're at with the testing program in the ESF, the cross-drift, Busted Butte, and then also in the engineered area at Atlas and also in the corrosion test facility at Lawrence Livermore.

We continue to try to address the key processes in the natural and engineered systems. And the data collected and analyzed that results from the investigations that I'm discussing will be reported in technical update documents. So there was a lot of discussion about how these results will be incorporated in SR. These results will be incorporated into technical update documents that will be made available as the different entities are reviewing the site recommendation consideration report. And if we see things that are considered impactive to what we've assumed in the SRCR, we will do impact analyses and incorporate those results into the SR as appropriate, all those results being made available to the public.

PARIZEK: Thank you, Mark, for very concise information as always. Questions from the Board? Debra Knopman?

KNOPMAN: Knopman, Board. Mark, would you comment on one of the conclusions of the previous presentation about the inadequacy or the incompleteness of the range of conditions that you're looking at for the corrosion resistant waste package material for the C-22?

PETERS: In terms of the water chemistry, et cetera?

KNOPMAN: Yes.

PETERS: Well, we feel constraining the environment, as you well know, that's an area of large uncertainty, and I won't sit here and say it's not. We feel like we were doing experiments over a wide range of temperatures. There's people who can elaborate in the audience, but 10, 100, 1000 times concentration of J-13, concentrated solutions. The range of pH is--I mean, it's a very large range. We feel like we've bounded the conditions.

KNOPMAN: What about the trace metal issue?

PETERS: I have some experience in analysis of lead.

You want to be real careful about looking at lead. Lead analysis is highly subject to contamination. So we would need to look very carefully at those analyses, particularly the hole lock analyses, before we can really say much about how much lead, for example, you might get in a drip that would come into the drift. The project needs to address those results, I don't deny that. But one needs to be very careful. We need to really take a careful look at what the trace metal concentration will be in the incoming water.

PARIZEK: Don Runnells?

LINGENFELTER: Al Lingenfelter at Lawrence Livermore. The test water in the long-term corrosion facility was prepared with de-ionized water. It's to mock the J-13. It is not J-13 water. So it may or may not have lead contamination in it. At a couple parts per billion, which is what J-13 has, it could be that high, but we've never analyzed it. We will after the question was raised, and we do, I believe, have some drip tests of J-13 on the hot surface which I will try to find those results today. But we will also have some way of arguing what concentration we're getting in build-up of the salts. So there are some tests going that could answer some of these issues in the fairly short-term.

PARIZEK: Don Runnells?

RUNNELLS: That answer and yours also, Mark, pretty well covered what I wanted to ask. But let me just finish it off by asking if you're analyzing the water from the drift scale heater test for any of these trace metals that we heard about earlier.

PETERS: Actually, Zell Peterman and I just talked about that in the back of the room actually. We're going to go out and thinking about trying to analyze for lead. We are not as of yet. Because of the contamination problems, that's a hard sample to collect, but we're going to look into trying to analyze for lead, for example. We're looking at strontium and uranium, because we're also doing isotopic analyses in those systems. But we're going to think about analyzing for some trace metals in the drift scale test waters to try to hopefully address that.

PARIZEK: Priscilla Nelson?

NELSON: Nelson, Board. Mark, a couple of questions. First, when do you think that Chlorine 36 test series will be concluded?

PETERS: It will be next year.

NELSON: Next fiscal year?

PETERS: Yes.

NELSON: Next calendar year or this calendar year?

PETERS: I don't know exactly, to be honest with you. It will certainly be next fiscal year. We should get a lot of information by the end of the calendar year, but I wouldn't want to commit that we'd have a report wrapped up with a bow around it.

NELSON: Okay. And a separate question, when you showed the contrast between the lith and the non-lith and the dye test, was that expected, predicted by the models that you have for those materials' performance?

PETERS: Well, let me answer it, I'll come at it maybe not from the model perspective, but as people from the geologic perspective are looking at the rock, the lower lith has a lot of fractures that terminate in the lithophysae. There were those who were surprised that the capillary to the matrix was so strong in the lower lith. So I would say that it was probably a surprise to some extent, the strength of the capillary forces in the lower lith. But the nature of the fracturing would suggest that you would get--the middle non-lith has longer throughgoing fractures, so you'd expect there to be longer pathways than you'd see in the lower lithophysal.

NELSON: Okay. Let me just follow up on that because my understanding of the idea of a capillary barrier would be that the capillary barrier would actually act against such strong forces that bring water in the capillaries to an opening such as the lithophysae. It would deflect water from moving towards an opening such as the tunnel on a larger scale. So I'm wondering about the water moving through the pores capillarity going to the lithophysae. Is that inconsistent in your mind relative to the fact that you expect that capillary area to shed water or deflect water on the openings?

PETERS: I'm probably not the right guy to answer that, to be honest with you.

NELSON: Okay. Talk to me about that.

PARIZEK: Paul Craig? We'll probably get ahold of Bo Bodvarsson to understand the physics of this. Paul Craig?

CRAIG: Craig, Board. Chlorine 36, at the last Board meeting, it was clear that the two groups were not talking to each other very much. Are they now talking to each other? If so, are there any new ideas as to what's going on? And if they're not talking to each other, why not?

PETERS: They're talking to each other. The answer to your first question is yes, they are. We're making sure that's happening.

Like I said, they're going to do a series of experiments on that reference sample to get at leaching techniques. We think it's in the preparation in the laboratory, the leaching, so Livermore and Los Alamos are working with the Survey to come together on how to go about doing those leaching experiments. Once we do that, then we'll be able to say a lot more. But we're honing in on the laboratory preparation of the samples as opposed to how they were taken in the field. There's no additional data really that sheds any new light than what you saw in June. There's still a difference.

CRAIG: And no new ideas?

PETERS: Well, again, we're focusing in on the leaching. I mean, it probably has to do with how much rock chloride-- it might have to do with how much rock chloride you're releasing that will tend to dilute the Chlorine 36.

PARIZEK: Alberto?

SAGÜÉS: Yes, thank you. I'm looking at the last couple of experiments, I guess, and I'm looking at the engineered barrier system testing of waste package materials. Do I understand that most of the testing or research is now limited to what has been done with the immersion, like for example, are there additional experiments being conducted on the stress corrosion cracking?

PETERS: Yes, there are. There are additional experiments on stress corrosion cracking and other areas. I probably should have added bullets to that effect. But you'll here, I think, even more about that. Pasu will talk quite a bit about the basis for the waste package drip shield degradation models this afternoon, and you'll hear probably more about that data in his presentation. That is ongoing as well.

SAGÜÉS: I see. Is any work planned on investigating fundamental issues of stability of passivity on the alloys considered for the waste package?

PETERS: Yes, that gets at I believe the Atomic Force Microscopy work where they're looking at passive film.

SAGÜÉS: That's examination of the cupons from passivity. But what I'm saying is is there any investigation aimed to address fundamental issues? Because that's simply characterization of microstructure.

PETERS: Al can speak to it.

LINGENFELTER: Al Lingenfelter, Lawrence Livermore. There's, let's say, a proposal dependent on funding to look at the passive film stability. We have Larry Kaufman from MIT lined up to put together a theoretical diagram based on thermodynamic calculations, and we also would hope to be able to analyze the films at different points in the voltrometry curve, and in that way, see if we can get experimental data that has any agreement with the theoretical.

We also have the Josephson on the list, and it, again, all of this is dependent on where the funding ends up. There are a whole suite of activities to look at phase stability and stress corrosion cracking, as Mark has said.

SAGÜÉS: But those are planned or proposed, but not necessarily supported?

LINGENFELTER: That's currently correct, yes. But this process is ongoing. We're in the middle of the budget process. So for me to argue one way or the other would be silly.

SAGÜÉS: I see. But activities which are budgeted are the continuation of the long-term, the cupon tests; is that correct?

LINGENFELTER: We have no plans to discontinue the long-term facility tests; that's correct. They will continue.

SAGÜÉS: Those are supported? Those are funded?

LINGENFELTER: Yes.

SAGÜÉS: Okay. All right.

PARIZEK: Debra Knopman, Board.

KNOPMAN: Mark, based on the results so far with the tests in the ECRB, Busted Butte, the drift scale heater test, how would you order your priorities if for some reason you had to cut back on some of this testing? What seems to you the most promising? What has the biggest bang for the buck in terms of additional scientific insight that will measurably affect performance among all the seepage tests, all of these activities that are currently going on?

PETERS: This is Mark Peters. I would say that the hydrologic characterization work in the lower lith and the cross-drift, in my opinion, is the most important. The work that's ongoing in there that we're just finishing up construction on, that would be where I would say the bang for the buck is.

COHON: It sure is lucky that they have a cross-drift.

PETERS: That's just for the natural system, Debra.

KNOPMAN: Right. Do you feel like you're into diminishing returns in terms of the Busted Butte results at this point?

PETERS: I think, yeah, we've learned, we've really validated a lot of assumptions that we had already made about flow in the Calico Hills. So, I mean, we're really looking to wrap that test up. We've done some good model validation there, but that test I think needs to be wrapped up, and we need to focus more on the cross-drift.

PARIZEK: That side-bar remark was from Chairman Cohon.

Mark, this is Parizek, Board, there were no drips observed when you were putting in the third bulkhead; correct?

PETERS: We haven't seen any evidence of any drips since, really since I talked to you all in May, and you went in there in May. We had them closed back up for on the order of close to two months, and we still haven't seen any evidence of drips.

PARIZEK: And Bo Bodvarsson will explain this wicking effect, how water goes from small openings to big openings, we hope, the physics of it, sometime in the day?

PETERS: He's definitely the right guy.

PARIZEK: Now, in terms of Busted Butte colloid experiments, you have 15,000 pads collected.

PETERS: Yes.

PARIZEK: Part of that was to look for colloids that you put in in terms of microspheres to see if they in fact move through the Calico Hills. How is that coming along? Because obviously colloid migration is critical in your modeling, it's been mentioned a number of places.

PETERS: Right.

PARIZEK: And have you found microspheres?

PETERS: We were having some problems with actually convincing ourselves that we were really analyzing colloids or not on the pads. So the Los Alamos folks are in the process of doing a series of calibration experiments. They've done some saturated column experiments with pads, and they've convinced themselves that they can collect colloids and analyze them there, and they're finishing up on saturate column experiments. Those, if successful, and they look good, we think we can then go take some of those pads and then start to analyze the colloids.

PARIZEK: That's an important part of the Busted Butte experiment still ongoing and needs critical analysis?

PETERS: That continues to be the one performance assessment, one of the first things they'll always say to us, is get at the colloids if you can. So we've spent a lot of effort on trying to get that protocol in place.

PARIZEK: Questions from Board advisors?

KESSLER: John Kessler, EPRI. On the tritium breakthrough tests, you didn't mention whether that was an expected result. Given that there may be Chlorine 36 coming through, but not tritium, is that something that was expected? What's the mechanism?

PETERS: Well, remember the way tritium is analyzed. Let's start with Chlorine 36. Chlorine 36, you're leaching the rock and really dissolving stuff, evaporated chloride from past water flow. Tritium, they're actually using a centrifuge and extracting water. It's probably mainly matrix water. So there could be disequilibrium between the fractures and the matrix and you're just not seeing it. So it's still early to be able to tell really what that means, but we were intending to look for other evidence of bomb-pulse to try to validate again the occurrence of bomb-pulse. And taken at face value, you don't see any evidence.

PARIZEK: Questions from staff?

(No response.)

PARIZEK: Okay, that's it, Mark, thank you so much. We've run deep into the next topic, volcanism and volcanic hazards by Frank Perry and Kevin Coppersmith. I won't introduce them because I think we really are tight. We'll run late for lunch, and we'll nevertheless cover their presentation before lunch.

PERRY: Good morning. I'm assuming you can hear okay.

I'm going to give a broad overview of the volcanics around Yucca Mountain, and also the history of volcanism studies which have gone on for 15 or 20 plus years, depending exactly on how you count.

It's interesting to be here. The last time I presented to the Board was early 1994, so it's been a long time, and the only familiar faces are Bill Nelson, Leon Reiter. And as an aside, about the time volcanism studies were beginning, Rod Ewing was my mineralogy professor.

So the purpose is, again, to provide an overview of the history and the history of the studies, and also to provide a sense of the type of site characterization information and data that was considered by an expert elicitation during the PVHA. And this talk will be against the background of the PVHA, the probabilistic volcanic hazard analysis, which is held in 1996 in an effort to quantify the probability of volcanoes respecting the site and what the uncertainty was. So I'll be talking against that background.

Studying of volcanism in the immediate repository site, there's been a history of about 4 million years of volcanism within about 20 kilometers of the site. And the quaternary, the last million years or so, there's been six volcanoes shown here in red. The recurrence interval of volcanic episodes, volcanoes of similar age, is about 300,000 years. So volcanism as an event doesn't occur that frequently, but it does occur.

The youngest volcano we've been able to establish in the last few years is dated at about 75,000 years pretty reliably.

The gist of the problem comes down to two things. One, what's the probability of disruption. And so really we want to know, given that somewhere in this area other repository, a future volcano will form, what's the probability that the volcano, where the associated dike system that feeds the volcano and has a larger area extent will actually impact the repository.

The second aspect is given that that happens, what are the consequences. So I'll talk about this framework. Kevin Coppersmith after me will talk about the process of PVHA, how the probability was actually determined. And then tomorrow, Kathy Gaither will talk about some of the aspects of consequences and why some of these volcanism issues have re-emerged through performance assessment.

The timeline, very briefly, volcanism studies began in 1978 as a joint study between Los Alamos and the USGS. It concentrated at that time on regional characterization of basaltic volcanism, looking very broadly several hundred kilometers out to get a picture of the broad patterns of volcanism, the time of volcanism. And then during that time, early on, Bruce Kerr of Los Alamos began developing a probabilistic approach to hazard analysis. Given that these are infrequent, somewhat random events, he felt that that was the best approach to look at this problem.

Phase 2 began in about 1987. At that time, we began focusing down to the immediate Yucca Mountain area, and on the last 5 million years of volcanism, the post-Miocene. Consequence studies began at Los Alamos during that time to look at the effects of volcanism should the repository be impacted. We began to look at how we could reduce uncertainty, particularly in the age of the youngest volcanic center, because there's a lot of interest. There was some hypotheses that this volcano could be as young as Holocene, which had sort of a political impact on looking at volcanism as an issue. And we applied multiple geochronology methods.

And in the probability arena, Bruce Kerr continued to develop alternative probability models and to look at the sensitivity of the results as a function of different approaches to probability. And then in 1996, the program kind of culminated with the year and a half process of the PVHA expert elicitation.

Post-PVHA, in the last two or three years, DOE has carried out some sensitivity analyses of PVHA results in light of new data that's come in, and I'll allude to that in a few minutes, and new interpretations. And in the last year, we've been supporting analysis and model reports which feed into the disruptive events PMR, which Kathy will talk about tomorrow.

Sort of stepping back to a little bit more regional view, this is really the volcanic system that the PVHA experts considered when they were estimating the probability of disruption. Basically, it goes out from the repository about 50 kilometers and it includes all the volcanism known, which has occurred in the last 5 million years. These numbers are the age of particular volcanic centers. The quaternary, the youngest centers, are shown in red, which is in alignment southwest and north of the repository.

Silicic volcanism hasn't been an issue. It hasn't occurred since 7 or 8 million years ago. And in all of these probability models, we really rely on the past patterns of basaltic volcanism as the basis for estimating the future probability of disruption.

The volcanic record includes the presence shown here in blue of inferred and known buried basalt centers. These were first detected by aeromagnetic techniques and believed to be buried basalts. One of these was drilled several years ago and dated at about 3.8 million years ago, which is very similar to this other episode, maybe the same age and just an extension of that episode. But several of these anomalies were known at the time of PVHA, including one up here, and were considered by the experts. They were counted as events using alternative interpretations. Some counted them, weighed them more than others. But they were considered.

There was also a hidden event factor in the PVHA that beyond the known buried events, a factor generally of 20 to 50 per cent was put in to account for events that we didn't know about yet. Since the PVHA, the NRC has done some work with ground magnetic surveys based on aeromag regional surveys, but has focused down on a few that look most likely to be basalt, and these three points here in black are sites where it's a pretty good interpretation that these are additional buried basalts, and these are published in Connard 2000 just recently.

Sort of our feeling is that this adds some new information. It doesn't really change the location of where the most recent volcanism has occurred, and the addition of a few buried anomalies in this area, was taken into account in PVHA because of this hidden event factor of 20 to 50 per cent.

Real briefly, this is just a quick look at the whole history of volcanism in the region, and the only point is that back in the middle Miocene from about 10 to 15 million years ago, there were enormous volumes of silicit eruptions, and this of course is the source of the tuff that the site sits in. But by about 7 or 8 million years ago, in a regional sense, this had all ended. Generally, there's a fundamental shift about that time to much lower volumes of basaltic volcanism. I should explain that these spheroids are--the radius is proportional to the volume. So it's just another way of getting at volume scale, and this is a log scale. So you had hundreds to thousands of cubic kilometer eruptions early, and then once the transition to the south, you're on the order of a tenth of a cubic kilometer, up to a maximum of about 10 cubic kilometers.

And in the last 5 million years, the period of real concern for hazard analysis, the Pliocene volumes were systematically larger, 1 to 3 cubic kilometers, and in the quaternary, the volumes have become very small, on the order of a tenth of a cubic kilometer. So volume has decreased through time, but the recurrence rate has probably increased in the last million years with more frequent episodes. But these are very small volcanoes.

I don't have a photo of these type of volcanoes. I assume most of you have seen these from the top of Yucca Mountain. But they're small volumes. In terms of volcanoes on the earth, these are the smallest type of volcano really. They generally erupt one time, and then never erupt again. They're monogenetic.

Two basic types of data that have to be considered to do a hazard analysis are the timing of volcanism and the spatial location, spatial controls. Timing was done through sort of standard argon techniques. Looking at the past 5 million years, the thing that really came out of that is there's probably six discrete episodes of volcanism during the last 5 million years, with some noise and some uncertainty.

The next slide will be just these points, which are the million year centers in Crater Flat to the west of Yucca Mountain. And in detail, if we look at those points, given the uncertainty of the data, it could be interpreted different ways, and the experts did look at alternative interpretations. The central of this model is that they're all a million years old, and it was basically a single event. But the data allows--these are also arranged from north to south, the northern most center to the southern most center of the four centers. There may be a systematic decrease in age as you go south. So this may be one event, or it may be several events, and this is the type of uncertainty that ran all through the PVHA. Almost any parameter was uncertain and had to be addressed by alternative interpretations.

This is one example of being able to reduce uncertainty. This is dating the youngest volcano. When the program started in the late Seventies, it was believed to be about 300,000 years old, the potassium argon days. By the early 1990s, using argon to argon, the interpretation was that it was about 140,000 years old, but the uncertainty was fairly large. By the mid Nineties, sort of a wrap-up of these studies, we were able to, using multiple techniques and just advancements in techniques, able to reduce the uncertainty quite a bit, and pretty confidently say that the youngest volcano was 70 to 80,000 years old.

The other is the spatial issue, what does the past location of volcanoes look like and how does that influence the understanding of probability of where future volcanism would occur. Most of these models have some aspect of structural thinking built in. The experts and other people have done this, have considered what type of spatial controls and tectonic controls exist, either implicitly or explicitly.

This is an example of kind of the current understanding of the tectonic setting of Yucca Mountain. It lies within this structural domain, the Crater Flat structural domain. And Yucca Mountain lies within the same domain as most of the volcanoes of interest, but it appears, and there's a consensus among people, structural geologists who have been looking at this domain, that different portions of the domain have different extension histories. The most significant thing being that the western and northern part of the domain, the Highlands where Yucca Mountain is a part of, is less extended, and the basin part of the domain, the

Crater Flat topographic basin where the basalts occur appears to have a history of greater extension. And this is shown by things such as fault offset, basin substance, cumulative fault throw, those type of things, topography.

Geophysical studies, also we enforce this. This is from Brocher, 1998, the seismic reflection survey across Crater Flat. And the western part of the basin where the volcanoes occur is deeper and, hence, more extended.

The way a lot of the experts addressed the spatial issue of volcanism was to draw source zones, and these are an example of different source zones by four different experts of the ten experts. The primary method that they employed to draw these source zones is really to look at the pattern of past volcanism. But also in their thinking was is there any conflict with the structural understanding of the basin. So kind of the position we have now four years after the PVHA is that these zones are certainly consistent with the patterns of past volcanism, and they're also consistent with our current understanding of the Crater Flat domain tectonics.

So one question is why do we have, in '96, why do we have an expert elicitation after 15 plus years of intense data collection and analysis. Previous to this, going back to 1980, the DOE approach had been to emphasize a suite of permissible alternative probability models, but all equally weighted with no attempt at discriminating between models.

We just really wanted to look at the range of possible models.

Over the years, there was disagreement among scientists over the specific modeling methods to model the probability, and the parameters that went into these models, which created difficulty in reaching resolution on these issues.

The reasons for this, what contributed I think is, one, a sparse record of volcanism. There's not a lot of data to base these estimates on. And an incomplete understanding of both the magmatic and tectonic process which control volcanism, and both of these things tended to fuel disagreements over the years.

So one of the reasons the PVHA was convened was to provide an independent assessment of the probability of volcanic disruption. And it was also important that the process gave us a good handle on the uncertainty of the probability models, and the uncertainty in the parameter values that were used in these models.

Just one example of parameter uncertainty is dike length. Dike length, as it increases, generally increases the probability of an intersection, because there's more likelihood of hitting the repository. But if you looked, each expert had an estimate of what the most likely dike length was, the 90th percentile dike length. If you aggregated that across the whole panel, the mean dike length was about 4 kilometers, and the 95th percentile dike length about 10 kilometers. So these are the type of inputs that went into the probability models

The last slide, which will lead into Kevin's talk on the process of the PVHA, the bottom panel is the results of the PVHA. This is the probability distribution for the annual probability of intersection, and it centers around 10 to the minus 8 as a mean value, and the 95th percentile span about two orders of magnitude.

And a point I really want to make is that the '96 PVHA was not done in isolation. There were many estimates before that, and there's been estimates since. The very earliest estimates were done in 1980. A series of estimates done, several since then, both by the state shown here in blue, but they had other publications before 1998, and the NRC shown here in pink, and over this 20 years of different probability estimates, the range is about two orders of magnitude. My sense is that there's pretty remarkable agreement over this 20 year period, and that the PVHA elicitation, incorporating the uncertainty, did a good job of capturing this uncertainty and gave us something to really work with in terms of consequent studies.

But I think, you know, these other studies have kind of shown the robustness of the number and the PVHA really confirmed that, that these estimates are fairly robust.

PARIZEK: I think we'll perhaps take questions after Kevin's presentation as well, combine the two. Does that make sense? And I'll remind you that there will be a public comment period presided over by Chairman Cohon after this presentation. So any of the concerns probably should stay put.

COPPERSMITH: Thank you very much. I appreciate the opportunity to talk about the probabilistic volcanic hazard analysis again. The last time I talked about it I think was give years ago, maybe four years ago. We had an opportunity to go into a lot of detail at that time of the nature of the study, the experts that were involved and interpretations that were made. This will be an opportunity to give more of an overview of the project itself, and I want to focus on some of the details of the process that were followed as we go through.

Again, I want to provide an overview. I want to talk about the process and the rationale, some of the key steps, example evaluations and the results and uncertainties, and will end with the probability distribution that Frank showed at the end, but I wanted to dissect it a little bit differently and talk about some of the components of uncertainties.

The purpose of the study at that time was to develop a defensible assessment of the hazard at Yucca Mountain, with particular emphasis on the quantification of uncertainty. The environment, in terms of contention, and Frank talked a little bit about volcanologists, I guess there are a few in the room, tend to be a contentious bunch, and the process of making interpretations and discussing those interpretations is a wonder to facilitate. But it also means that data related to volcanism or volcanic hazard in particular is not unique. It's subject to interpretation, and this was an opportunity to put together the data that had been collected over a long period of time, to put that in front of hazard analysts and give them an opportunity to develop their interpretations. That data and that information provided a strong basis for uncertainty quantification.

One thing that Frank alluded to that's kind of ironic is the fact that there's relatively low levels of volcanism in the area. If we're in the middle of a large volcanic field with a lot of volcanoes, the hazard analysis would probably be a little bit easier to do. There may be other problems associated with that, but the hazard analysis would be easier to do.

The expert elicitation allows for a quantitative assessment and incorporation of alternative models and parameter values. I would contend that all interpretations that we'll hear about today and tomorrow involve expert judgment. This expert elicitation process allows us to formalize the eliciting of the information and quantify the uncertainties associated with it. And as we'll see, there's alternative models as well as parameter values that go into those interpretations.

At the time the elicitation was done, we were following all the applicable guidance for explicitly incorporating expert judgment, the NRC Branch Technical Position. At the same time, a large study sponsored by DOE, NRC and EPRI had come out on the use of expert judgment, and formal expert elicitation process, the so-called Shack Study, and all of those procedures and processes were followed during the course of the PVHA.

The product of the PVHA is a probability distribution of the annual frequency of intersection of a basaltic dike with the repository footprint. The probability distribution, I'll get back to that in a minute. Its application goes on from there. The application then becomes conditional that you have some part of the dike intersect the repository footprint, and you look at the consequences of that, consequences in terms of intrusive consequences or extrusive consequences, and so on. And those will be discussed by Kathy Gaither in the context of the process model report tomorrow.

So the steps in the probabilistic volcanic hazard analysis are typical of formal expert elicitations or process for selecting the experts on the basis of their expertise and applicability to this particular problem. There is a candidate, a pool of candidates, over 70, and a process of winnowing those down occurred.

Identifying the technical issues is key to this type of elicitation. Here, we needed to focus on the issues that need to be developed, need to be actually discussed, the applicable datasets, the uncertainties in those datasets. These discussions of issues and alternative interpretations occurred in a series of workshops. These are interactive workshops that allow for discussion. All of the project scientists and scientists from the state, the center, NRC sponsored research, and so on, were presented to the expert panel, as well as in a series of field trips. So they had an opportunity to basically climb the learning curve in terms of the information that was available.

Also, part of this process is one of interaction, of people when we get into the later parts of the study, preliminary interpretations by the panel members themselves were presented, and they had an opportunity for their peers to challenge their interpretation, to offer defense in their uncertainty characterizations, and so on.

That interactive process is much preferable to an independent expert analysis where you don't have the interaction among the expert panel.

Field trips were held, as I said, to give an opportunity to the recently gathered data, as well as the history of investigations. The actual elicitations occurred in a series of individual interviews, two day interviews with each expert, followed by feedback. There were calculations done to show the implications of their assessments, and they had an opportunity to look at those implications and to make changes.

We also had a feedback workshop that gave an opportunity to see the implications and to challenge and defend interpretations made by all members of the panel.

And, finally, documentation of the process and the evaluations themselves.

This is the expert panel. I won't go through the resumes of them all. But basically, it's a diverse set of individuals who range from geochemists, geophysicists, volcanologists, some with a lot of hazard experience, for example. Alex McBirney has developed some of the procedures for developing probabilistic hazard analysis for the IAEA, and so on. Others, George Thompson, a geophysicist who would focus on a lot of the potential field data, and so on.

All of these geologists tended to be well known in their field, and focus on and were able to bring to bear a lot of experience, and that experience from other locations is key in making interpretations for the data that exists in the Yucca Mountain area.

I should say this included people from within the project, like Bruce Crowe, as well as those who had not worked on Yucca Mountain at all beforehand.

The problem of hazard analysis or volcanic hazard analysis is very typical for those that are familiar with other types of analyses, like seismic hazard analysis. There's two parts to the problem, a temporal part that deals with the frequency of occurrence of volcanism in this case, and a spatial part that deals with where volcanoes are likely to occur in the future.

Some of the models that were considered in terms of temporal models are simple homogeneous type models that deal with essentially a memoryless type of system, to those that are more complicated and non-homogeneous, those that deal with a changing system, either a waxing or a waning system, or one whose timing is controlled by the volume of past eruptions. These types of temporal models were all considered and in some cases incorporated.

The spatial aspect is very important in volcanic hazard analysis, unlike for those who are familiar with other types of hazard analyses, this is a very important component.

Homogeneous models and non-homogeneous models were considered. The homogeneous models are ones, we've heard the term source zone that Frank talked about. Basically, you divide regions into areas that would have a different rate density of occurrence of volcanism, and those areas, with their own rate density, or locally homogeneous rates, are called source zones.

The boundaries of those zones are defined in a variety of ways based on tectonic information, or regions that surround volcanism that has a certain geochemical affinity or a certain age. The boundaries to those source zones are uncertain in many cases, and the uncertainty was handled through either alternative source configurations, just different maps, those source zones, or through uncertainty, explicit uncertainty in the source boundary that could occur over a certain area or certain region. But it was important and known that since we have local volcanoes in the Crater Flat area that the nature of the spatial configuration of Crater Flat versus Yucca Mountain would be important and was focused on quite a bit in the study.

Non-homogeneous models, in terms of spatial models, are also quite interesting. There's some of the parametric, actually certain field shapes that have been invoked based on experience at other volcanic fields, or nonparametric approaches, smoothing, if you will, that invoke the spatial stationarity concept that says that where you've had volcanic centers is where you expect them to occur with some uncertainty. And the smoothing of the spatial distribution of past events--on the future distribution of events, and naturally different functions are used and different smoothing operators for that type of procedure.

These are some examples that Frank showed before of different spatial configurations of source zones. I just want to make the point that the uncertainties associated with these was very important, whether or not, for example, there's a hard boundary that separates the Crater Flat area or source zones in the Crater Flat area from Yucca Mountain is obviously an important issue from the standpoint of volcanic hazard and the repository footprints as shown here. That uncertainty was something that we tried hard to have explicitly quantified in dealing with the experts.

But the issue here is also one there's larger source zones around here, so the real difference is the rate density, the number of expected events per year that would occur within the source zone versus the rate density elsewhere. There is a rate density. There's a finite probability of occurrence per year of volcanic features of volcanoes everywhere in these zones. It's simply a function of where there are differences in rate. And for those that are familiar with the seismic hazard issue, this obviously is the most important component, that recurrence component.

Just some other examples of interpretations of source zones.

I want to show, this is just a logic tree. In fact, it's virtually impossible to read. Hopefully the hard copy is a little bit better. The point here is that there is uncertainty in the temporal models and the nature of source zones, the spatial models that are used, which source zones are actually used, and whether or not there are alternatives. All of these components have incorporated quantified uncertainties. The temporal model, for example, whether or not we're going to use the homogeneous Poissonian type model or one that takes into account waxing or waning, changing processes as a function of time, both of those models could be used by an individual expert and incorporated with alternatives ways to express the degree of credibility of those models.

And likewise, multiple interpretations of source zone configurations like we talked about before could be taken into account.

And important aspect over here has to do with the nature of the event itself. Most of these processes, volcanic hazard, exactly like seismic hazard, deals with events first as a point in space, and then the issue is here we actually have a process that is spatially distributed. A dike itself has an orientation and a length that needs to be taken into account, and that dike, orientation and dike distribution were things that were independently elicited and are incorporated into this. So that event definition itself becomes important.

As an example, if an individual event starts out as a point, or is treated as a point, it might look like this. Variation or uncertainty in the dike orientation and its length can allow for a finite probability of intersection of these dikes with the repository footprint. So that uncertainty was incorporated as well.

In fact, much of the contribution of volcanic hazard to the site comes from the distribution of events in the Crater Flat area and the potential for longer dikes to intersect the repository.

So the outputs are the unconditional probability distribution of the annual frequency of intersection. We'll look at that in a minute. Also some conditional probability distributions on the length and azimuth of an intersection dike, and on the number of eruptive centers that might have occurred along those dikes as a function of dike length itself. There's also a marginal distribution on the length of dike that would intersect the repository. These outputs are outputs that are required for consequence analysis. You need to know how much of a dike, if it does hit the repository, how much would it hit and what would be the orientation, given that it did. And this would then be propagated into considerations of intrusive effects, eruptive effects, and so on.

And finally, the probability distribution on the annual frequency of intersection across all of the experts. I can get into a lot of--we did a lot of dissection of individual expert means and medians to see whether or not in fact there was a broader distribution. It's interesting for those who have looked at this before, the actual distribution of medians and means is pretty comparable.

When we looked at the intra-expert versus expert uncertainty, the contributions, in fact most of the contribution comes from the within expert uncertainty, not the so-called diversity component, which was also in agreement with some previous studies that have been done, and this type of thing.

But, again, we're looking at a range here, the mean distribution is about 1.5 times 10 to the minus 8, a broad range of interpretations. The chart, I have it as a chart in here, and the diagram that Frank showed, some of the other assessments that have been made. One example is the NRC has suggested that a range of 10 to the minus 8 to 10 to the minus 7 might be a reasonable range, and I think will have some sensitivity analyses that show the results out at 10 to the minus 7, but the mean estimate is just over 10 to the minus 8, using this distribution.

And I'll stop there and take any questions.

PARIZEK: Thank you again for a clear presentation, as always, Kevin.

Questions from the Board? This will be important input for what's coming later in the program about doses. Priscilla Nelson?

NELSON: Nelson, Board. Just what's probably a point of clarification. There was a small pink area immediately west of the repository footprint on your maps, and I'm not familiar with what that might be. Can you tell me what that might be?

COPPERSMITH: Do you want to talk about that? That's the Solitario Canyon Dike.

PERRY: Yes, Frank Perry. That's the Solitario Canyon Dike, which is about 11 million years old, and it's the, as far as I know, the only basaltic volcanism that's occurred in the block.

PARIZEK: Advisors to the Board?

MELSON: Kevin, I'm wondering, given the young volcanism, how you feel about following geophysical measurements in the future. Is this something that's going to be buried with time or, for example, the EDM measurements, the GPS deformation measurements. Do you feel this is worth doing? I'll also address that to Frank.

COPPERSMITH: Well, let me first make it, as a hazard analyst, an uncertainty person, the types of things that move the needle here are few. For example, the issue of, let's say, GPS measurements or other types of crustal geodetics that show that in fact the rates of extension into this area are a certain level. Those measurements are important. I think they help serve to calibrate a lot of the information related, for example, to longer term estimates of deformation, like the fault slip rates, which are fairly well determined throughout the Yucca Mountain area. Those comparisons, though, if they don't agree, it isn't clear how hazard analyses would be affected.

For example, in the probabilistic seismic hazard analysis, those geodetic results were presented. Warneke, for example, made presentations. Others have looked at geodetic information. But it doesn't tend to be a controlling dataset in assessments of the rate of occurrence of earthquakes.

And likewise here, the geologic observations tend to be more of a control in terms of actual hazard analysis. If those observations said, for example, that we would add orders of magnitude in the number of events, or in fact the past occurrence of volcanism had been in a different place than it is now, then they would have hazard implications.

But otherwise, they don't tend to affect the two things that matter most, which is the overall rate density, which needs to change a lot.

MELSON: A different question. Specifically, suppose new dikes form, or something happens, how well are you set up, or is the DOE set up to even spot such an event in the future? The whole extrapolation of probability into the future, as you've pointed out repeatedly, and everyone else, is full of vast uncertainties. So I'm going to--

COPPERSMITH: One thing, we did ask the experts at the end of this what would change your assessment, and in many cases, precursory information related to an impending eruption was something they said they would take a lot of interest in. It wouldn't necessarily change their hazard estimate. Go ahead, Frank.

PERRY: Yes, Frank Perry. My thought is that these types of volcanoes represent extremely transient events. And from the time you might first detect it to the time an eruption stops might be a few years at most, as opposed to some other types of volcanoes. So if you set up a 200 year experiment to monitor for signs of some type of magmatic activity, given these recurrence rates of a few hundred thousand years, the chance is nil that you would ever see anything. You can't run an experiment that long.

COPPERSMITH: We can say there is, in terms of the seismicity, this is a well instrumented area. So inasmuch as they would be represented by small magnitude earthquakes, that would be captured in the ongoing--

PERRY: Right. If we saw dike swarms with associated seismicity coming out, they would be captured, just from the seismic net.

PARIZEK: Rod Ewing?

EWING: I'm out of my field, but I'm trying to think about how to characterize the uncertainty from the process of expert elicitation. I'm wondering if you limit the interaction between your ten experts, say divide them into three groups and then go through the process, do you think the distribution would be essentially the same, or very different?

COPPERSMITH: Well, we haven't done that test and we haven't convinced DOE that it's worth doing. But I think I would say no, primarily because the distribution, as we saw the dominant contribution is actually within expert uncertainty.

EWING: That's with the interaction.

COPPERSMITH: That's with the interaction. It could be argued that in fact if we didn't have it, it might be broader. These studies, once Livermore did a series of seismic hazard analyses, expert elicitations, without any interaction. EPRI did exactly the same area, and had much more interaction. We looked at the distributions in those cases, and in fact they were fairly comparable. It's an experiment that really hasn't been done. My guess is that there really wouldn't be too much of a difference.

EWING: Because in my own mind, I think we have to distinguish between the uncertainty associated with the expert elicitation process and the uncertainty in what we're trying to describe, in this case, volcanic activity. So a followup question would be in other areas where expert elicitation is used, where later you can actually make a measurement and determine what the actual value is, what is the kind of general quality of success?

I mean, here, we have a case where we'll never know what the answer is, but we're going to go ahead and use the results of the expert elicitation. But there must be other areas, say estimating the cost of large construction projects where a group of experts get together, go through a process, and they have a number, and then the structure is built. Has anyone looked at the success rate for such efforts?

COPPERSMITH: Yes. And this is a whole field of work, and I won't begin to get into it. But yes, the information we have, almanac type information, ability to post facto see how close you are, this gets into whole areas of cognitive biases beforehand, the training that's required to avoid those biases. All of that occurred prior to this elicitation.

EWING: Okay.

COPPERSMITH: I won't go into all the details.

PARIZEK: Questions from staff? I want to thank all the speakers--oh, sorry, John Kessler?

KESSLER: This may be a premature question, since to wrap this into risk, obviously we need to work on the consequence side as well, which we haven't heard yet. I assume we're hearing that tomorrow. I just want to make sure I understand when these probabilities were developed, they're based on the volcanoes that are there, which is mostly an extensional environment. So it's the kind of volcanism you'd expect in an extensional environment?

COPPERSMITH: That's right.

KESSLER: And so generally they're not associated with really highly energetic types of volcanoes?

COPPERSMITH: Right.

KESSLER: Okay. My understanding is, leading into the consequence side, I understand that the consequences may be on a different kind of volcanism, which will be something that we should look for.

COPPERSMITH: Yes, there will be more discussion of that. You saw on Frank's chart, for example, the volume of eruptions from thousands of cubic kilometers now to less than a cubic kilometer.

PARIZEK: You both will be present for the later presentations?

COPPERSMITH: Yes.

PARIZEK: Good. So if we have questions, we can go further into that. Thank you.

I want to thank again the speakers of the morning, a very full program, as we knew it would be, and Chairman Cohon.

COHON: Thank you, Richard, and thank you for chairing this session. I think it was excellent.

I believe we have two people who would like to speak during this public comment period. I just need to confirm that, because four people signed up. My understanding is that John Hatter and Judy Treichel, although they signed up, are yielding their time to Kevin Camps. Is that right?

Okay, so we have Kevin Camps and Chuck Connor. I'll call on Kevin Camps first. And though they've yielded your time, they had less time to yield because we ran over here. If you could keep your remarks to ten minutes, that would be greatly appreciated. But we understand if you run over. And if you'll state your name again and give your affiliation, that would be appreciated.

CAMPS: Yes, my name is Kevin Camps, and I am Nuclear Waste Specialist at Nuclear Information and Resource Service.

COHON: And, Kevin, if you've got notes, you might find it more convenient to come up here. Would you like to come up here?

CAMPS: Sure. Thank you.

Chairman, members of the Board, thank you for this opportunity to speak. My name is Kevin Camps. I am Nuclear Waste Specialist at Nuclear Information and Resource Service. NIRS is based in Washington, D.C. and is an information clearing house for concerned citizens and grassroots organizations about nuclear power issues and radioactive waste issues.

For the past month, I have hauled a replicate of a nuclear waste truck cask container across the country along the actual projected transportation routes to Yucca Mountain. Our tour began at the Cook Nuclear Reactors in Michigan. We then travelled through Indiana, Illinois, Missouri, Nebraska, Wyoming and Utah. We are culminating our tour here in Carson City, and in a few days, in Las Vegas.

I've spoken with hundreds, if not thousands, of individuals across the United States on this tour. These have included residents along the roads and rails, emergency responders, landowners, real estate agents, elected public officials, teachers and school children, Native American tribal councils and members, truck drivers at rest areas, and many others.

When people learn that I'd be attending this meeting of the Nuclear Waste Technical Review Board, they asked me to communicate to you their concerns. I've spoken with many of these people and they've expressed concern that the corridor states have been overlooked in the Yucca Mountain site characterization process.

Persons living and working along the transportation routes were upset to learn that the dose receptors referred to in the Yucca Mountain Environmental Impact Statement refer to themselves and their families. Many people were surprised and even shocked to learn that both the truck and train transportation containers bound for Yucca Mountain would release radiation even during routine incident-free transports. Many of these communities were not consulted with public hearings, and did not even know about the environmental impact statement process which they could comment on, which is now closed to public comment.

One of the more interesting experiences on this tour was being stuck in a three-hour long traffic jam near Chicago on the toll road. People actually got out of their vehicles to ask me questions about the mock nuclear waste cask that I was hauling. Neighboring motorists were very concerned to learn that had this been an actual shipment of high-level radioactive waste, they could have received the equivalent of three chest x-rays during that three-hour traffic stoppage.

One person that was especially upset was a pregnant woman who had small children with her, and she said that as soon as she got home, she'd contact her elected officials and ask them why she did not even know about the environmental impact statement process, even though there was a hearing held in Chicago.

Toll both attendants were similarly concerned about their repeated exposures to thousands of such shipments, especially when traffic jams like this one would slow traffic to a crawl, or even a full stop at their toll booths.

State Highway Patrol officers that I met across the country had interesting perspectives on this. They were concerned about their current exposure to other hazardous materials on the roads, even to the radar transmissions in their own vehicles, and they were concerned about the cumulative effect of now being exposed to low level exposures to these shipments. They were also concerned about their current lack of training for emergency response, and their potential exposure to acute doses in the event of an accident.

In addition to the highway routes, many of the rail routes across the country paralleled the path that we travelled. In Chicago, Illinois and in Lincoln, Nebraska and in Cheyenne and Laramie and smaller towns across Wyoming, the projected rail transport routes actually pass directly through neighborhoods and near businesses. Numerous residents' homes were right next to the tracks, and parents were especially concerned about the radiation doses their young children would receive from the thousands or even tens of thousands of rail shipments that would pass by under this program.

In addition, homeowners and business owners worried about the negative implications for their property values. All felt that the DOE's Yucca Mountain DEIS treatment of incident-free exposures was inadequate, especially for their specific neighborhoods located right on the tracks. Concerned citizens that I met in Chicago, in St. Louis, in Lincoln, Nebraska, all who had attended the Department of Energy DEIS hearings in their communities expressed deep concerns that the incident-free exposures and even the accidental releases from transportation accidents were inadequately addressed in the EIS.

For instance, DOE assumed 25 year old fuel in its analyses, and people asked about how old the fuel would be in these transportation containers, and were concerned that the exposures that were calculated were not accurate for what they could be exposed to with younger fuel. They were also concerned about the dollar value impacts of a severe accident, and were puzzled why such dollar values were never mentioned at the EIS hearings, or in the EIS document itself.

People were confused why the dollar values hadn't been published, and they were puzzled why the only measure of protection was against latent cancer fatalities. They asked me questions about the broad range of other health impacts that could result from incident-free or accidental transportation scenarios.

People we met with were also very concerned to learn that transportation casks are not subjected to full-scale physical testing under the NRC's certification process. Many expressed their desire that tests to destruction be conducted. They requested me to ask the Nuclear Waste Technical Review Board to urge the Department of Energy to conduct full-scale physical tests to the point of destruction, especially since NRC does not require such tests.

In conclusion, many hundreds of people who I spoke with who live and work along the transportation routes are concerned about the risks to their communities from the transportation shipments, and are feeling very forgotten and overlooked in the Yucca Mountain characterization process, and they urge the Nuclear Waste Technical Review Board to hold the Department of Energy to the highest level of technical standards.

Thank you.

COHON: Thank you very much. Now we'll hear from Chuck Connor, who is from CNWRA.

CONNOR: Okay, I'll try to be brief. I have a few technical and scientific comments about the volcanism studies which have been presented. And, again, my name is Chuck Connor of the CNWRA. I work under contract to the U.S. Nuclear Regulatory Commission.

There's several concerns we have about the PVHA and the volcanic hazards assessment that were presented earlier today. Our probability models for volcanic disruption of the site vary from those presented by Frank and Kevin. Basically, we have a higher range of probability of volcanic disruption.

Frank presented a slide which showed that there's considerable overlap. That's certainly true. But we have to be careful that we first define the event that we're talking about. I believe it's fair to say that Frank and Kevin presented results of igneous dike intersection of the repository which lead to either intrusive effects or extrusion of volcanic products at the surface of the earth.

Our analyses only consider extrusive products. Those are volcanic eruptions which occur which might transport ash and presumably contaminants into the atmosphere to elevations of, say, two to seven kilometers above the site, and disperse those materials down range on the order of tens of kilometers. So our analyses are only dealing with the extrusive events.

We also take a different view of tectonic models. This is a probability map which shows one realization of analyses for the probability of volcanic disruption of the area based on the distribution of past events and tectonic models, and that's basically contoured at events per square kilometer per 10,000 years.

Basically, we agree, of course, that the highest probability of volcanic activity is in the central part of Crater Flat where volcanism has been prevalent in the past. And that probability decreases to the east, but we don't put a--draw a line on that map or consider a barrier to be there.

I'd talk more to any of you individually if you wish. As Frank mentioned, these results are published in the Journal of Geophysical Research in last January's issue. We also recently released documents to the public document room in the NRC in February summarizing about ten peer review publications we have on this topic.

If we look at the probability of volcanic disruption of the repository, this is times ten to the eighth, so these numbers vary from one times ten to the minus eight, to one times ten to the minus seven per year, and vary the maximum length of a volcanic alignment that might be associated with that. You can see that our values for different recurrence rates vary from one to five volcanic events in the system in the next million years, vary between over about an order of magnitude and are all greater than one times ten to the minus eight. So these are for extrusive volcanic erupts leading to the dispersion of wastes in the atmosphere and down range from the site.

So we can conclude from a lot of these analyses, and I don't think people disagree with this too much, that Yucca Mountain is located within an active basaltic volcanic field, active in a geologic sense. I think everybody expects that that volcanic system is likely to experience volcanic activity again in the future.

A lot of detailed geophysical studies, geologic analyses reveal that structure controls the location of basaltic events on several scales. Most importantly, recognition of control of the Bear Mountain Fall located west of the Crate Flat and the Yucca Mountain site basically doubles our hazard assessment. So rather than decreasing hazard assessment, which has been a primary result of the PVHA, we believe that recognition of the structure actually leads to an increase in the probability of volcanic disruption of the site.

Based on that, the probability of volcanic eruptions, that is, the dispersion of volcanic products into the atmosphere, is on the order of ten to the minus four to ten to the minus three in a 10,000 year period. And getting back to Bill Nelson's question earlier, that means we can also say that based on these analyses, there's about a 5 per cent chance of a volcanic event within the entire system in the next 10,000 years, and about a 25 per cent chance of a volcanic crisis, that is, that sort of dike injection which may or may not intersect the repository.

That means that people monitoring that site in the future might have to deal with a volcanic crisis in the system. That's something that people generally try to respond to in some way. It's quite different from simply the probability of volcanic intersection of the site.

So thanks for the time. I just want to point that out. One path toward resolution on this particular issue might be to go forward in the license application with some of these higher values for probability, and make sure the consequence analyses take place and risk is calculated recognizing that there's some probably natural divergence in scientific opinion on this topic.

Thanks.

COHON: Thank you. Do we have a copy of his overheads? Can we get hard copies? Thank you.

Any reactions, comments or questions for Chuck Connor? David?

PARIZEK: I was just going to ask whether those overheads are in that January publication in '99, JGR?

CONNOR: Yes, they come straight out of that publication, yes.

PARIZEK: Okay. Then we're okay.

COHON: Let's get it anyhow, because the recorder would like a copy of it I'm sure.

DIODATO: Diodato, staff. One question. With your hazard curves that you had for the different one event, five events, versus length, and then there's a positive slope to those curves. So that slope suggests that the probability of the longer lengths was also increasing? But, no, not really, that was just assuming the longer length?

CONNOR: No, that shows one range, one ensemble of analyses essentially. And, you know, there are a host of parameters that go into these models. That's one realization, one set of parameters that we think reasonable bound the problem. I think, I don't want to put words in his mouth, but Gene Smith from the State might argue that we'd want to use longer alignment lengths based on some of the work that he's done, that kind of thing. So there is reasonable room to move on some of these kinds of parameter analyses.

DIODATO: But then clearly, that does not suggest an increased probability of longer lengths.

CONNOR: Absolutely not.

DIODATO: Okay, thanks.

COHON: Thank you very much, Mr. Connor. And our thanks again to all the speakers today. I think all presentations, both from scheduled speakers and from the public, were of very high quality. We appreciate that very much.

We'll now take a lunch break for one hour, until 10 minutes to 2:00, 1:50.

(Whereupon, the lunch recess was taken.)

A F T E R N O O N   S E S S I O N

PARIZEK: Dr. William Glassley from Lawrence Livermore National Laboratory and he's going to be addressing coupled thermal-hydrological-chemical processes in using high-performance, massively coupled computers. And, again, this is the last of the session that I will chair this afternoon.

Dr. Glassley?

GLASSLEY: Thank you very much. I appreciate the opportunity to address you on a subject that's something that is near and dear to my heart and we've--

PARIZEK: Quiet in the room, please.

GLASSLEY: What I will do is provide you with a description of what the capabilities are that we've been developing over the last few years. I'm the team leader of a project at Lawrence Livermore that has been going now since 1998. The outline of the presentation will first cover briefly why the laboratory invested in this effort and what the goal is. I'll then discuss a little bit of what the current capabilities are today and where we expect to be going in the future and some of the activities we're pursuing. I'll then provide an example of an application we have been interested in with some preliminary results, particularly as they pertain to a generic kind of performance confirmation activity and then I'll conclude.

I want to point out the team members who have been participating in this. John Nitao is probably one of the--is the preeminent member of the team. He has responsibility both for code design and implementation. Olivier Bildstein, Tom Boulos, Mary Gokoffski, Charles Grant are all involved either in computer science or geochemistry. Olivier is a post doc. If anyone out there is interested in getting a good post doc, he's the person to get. His contract ends in October. Jim Johnson, many of you know as the guru for the EQ-36 gembox database. Jim Kercher is an environmental geoscientist biologist, but with a lot of experience in parallel processing. JoAnne Levatin, computer scientist with some experience with parallel processing, and Carl Steefel, world-renowned geochemist in the area of reactive transport.

Why would the lab invest in this effort? What is their interest? It comes from a long history, literally decades long, of activities in two key areas; one, the area of subsurface flow and transport and the other in the area of application, development, and design of code for state-of-the-art computational platforms. The expertise in the area of subsurface flow and transport made it clear a long time ago that to really do simulations of high-resolution with complex chemistry in natural systems required computation platforms that simply didn't exist until very recently. That, along with the interest the laboratory is having in computation capability, made a very interesting marriage a couple of years ago. People began to realize the laboratory was in a position, with the acquisition of the Blue Pacific machine and IBM SP-2 with 1200 processors, it might be possible to actually put together a simulation tool that would allow us to work at subsurface flow and transport in a way that was different from anything that had been done before.

Briefly, the reason why massively parallel platforms are so important in this arena is represented here. On the right hand side of the figure is something we've modified from a publication Dennis Norton did in 1984 which is an attempt to show the way fluid rock interacts in the subsurface. It's something that's been recognized for decades in the geological community. Dennis made it graphical and we made it colorful. The key thing is that there are three domains that one has to be concerned with. One is the thermal-hydrological, one is the geochemical, and the third is the thermal-mechanical. All of these are linked in a very strong way to the modification of the porosity permeability field that can result either through chemical interactions or through thermal-mechanical effects and how those then feed back to the thermal-hydrological domain.

To represent this system at high-resolution and with fidelity to the natural complexity of most geological systems requires that one deal with something on the order of 50 to 100 independent components in a computational cell for the calculation. The number of cells necessary to represent complex domains varies tremendously, but in three dimensions you're talking of something on the order of 10^6 to 10^{12} cells and representing spacial regions on the order of millimeters to 10s or 1000s or more of meters. Clearly, that kind of complexity requires computational capability that is not work station size or laptop size. It requires something more than that. With the advent of the massively parallel computers a few years ago and access to those, it became possible to seriously couple this stuff being true to the complex chemistry.

Quickly, I just want to show you what's happening in terms of computational power. Vertical axis represents the memory that's available for a calculation. Horizontal axis in years from '96 to 2001. Work stations far within the range of about a gigabyte, a billion bytes of memory available, more or less. This is all approximate. With the terrabyte machines or the massively parallel machines, we're up into the terrabyte range. The green dots represent the trend of memory available for computation that the Department of Energy is investing in in what is called its Accelerated

Scientific Computative Initiative.

As that memory expands, more and more complexity and fidelity to three dimensional simulations is possible. We're currently at the point of being able to represent hundreds of waste packages and many drifts with the complex chemistry fully represented. Now, I'll show you an example of that kind of simulation in a moment.

The code that we have been using on the ASCI Blue Pacific machine is a code that represents full equilibrium and kinetic reactions in a saturated or unsaturated environment for multi-phase flow under isothermal or non-isothermal conditions. Chemical changes are through porosity and chemical changes as a result of dissolution and precipitation processes, modify the porosity, and hence, the permeability. And then, through that, modify the flow regime.

The code is capable of dealing with an unlimited number of chemical reactions, both for individual mineral phases and speciation reactions, but the current limitation is expressed primarily in the database that we have available to us in terms of properties of minerals and aqueous species; about 800 to 1000 minerals, 2000 to 2500 aqueous species. The code is designed to deal with both equivalent continuum and dual continuum models and the simulation I'll be showing you later on is a dual continuum simulation. In many of the cases that we're looking at right now, they're fully three dimensional simulations representing resolution from tens of centimeters to tens meters.

The activities we're currently focused on are represented here. There are three things that are particularly important when trying to understand the expression of coupled effects in geological materials. One is trying to understand how the assumptions that you've made and how the features of the natural system are represented feed into uncertainty in your calculation. We have a number of simulations going on right now and have done a number in the past looking at specifically this issue. Our concern is in how one represents chemical complexity in the system and how one represents reaction rates. It turns out that those two variables can have a tremendous influence on the chemical result one comes up with.

We're also conducting a number of large three dimensional simulations trying to understand what kinds of processes or consequences of coupled processes materialize in short time frames of a few hundred to a few thousand years and longer time frames. How did those differ and what kinds of contrasts are there and responses of natural systems.

And, finally, we're trying to develop a database or a knowledge base useful for designing or conceding performance confirmation efforts. Particularly of interest are the questions what is it you should measure, where should you measure it, and what value should you expect in order to have an efficient, cost-effective performance confirmation effort. Simulations that deal with this particular question are the ones that I will be showing you momentarily.

Long-term, where we want to go, at least as far as application of this tool to Yucca Mountain issues, is through the definition of what the near-field environment is that a waste package will experience. This is a three dimensional block through multiple drifts, multiple waste packages. What we want to be able to do is represent at the centimeter scale the kind of chemical conditions a waste package will experience and feed that to the people who are doing the materials testing and materials programs who can then generate, as an example, probability distributions for certain kinds of failures as a function of time.

In order to do that kind of thing requires that we understand specific response of a repository-like setting in terms of chemical and physical processes. On the left, both of these figures are cross sections through a single drift, and the figure on the right is just a blowup of this particular region in this figure. These two figures harken back to the processes I showed in that color viewgraph, colored image that Dennis Norton did earlier, our modification of it.

What's important are the processes that one has to be concerned with. The movement of fluid within this environment as a result of heat being deposited by the waste package and either boiling or vaporizing/evaporating whatever water is present, fluid moving to lower pressure, lower temperature, environments condensing chemically interacting with the rock, and the potential for the chemical evolution of the fluid phase that could potentially get back into the repository.

Another important process though that has to be of some concern and consideration is the modification of the physical conditions, porosity and permeability, in the immediate vicinity of a waste emplacement drift. Particularly important are the interactions that take place along fractures which is what this is supposed to represent. Fluid moving up along the fracture system as a result of heat will eventually condense flow back down along the fracture, but it's distilled water. It's chemically far from equilibrium. It will react with the rock and the fracture mineralogy picking up solutes, eventually depositing them at the thermal front--whether it's a boiling front or just a high temperature zone doesn't matter--precipitate what it has and recycle. We need to understand those processes.

Now, what I wanted to show you are the results of some simulations we've been doing of a 5 drift, 100 waste package scenario. Each waste package has its own heat output. So, each waste package can be treated discretely. The chemical system is represented here and there are approximately 50 aqueous complexes in that system. 34 minerals, I think, about 12 of those are primary mineral phases, and the rest are potential secondary mineral phases. The block of material that was simulated is a 2km x 2km by about 760 km block of Yucca Mountain-like stratigraphy, 10 cm resolution in the vicinity of the drifts, tens of meter resolution further away from the drifts.

The heat output for the waste package, I want to comment no this because it's important. The heat output was constructed such that the drift wall never got above boiling. We were interested in a low temperature system trying to understand what kinds of interactions might take place under those conditions. We were interested in the low temperatures because that's one of the issues clearly of how to manage the repository and operate it. We wanted to find out what chemical interactions could take place under those conditions.

What you will be seeing are a single drift out of this 5 drift simulation that we did. The simulation will cover 15 years from 85 years after waste emplacement to 100 years after waste emplacement. We were interested in that time period because that covers the time period that, more or less, represents the performance confirmation window. We wanted to understand in this kind of situation, where we have Yucca Mountain-like stratigraphy, but heterogeneous heat output at low thermal conditions, what kind of responses might one expect. Questions we were trying to answer was what chemical parameters would be useful to measure, on what time frames would they change, and which chemical parameters would be of no significance. I shouldn't say of no significance; what chemical parameters would not be changing within that time frame.

What's shown here in green or this block is a small section of the simulation. There's a single tunnel represented along here. Let me rotate it a bit for you so you can get some idea of the dimensionality of this. I apologize for the slow response time of the computer. The dataset is very large and the computer's memory is very small. But, you can get some idea of what the three dimensional form of this thing is.

Now, what I'll do is run it forward in time in, I think, it's five year steps. There are two things that will be changing here. There are two different surfaces, parameter spaces that we're looking at. The green surface encloses that environment within which the bicarbonate concentration in any fracture water that's there is less than 10^{-5} molal. So, it's low bicarbonate concentration. The yellow envelope that doesn't show up right now, but you will see as the simulation proceeds, encloses that region where the PH is approximately 9 or higher.

Let's try step through it manually. This is at 86 years and you can see that what's beginning to happen is that what was a little bit of relief on the bicarbonate surface is now separating out into fingers. Those fingers are quite interesting from a simulation and from a measurement point of view because the magnitude of the change that's taking place and the spatial scale upon which it's changing is adequate to do in-situ monitoring if it's possible to do in-situ bicarbonate measurements or a sample and measure it. The pH, you can see as the yellow lips under here and that moves almost not at all; which means if you wanted to measure pH as part of a performance confirmation program, there's not a lot of information you would be able to gain from it because it's not going to change much with time. Bicarbonate, on the other hand, at least in this simulation, is showing a substantial amount of modification as a function of a time, a relatively short time period. It's the kind of thing that gives you an opportunity to actually do some measurements and see if the simulations that you have conducted, in fact, represent what happened in the mountain. If they don't, it gives you an opportunity to either reevaluate the way the mountain is represented in the code or the way the code operates or reevaluate what's happening in terms of the response of the mountain, itself.

BULLEN: What causes the periodicity? Is it waste package spacing?

GLASSLEY: It's waste package spacing and different heat outputs, yes. So, the way this is set up, I should have mentioned this. Thank you. It's a hot waste package, cold waste package, hot, cold, hot, cold. This is just a part of that 20 waste package drift.

The other thing that's interesting about this is that you can see even though these waste packages have the same heat--the hot ones have the same heat output in this particular segment and the cool ones have the same heat output. The response spatially is different. The reason is that the heat output of the waste packages in this direction and in that direction are not the same as this periodicity here. So, there's some interaction as a result of that, mainly expressed through gas phase circulation.

So, that's the nature of the kind of response you can get in a complex system like this. We've done simulations also where rather than looking in this case at the central part of a drift, we've looked at the end of the drift adjacent to mountain block itself where there are no other waste packages and found an interesting response there. Similar kinds of patterns evolve, but they're on a much finer scale on the scale of tens of centimeters to a few meters. The reason seems to be that the strong thermal gradient that develops at the edge of the repository has a very strong influence on the circulation patterns and chemistry that result. So, the response would be quite different from what you see here, but with a simulation tool like this, it's possible to make predictions about what the spacing of those various chemical reactions or responses would be and go out and test that.

PARIZEK: Can I interrupt here?

GLASSLEY: Sure.

PARIZEK: In that green connections, the fingers between the cooler and hotter waste packages, that could be also focused flow or do you see any evidence that between packages you could actually get water coming into the drifts because of the thermal gradients of the ceiling, say?

GLASSLEY: For this time period, no, we don't see that. We don't see of much of that kind of effect, at all. It's very early on in the simulation. It's only 100 years after the waste was emplaced and most of what we see appears to be a response more to gas phase circulation than to differences in liquid water distribution.

The direction we're going with this right now is to examine at higher resolution the processes taking place within the drift along the drift wall. We're also examining the consequences for systems in which backfill may be used trying to understand how backfill properties may evolve through time. And, also, conducting long-term simulations carrying this out to 250,000 years trying to understand if you make a measurement in particular regions here during the performance confirmation period, how much certainty can you attach to long term projections from measurements here to what the conditions will be 250,000 years into the future for those various chemical components in the system. That's a difficult thing to do. The complexity, particularly related to the thermal regime, appears to make that somewhat problematic.

So, in conclusion, there are a couple of points we think are important about the availability of a simulation tool like this. One is the fact that it's possible now to take a representation of a mountain system like this and vary any parameter you want and understand the long-term chemical consequence and physical consequence and thermohydrological consequence of that parameter variability. What we have seen, so far, is that the things that appear to have the strongest impact on what one considers to be the chemical response are how one represents reaction kinetics and how on represents the complexity of the chemical system. It's extremely important that the chemical system be fully represented. If it's not, much of the chemical variability that is possible simply can't be represented.

In terms of reaction kinetics, one of the things we found is that the response time of the mountain to a perturbation appears to be something on the order of 10,000 to 20,000 years. Within that time period, kinetics dominate the reaction process in the chemical system. Beyond that time period, the system tends to approach a steady-state condition and at that point, kinetics become much less significant. Those are just two examples of how simulations like this can provide you with a handle on what the consequence is for uncertainty of certain variability in particular parameters.

Using this kind of tool, it's also possible to consider how best to design a performance confirmation program and, in fact, with a tool like this, it's also possible to actually do it almost real time. As you learn a waste package and understand the hat output of that waste package, you can run the simulation, decide where best to put the sensors or probes or whatever it is you're going to be using during your performance confirmation period around each particular waste package. It's a way of optimizing the performance confirmation data collection activity and a way of minimizing cost.

So, that's essentially where we are with this. I thank you for the time to present this material. I'd be happy to entertain questions.

PARIZEK: Questions from the Board?

KNOPMAN: I have a bunch of questions, but I'll just ask one now. That has to do with the capability of the simulation tool to look at model--sensitivity to model conceptualization, not just parameter changes. It seems to me it is a very powerful tool here. It gives you a chance to really examine the importance of coupled processes depending on the way you choose to model coupled processes.

So, you know, you've got the circularity in the analysis that has to be broken through a tool like this where you can really start pushing these models into realms that they'll start breaking down if they're not the right ones that are best representational of the system.

GLASSLEY: Yeah. You're absolutely right. And, one of the things we're very interested in doing is trying to intelligently construct different conceptualizations and see what is the consequence of that. There are a variety of ways of doing it, whether it's how one simplifies the stratigraphy or what one does with properties of materials or any of those issues or even how one conceptualizes the linkage between porosity and permeability. All of those things can be looked at in a very straight forward way. The advantage of a tool like this is that it can be done quickly. With, you know, 1200 processes to thwart a problem, you can do some very complicated stuff pretty fast.

KNOPMAN: But, you could also be comparing TSPA conceptualization of the system versus the underlying process model kind of conceptualization to see what you lose in the abstraction.

GLASSLEY: Yes, conceivably, that's possible, but I think that would be an extremely difficult thing to do because the simulations are so different. We've struggled with how best to compare results that we can generate with those that come from other kinds of simulation tools. Because of the resolution, we can bring to it and because of the fidelity with which we can represent the physical processes, we can see things in such greater resolution and detail that it's hard to compare. It's possible to do it, but--

KNOPMAN: You could always aggregate.

GLASSLEY: Sorry?

KNOPMAN: You can always aggregate over your defined model to get it up to the same comparable scale.

GLASSLEY: Sure, yeah.

PARIZEK: Priscilla Nelson?

NELSON: How long have you been developing this model?

GLASSLEY: We started in 1998.

NELSON: Okay. Do you plan on including heterogeneity in the rock mass variability and pressure distribution?

GLASSLEY: Yeah, we're looking at the possibility of--or we're trying to figure out how best to statistically represent chemical-mineralogical heterogeneity within the rock units and porosity/permeability distribution, as well.

NELSON: And, when might you be reporting on that?

GLASSLEY: That's a good question. We expect it would probably be six months to a year before we can actually get those simulations done.

NELSON: And, just one final question. It's been my experience that as you make systems more complicated, sometimes an extreme event of unanticipated impact might occur. Something unexpected. Have you found any of those for your more complex system and would you expect any?

GLASSLEY: We haven't found any. There's nothing we've tossed around, so far, that is likely to do that. Most of what we've done in the simulations or most of what we've seen in our simulations suggests that the bounds the Yucca Mountain Project has placed on certain properties of the system, chemical responses, mineralogical changes, thermohydrological effects, the results we have are well-within those bounds. We've found nothing that would represent an outlier or something that would suggest something significant has been missed. What we tend to see is just much more detail at much higher resolution. But, most of the variables that we're finding, the values for the variables, the ranges that we see, fall within the kinds of ranges the project has talked about in the past.

RUNNELLS: Could you comment, generalize, I guess, in terms of magnitude of the changes in porosity and permeability that you have predicted from this model?

GLASSLEY: During the short time period, for example, the performance confirmation time period, virtually nothing happens. I mean, you see very, very tiny changes. It's clearly not a variable that's going to be important for performance confirmation. Long-term simulations going out 100,000 years kind of thing, we see very complex changes, a spatially complex distribution of changes in porosity associated with several specific regions around the drift. One is along the base of the drift on the sides where the fracture porosity tends to close up. We've seen changes on the order of 20 to 30 percent.

The magnitude of the change, though, depends upon what secondary minerals one puts in one's simulation. You can end up with very little permeability change or porosity change if one simplifies the chemical system. The alumino silicate system, though, is very sensitive to a lot of local conditions that generate on a fine scale in these models. So, we do see over a long time period changes of 20 to 30 percent when the full chemistry is represented in the system.

Above the drift, we tend to see some ceiling and regions where it opens up and they're opposed against each other. So, there appears to be a spatial relationship there where they're kind of feeding each other. It appears that dissolution occurs above, fluid comes down, precipitates some material, and that system slowly migrates away from the drift. Magnitude of change above the drift is something on the order of 10 percent over a period of about 100,000 years. Along the sides, it's a much more complex distribution. It depends upon the heat output of the waste packages.

RUNNELLS: Okay. And, one quick followup. You mentioned that the kinetic expressions are very important and that the mountain responds on the time frame of 10,000 to 20,000 years, I think you said. Then, you followed that remark with something about beyond 10,000 or 20,000 years. Could you repeat that, please?

GLASSLEY: Yeah. Let me put up a graph and perhaps it will clarify some of this.

(Pause.)

GLASSLEY: This is one simulation we did trying to understand what kinds of variations would one see in chemistry in particular parts of the system. Now, the absolute values that we have here probably aren't realistic. We've refined these simulations. This was done on some preliminary mineralogy and representation that we were looking at, but this pattern is something that consistently comes out regardless of the chemical environment you see. During the first 10,000 years, the system--if you perturb it, the system goes through a lot of variability, variation. The response is complex. And, the reason is that we're looking at many minerals, each with a very different dissolution rate coming into importance during this perturbation, perturbed period. But, after about 10,000 to 20,000 years, what tends to happen is that the system reaches kind of a steady-state response and just continues to evolve along a relatively uniform response pattern. The changes aren't very significant. This happens over and over again.

What this is suggesting to us is that during this time period, if you want to have an accurate representation of how this system is going to be responding, you have to accurately represent kinetics. That's absolutely fundamental. If you don't, then you could end up anywhere in the system and it's difficult to say what your response--what your chemical simulation actually means.

PARIZEK: Dan Bullen?

BULLEN: Just a quick followup to this one. You mentioned that during the confirmatory testing period that there really wasn't much change that was identified in your code. But, if you wanted to benchmark the code to be able to use it as a predictive tool, could you foresee, for example, in the types of curves that you just saw an experiment that you would run that you'd use to benchmark the calculations? Maybe a cooler waste package with water, change the near-field chemistry, and then try and predict the performance and see if you accurately modeled that? Is that the type of long-term experiment that you could do in the confirmatory testing phase that would benchmark what you're doing?

GLASSLEY: Yes. Conceivably, you could come up with an accelerated response kind of thing. We hadn't thought of that, but that's a really interesting idea. You could do that.

BULLEN: I guess the followup question is are you severely limited by the kinetics because sometimes you just can't push the kinetics to not make it go and so.

GLASSLEY: Well, if you have an experiment that's going to go for a few decades or more, yeah, I think you could do it. You can accelerate things sufficiently so that you'd be able to see a response and be able to see if it's in the place you expect it to, is it the right magnitude, and is it the kinds of chemical changes that you expect?

Something that would compliment that, though, I think is the natural analogue systems that are potentially useful for doing this kind of validation activity. Those are the only things we have access to that go for the kind of time periods we're talking about. I think we have an opportunity to really explore validation exercises with tools like this using natural analogue systems. There are a number of them out there. They've been talked about for a long period of time. It's one of the things that we're very interested in pursuing.

I should have mentioned that there is an ongoing validation activity associated with this thing. One can never completely validate a code like this. So, it has to go on forever.

BULLEN: Thank you.

PARIZEK: Just following up on that point, do you have any sort of simpleminded experiments like, you know, maybe steam weld hole plugs with silica or something where you can say, you know, in spite of its complexity, it will even predict something simple that you actually have some experience with?

GLASSLEY: There are actually some laboratory experiments that already have been done that either have already used the code to simulate the results and it comes up--it's one of those rare experiences where you sit down and you put in the parameters for a simulation, you turn it on, run it, and come back a few hours later and it matches. You didn't have to twiddle anything. It's scary.

Because experimental programs are difficult to set up, we're trying to find places where experimental programs already exist and tie in with them. One of the things that we're looking at right now is a relationship with, of all places, Denmark where they've done some or are in the process of doing some low temperature geothermal work. We're interested in linking with them to look at their natural system and see what the response is when they perturb it and do the simulation to see if it's consistent.

PARIZEK: These are like bricks. I mean, when you heat up a brick, there's some mineral changes and the thing dries out and, you know, something dumb like a brick making factory, you know--

GLASSLEY: Yeah.

PARIZEK: Any other Board questions?

COHON: Do you talk with the people doing TSPA?

GLASSLEY: Yeah, on occasion, we do. I have to admit this effort has been so intense, we have been in the trenches doing this with our blinders on. We've communicated with the project on numerous occasions letting them know where we are and what we're looking at. I think, the communication, although it hasn't been as complete as we would like, is there in several different guises. We have individuals at Livermore who are working on the Yucca Mountain Project who interact with TSPA people. That added communication exists. We've also talked directly with them.

COHON: What are the biggest data deficiencies from your point of view or what would be the highest priority areas for additional data collection for your modeling effort?

GLASSLEY: Well, thinking of the long-term response of the mountain and from that perspective, there are probably two things that are really fundamental limitations. One is the thermodynamic data that are available describing dissolution of all the mass action laws for various mineral phases. The database for that is limited and the quality is highly varied. The other is really understanding and having data for dissolution and precipitation kinetics. That's a real problem. From all the simulations we've seen, those are the things responses are most sensitive to. The greatest uncertainty seems to come from uncertainty in those datasets. It isn't so much data for the mountain, the properties of the mountain, local mineralogy or things like that. It's more fundamental than that.

PARIZEK: Board advisors?

EWING: Just to follow up on that comment, you've listed that the database for the kinetic reactions includes 800 minerals. Do you want to elaborate on that? I mean, my impression is that number should be considerably smaller.

GLASSLEY: What we have done is take the data that exists for laboratory measurements for dissolution and precipitation--mainly, dissolution kinetics, very little on precipitation kinetics. On the basis of mineral type and mineral structure, assigned dissolution rates for those minerals for which they haven't been measured. So, that database exists.

EWING: So, there aren't experimental data on 800 minerals because that would be 20 percent of all known minerals.

GLASSLEY: No, no. Absolutely no. Yeah, and that would be wonderful.

EWING: Right. And, I just point out that the issue here is the dissolution of spent fuel, and so for the relevant uranium phases, I would say the number is five or less with your data.

GLASSLEY: I think you're right about that, yeah.

EWING: Yeah. So, my last comment is this is exciting, but I think the increased computational capacity has to go hand-in-hand with increased fundamental database and site characterization. Having resolution to a few centimeters is not useful unless you have a description on an appropriate scale.

GLASSLEY: I agree with you and I think one of the things that's important about a tool like this is that it gives you a chance to really understand what are the--given that you need dissolution kinetics and precipitation kinetics data for, say, 700 mineral phases, which of those really are going to make a difference in the results you will be generating? It provides you with a screening process. But, I completely agree. The simulation tool like this at this point is data limited and the data limitations that we have are in the thermodynamic properties and the kinetic properties of mineral phases.

MELSON: A real quick question. As complex systems are looked at in chemistry and other areas, more and more chaos shows up. How the hell do you take that into account? And, secondly, do you have a phase rule check on your results?

GLASSLEY: Yeah.

MELSON: You do?

GLASSLEY: Yeah. I agree with you about chaos. We've been really interested in--in fact, Tom Boulas (phonetic), one of the people on the team, that's his expertise. He's been very interested in looking at the results to see if there is any evidence that we are getting into a chaotic realm. So far, the things that suggested maybe we were turned out to be numerical problems, not really chaotic responses. Nothing we've seen, so far, appears to suggest that we are, in fact, entering a chaotic environment, chaotic regime. But, that doesn't mean it won't happen.

KESSLER: Perhaps at the risk of pushing an inference from one of your statements too far, you had something about one of the result, a fracture permeability reduction on the order of what; 20 to 30 percent, you said? Maybe Bo or somebody can get up and put that in context of what the current variability uncertainties are in fracture permeabilities now to understand whether a 20 to 30 percent reduction is important or it's already in the noise of the uncertainty or variability that's there.

BODVARSSON: Thanks a lot. The fracture permeabilities, you know, vary by four orders of magnitude from something like 10^{-14} meter squared to 10^{-10} meter squared. So, there is a large variability.

GLASSLEY: Yeah, I think it's important to recognize, too, that the coupling from porosity to permeability is very, very sensitive. So, how that 20 to 30 percent change translates into a permeability change is a matter of a lot of debate.

PARIZEK: Questions from staff?

RUNNELLS: I just want to, I guess, repeat or reemphasize what you have said; namely, that the thing is data limited to two primary needs that you identified, kinetic expressions and the thermodynamic data, and yet if you look at the literature or you look at university research or you look at Government research, there is precious little of that kind of research being done. It is not sexy to derive thermodynamic data. It's slow, the publications come out slowly, and it's not admired to derive thermodynamic data experimentally, real data. I think the kinetic expression work is a little more sexy and we see a little more of that.

But, do you have any ideas on how to stimulate how to enhance, how to generate real numbers from experimental work anywhere that will feed into these increasingly sophisticate thermodynamic kinetic models.

GLASSLEY: Offhand, no. What I hope long-term is that as more and more simulations are done like this and more and more people have access to machines like this and get their hands dirty doing these kinds of exercises, there will be more and more pressure, more and more interests, and it will become more sexy to do those kind of measurements. But, it's going to be--I think, we're going to be data limited in that realm for a long time.

RUNNELLS: Increasing level of frustration may drive us to it, right?

GLASSLEY: Exactly.

RUNNELLS: Okay.

KNOPMAN: Can I just clarify a point? The information we've seen, so far, coming out of the project suggests that coupled processes really are not that important in terms of overall performance. Do you agree or disagree with that statement?

GLASSLEY: I would have to disagree. I think there are a couple of things that I think reactive transport and coupled processes play a very important role in. The performance confirmation activity, it seems to me, if it's going to be successful and it's going to increase people's confidence in repository models, will have to look at the consequences of coupled processes because that's all you're going to be able to measure. When dealing with long-term radionuclide transport, flow pathways are going to be changed through chemical interactions with the mineralogy. Secondary mineral phases are going to form. The only way radionuclide transport can really be rigorously represented is by interactions of the fluid phase containing the radionuclides with those secondary mineral phases, that's important. That's part of my soapbox.

DIODATO: I can understand your concern importance of getting the kinetic parameters for the minerals, getting a better handle on those. Does that make sense? But, I'm a little confused about downplaying the importance of quantifying the heterogeneity and dispersivities (phonetic) in the model domain because, it seems to me, that's kind of a primary thing, controlling all flow even in the absence of heat, you know, before adding heat.

GLASSLEY: I didn't mean to downplay it. Those are important things. They do have a very strong influence on the behavior of the system. I was thinking in terms of being able to do long-term calculations--both short term and long-term calculations looking at chemical parameters. For those things, the kinetics and the thermodynamics really are dominant and how one represents the thermohydrological system is secondary.

When talking about flow on the other hand, it's a different ball game. There, the thermodynamics and kinetics still are critically important because those are the things that will modify the flow pathways, but being able to accurately represent those flow pathways is fundamentally important. You need to have that framework in place before you can do a realistic simulation.

DIODATO: Right. Because I mean the fundamental questions that need to be answered by the project hinge on the results of flow and transport calculations of radionuclide transports.

GLASSLEY: Sure. Yeah, yeah. No, I would agree with you.

PARIZEK: I think that concludes the section then on scientific and technical issues and we appreciate the comments. I hope for those members of the public who are still with us, they'll understand why all of this review is important because from now on, it's the total system performance assessment discussions. That's where it all feeds into the next level. Dan Bullen will chair that next session.

BULLEN: In the interest of time, I actually thought about truncating the speech that Leon Reiter wrote for me, but it's a really great speech and it lays the groundwork for what we're going to do next. So, I also just checked with our Chair to get his prerogative, and since there's no public comment period at the end of today, we're probably just going to extend the schedule that will go until a little before 6:00 o'clock. I want to give every speaker for the rest of the day the amount of time. So, just adjust by about 30 or 40 minutes the schedule that you see in front of you. With that, I'll start the great speech that Leon wrote for me.

My name is Dan Bullen and I'm the Chair of the first session on total system performance assessment for site recommendation. In shorthand, we refer to this as TSPA/SR. There have been, at least, four previous iterations of TSPA for the Yucca Mountain site, the most recent of which was the TSPA/VA, the performance assessment conducted for the 1998 viability assessment. Chairman Cohon has in his opening remarks pointed out the significance of the current iteration. It will indeed provide the primary technical basis on which a decision of site suitability will be made.

TSPA is, by its very nature, a very complicated and, sometimes, opaque model or group of models. There are possibly thousands of input assumptions and parameters needed to model the performance over thousands of years of this complex mixture of geology and engineering. Whether we like it or not, people think of TSPA as a giant black box with many knobs that need to be turned and set before the results can be calculated. It is incumbent on those carrying out the TSPA to point out which knobs are being turned, which of these are really important, what they are being set at, and why the chosen settings are technically sound.

In addition, these assumptions will always be associated with uncertainty. It is incumbent on those carrying out a TSPA for decision-making purposes to carefully describe, quantify, and display these uncertainties. At the May Board meeting, we were informed that some input parameters would be chosen to represent "conservative" or "bounding" values. Choosing such a conservative value can in certain situations lessen the need to articulate all of the uncertainty. This, however, places a heavy burden on those who claim conservatism to demonstrate that it indeed exists. In addition, using a mixture of conservative, realistic, and possible non-conservative assumptions can greatly complicate efforts to assess overall uncertainty and conservatism. Such estimates are needed for decision makers. It's a tough job carrying out a TSPA for Yucca Mountain repository. The DOE has shown great progress in its successive iterations. We are looking forward to what this latest and most important iteration can and cannot say. We hope that we can get some answers to the issues that we have raised.

As you can see by your agenda, TSPA will occupy the rest of today's meeting and most of tomorrow's. We will start off today with a tag team presentation by Abe Van Luik and Bob Andrews on the overall structure of the TSPA/SR and its results. We've asked for a general discussion of the models, the data, the results for the different time periods and scenarios; for example, nominal and disruptive events, the overall uncertainty and conservatism of these results, and a comparison between TSPA/SR and its predecessor, TSPA/VA. We would very much like to know what the analysts believe are the potential uses and limitations of TSPA/SR.

Following their presentation, we will begin a series of presentations on the individual components of the TSPA, summarizing critical assumptions, underlying technical bases, and sensitivity tests carried out to assess the effects of different assumptions. These presentations will continue tomorrow. Today, we will hear from Bo Bodvarsson who will discuss the assumptions regarding unsaturated zone flow and transport, Ernie Hardin who will discuss the engineered barrier environment, and Pasu Pasupathi who will discuss the waste package and drip shield.

Now, our first speakers. Abe Van Luik is senior technical advisor to the assistant manager for licensing for the Yucca Mountain Project and is responsible for the application of TSPA to determine compliance with safety standards and to help guide design and field investigations.

Abe is an environmental chemist by training and has emerged as the DOE's chief spokesman on TSPA.

Bob Andrews is the performance assessment department manager for the M&O. As such, he is responsible for delivering a good TSPA to the DOE. He probably knows more about the different aspects of TSPA/SR than anyone else. Bob is a hydrogeologist by training.

And, with that, I turn it over to Abe and Bob for our first set of presentations. Great speech, Leon.

VAN LUIK: I've got to talk to Leon more often because my job title has changed. He didn't know it. It's correct here.

Let's get right into it. Although, before I do, I wanted to mention that I first became friends with Nick Stellevato at a TRB meeting after I had given a talk. He took me out in the hall after my talk and told me in no uncertain terms that I was wrong and he disagreed with me, and at the same time, we became friends. Now, I learned something from that that I've been trying on my kids and certain DOE staff and it doesn't work as well and you've got to have a mixed personality to go along with the approach.

We're going to talk about regular regulatory requirements, our objectives, major improvements, a few things about the design, just list the process models, and a very important statement on the current status of what you're about to see.

Regulatory requirements. We're dealing with proposed DOE, NRC, and EPA regulations, as you well know. They require a TSPA to evaluate Yucca Mountain. Must include all relevant features, events, and processes. Must analyze performance in terms of individual protection requirement, groundwater protection requirement, and human intrusion. Individual protection must include both the probable behavior, as well as the effects of potentially disruptive low-probability, high consequence events like volcanism.

Some nice quotes mostly from the EPA proposed standards. So, this is still subject to change. The only thing I wanted to point out here is our position and intent is that unequivocal numerical proof of compliance is neither desirable nor likely to be obtainable. This is a direct statement saying that they expect uncertainty in their look at the system. The focus of our work should be on the full range of defensible and reasonable parameter distributions, something that Dan just said, and not on the tails of distributions since the goal is to evaluate likely performance and not unrealistic or low-probability performance. I think these are nice concepts to keep in mind when you listen to the presentations on TSPA.

TSPA/SR is one in a chain of project-conducted TSPAs. I've already said that these are the things that we're going to address because of the regulations. The EPA standard also requires that we look at peak dose whenever it occurs and report it in the EIS. And, it's important for you to understand that the TSPA/SR evaluates the significance of the quantified uncertainty in the underlying process components.

We've made improvements since the TSPA/VA. We actually have. We enhanced our models to address review comments on TSPA/VA to the extent that we could in the intervening two years. Models with major enhancements include the climate and seepage models, coupled thermal processes, waste package degradation where stress corrosion cracking is now our major potential failure mode, and we have modeled initialed defects and weld flaws in concert with looking at stress corrosion cracking. Thanks to Nye County work, we have a much better picture of the saturated zone and I think we are doing consequence modeling for volcanism now which we did not do for VA. We are also having to modify the approach to address NRC and EPA draft requirements or proposed requirements.

The process improvements, everything is now controlled under common QA procedures. We're using analysis and model reports to trace information flow. This is one that really should be in both places, but for this TSPA, we did an explicit evaluation of features, events, and processes so that everything is traceable in terms of assumptions. TSPA/SR model is used to assure that a person can track it to the datasets that were used and the Q-status of all data, models, and software is now being tracked. So, we feel that there is great improvement and a lot of work has gone into this between TSPA/VA and SR.

TSPA/SR is based on the site recommendation design. The repository design considers an average thermal load a little bit lower than VA of 62MTHM/acre--I love those units--which is lower than the VA. It's important for you to realize that there is no performance impact expected from liquid water removed through heating. That was a VA thing. it is no longer that way in the SR. We're expected 50 years of ventilation, but that's a flexible parameter, as you heard this morning. And, blending of fuel at the surface to levelize the thermal load. Those are some of the operational parameters that will help control the engineered system.

The system has a titanium drip shield placed over waste packages, no backfill, and a line load of about 1.4kW/m.

The waste package design considers still waste packages for commercial spent fuel and co-disposed defense spent fuel and defense high-level waste. It's an outer layer of corrosion resistant Alloy-22, 20mm, or with an inner layer of stainless steel, 100mm worth. There's a Dual-Alloy 22 lid closure weld. The outer lid closure weld stress is mitigated by solution annealing and the inner lid closure weld stress is mitigated by laser peening. You'll hear more about that later.

In my humble opinion which is not so humble, we have done a very good job this time of integrating from the science and the engineering from the bottom up into the TSPA. This is just to show you the process model category and the process model report list on the right where all of this work is documented. Of course, the analysis and model reports are referenced in the process model reports. So, there's a complete traceability all the way from the top to the bottom.

This is an important slide. The results, Bob Andrews is actually going to show you some results. They are preliminary and still subject to change. They're intended to be used for general discussion of sensitivities and barrier important analysis in this meeting, but they're still undergoing checking. They are not suitable for making regulatory compliance judgments of any type.

The calculations after checking and after we make sure that they are the ones that we want to put into the TSPA/SR Rev 00, are going to support the TSPA/SR Technical Report, the Repository Safety Strategy Rev 04, and the SRCR. There will be some updates of these calculations that Bob will show you today and that will create a TSPA/SR Rev 01 and that is what is expected to support the final SR.

And, now, I'd love to turn the time over to Bob who will get into the technical details. I just wanted to set the stage so there's no sense asking any questions. Thank you.

BULLEN: Abe, I'd never let you off that easily. Chairman Cohon, did you have a question or--

COHON: Yeah.

BULLEN: Okay. Go ahead?

COHON: Do you have a deadline for revisions that will go into Rev 01?

VAN LUIK: I believe there is a deadline in the schedule and Bob can correct me if I'm wrong, but isn't it about the February time frame? December/January.

COHON: December/January. Sorry, this went by very quickly, Abe.

VAN LUIK: Yes.

COHON: One thing you said or--yeah, on Slide 7, the second bullet on repository design considers no performance impact from liquid water removed through heating. I didn't want that to go by unremarked because that seems a significant conclusion or design consideration since moving water through heat was a major part of the strategy up until this morning. So, that's just a remark. That's not a question. You know, you can respond to it if you want, but it seems significant.

VAN LUIK: Well, that's the reason we put it up there. We wanted you to make sure that we are no longer looking for a positive performance impact from the heating, itself.

COHON: We are still moving water, however, through heating. It's just that it's not a key part of the strategy.

VAN LUIK: Right.

COHON: Okay. That's all for now, but I'm sure I'll be back.

BULLEN: Another followup question on this slide. Your inner layer of stainless steel is now 100mm thick instead of 50mm thick. When did that change?

VAN LUIK: Okay. We need a waste package person to raise their hand and tell me either that my slide is wrong or that it changed at a certain point in time because these numbers seem quite familiar to me.

PASUPATHI: I believe it's 40 to 50--

BULLEN: Pasu, identify yourself?

PASUPATHI: I'm sorry, you were saying it's 100?

BENTON: Our current design in the maximum one is 100mm for structural purposes. It is not a corrosion barrier.

BULLEN: Hugh, don't go away yet. There's a quick question I'm at. Is that for an extremely heavy waste package and, on average, they're going to be 5cm or are they all going to be 10?

BENTON: All the large ones are 10. The smaller ones are lesser than that. We haven't got the exact number because we've been focusing on the larger ones which from the structural standpoint are the bounding ones. But, our 12 BWR and 24 BWR will probably wind up to be less than 10. We want to optimize that for reduced cost.

BULLEN: Okay, thank you. Paul Craig and then Debra?

CRAIG: Staying with Jerry Cohon's point of a moment ago, could you clarify that no performance impacts? One of the concerns in the past used to be the possibility that mobilized water might reflux and produce corrosion effects. Is that kind of consideration now no longer to be included?

VAN LUIK: That is the kind of consideration where we selected this Alloy-22 material and there will be a later speaker that will address these things in more detail. But, we selected it because of its immunity to the type of environments that would be presented by a trickle of water. It doesn't matter whether you're over 60 percent relative humidity or if you have a little water or a lot of water. The behavior as it is modeled currently, if you believe the model, is the same. So, there is no benefit. There's also no detriment to moving some water around.

CRAIG: Okay. So, does that mean you're no longer modeling water movement?

VAN LUIK: No, we are modeling it because it's part of the overall picture of--

CRAIG: But, not taking credit--

VAN LUIK: Right. There's no--

CRAIG: Either plus or minus?

VAN LUIK: Yes.

CRAIG: So, if it should turn out that C-22 is impacted by water, for example, because of lead as we heard this morning, one might have to rethink this?

VAN LUIK: We will have to get the lead out.

BULLEN: Debra?

KNOPMAN: Abe, you'll regret having put that line up by the time we're finished. It seems to me the word you meant to put there instead of impact was credit and this is a huge difference in meaning and I think that's what the Board is reacting to here because you cannot assert there is no performance impact unless you can have something to back that up. Now, you can say you don't know or you're stilling investigating it or you do know, but you're not taking credit for it in terms of performance. A very large difference in meaning. So, I know you're always updating and you're editing your slides. I suggest you change that word there because it gives off a very different meaning.

VAN LUIK: It's true that had I had my original intent, it would have said credits, but I was reminded of a couple of things. One is that the sensitivity studies that we have done show no impact one way or the other because we are talking about the first failure of the waste package. A very long time like this would be a prehistoric event, a few hundred years above boiling temperatures. And, the second is that even with the VA design, if you look at the DEIS, from a 10,000 year performance point of view there was no impact. So, those things added up to saying that there really never was any impact.

KNOPMAN: Well, wait a second. We just went through a discussion about coupled processes here and the PMR for the unsaturated zone does draw a conclusion that there's virtually no impact on flow and transport. Bill Glassley's results suggest otherwise over a 10,000 year time frame. There's a very big difference. One percent change in porosity which is what's in the PMR versus a 20 to 30 percent change. This doesn't seem trivial to me, at all. So, again, I say you're not sure you want to stick with that statement there given what we've just heard.

VAN LUIK: I'll be happy to change it to credit right now and move on.

KNOPMAN: It's not just--I mean, it's a question of what you think, what the project thinks.

VAN LUIK: But, the point is is that Bill Glassley's modeling has not been verified in any sense. So, it's an interesting indicator that in the long-term you may get a little bit more effect. But, I don't know what his assumptions were as the thermal loading, how long it lasted, etcetera. I don't think--well, unless he has really communicated with his own PA people at Livermore, I don't think that this is the particular design that he was looking at, this kind of thermal loading. But, I'm not sure. So, we would have to investigate that.

I think this is an important point. At this point in time from the modeling that we have done an the assumptions we have made, it looks like it has no impact. If it shows that the modeling has an error or the assumptions were wrong, of course, we will have to revisit the issue.

BULLEN: Other questions from Board members?

(No response.)

BULLEN: Board advisors?

KESSLER: I think this is a comment directed at Debra more than Abe. Why I asked Bill the question I asked him was to say is there an impact and he said, well, there's about a 20 or 30 percent reduction. And, that's why I asked Bo to get up and say, well, what is that in terms of the current uncertainties that are being brought along? I'm led to the conclusion that Bill's results suggest that there isn't that much of an impact. So, I beg to differ.

COHON: Wait a minute, hang on. I'm really glad you so did, John, because it reveals a very key point. TSPA is filled--you know much better than I--with big and small assumptions and conclusions of just that sort. And, they add up and add up, accumulate and accumulate, and suddenly, you don't know what you got. So, when there are specific aspects of the system that are modeled in two different ways and you get discrepancies and it's an important mechanism for the mountain for the project, I think, you're got to resolve those differences and not say, well, it doesn't really matter.

So, here, we have a PMR that says there's no--this is not--we're not interpreting or making something up. This is a quote. The change in whatever the parameter is--I don't want to get the wrong one--that Glassley was talking about is one percent. And, he says it's 20 to 30 percent. Because the way processes are represented in the model matters hugely, if we can't explain differences like that, then we won't be able--no one will be able--to rely on TSPA. So, I don't think you can only look at the bottom line, you know, the dose, because there's so much going on in the model.

Do you see my point? I mean, it's just like saying coupled processes don't matter because we don't have coupled processes in the model. So, I mean, how do you show that it has any impact, whatsoever? That's an oversimplified version of what I'm saying.

SPEAKER: I don't think I said that.

COHON: No, of what I'm saying; not of what you're saying.

BULLEN: Jerry, Bo Bodvarsson is standing right there ready to respond. Bo, you want to say a word or two?

BODVARSSON: I just wanted to clarify a little bit which I think is a little premature to jump into comparisons of those true results. Number one, the most critical factor that controls the T-H-C processes in my view are the mineral assemblages are not the fluids. They are the fracture porosity and heterogeneity of the medium. The main reason that the UCPMR concludes that this is not an issue based on our current models is that measurements have indicated up to one percent fracture porosity. That means out of the mountain, a whole percentage is void space in fractures. That's a huge void space. And, therefore, in order to pluck it up, you require a huge amount of mobilization of water that dissolves solids that then participates and fills this void space in very small type volumes. So, the recommendation that since we do not know Bill Glassley's parameters, especially the critical ones with regard to spacial variability and absolute values of fracture porosities and stuff like that, there's really nothing we can compare.

BULLEN: Other comments from Board advisors or Board staff? Alberto Sagüés?

SAGÜÉS: Yeah, maybe I didn't quite understand. Do I understand that the Alloy-22 is considered to be so corrosion resistant that whether there is or there isn't water on it, it really doesn't matter because corrosion would not take place?

VAN LUIK: If I conveyed that impression, it's probably an overstatement. The model actually does take into account water, but it seems like the difference that we see for a hundred or two hundred years in the seepage, whether we have heating or after the heating, that is what doesn't make any difference in the performance of the overall system. So, the Alloy-22 is corrosion resistant to the point where if there is a little water or over 60 percent relative humidity which allows, you know, sodium nitrate to collect water around it, it doesn't make that much difference. In fact, a little bit more water would help wash off the salts, maybe. But, the modeling takes into account the extreme resistance of this material to the type of environment that we expect.

Now, this morning, we heard that, you know, there are other environments that might have a very different effect on that material, but we have another talk coming up that's going to talk about the design and perhaps that particular question could be given to a person that's an expert in that area; I'm not.

SAGÜÉS: But, you're still referring--it seems to me that that statement over there, does that refer to the very beginning like the first couple hundred years or is that--

VAN LUIK: It's a statement of the impact on 10,000 year performance that we see no difference when we vary through the parameter space that describes the variability that is introduced by the heat loading. We see no difference in 10,000 year performance, you know, the dose performance, from whether or not we have water mobilized by the heating for the first couple of hundred years. The real answer is that when you have a waste package that lasts thousands of years, what happens the first hundred years is just prehistoric. It's the conditions over the very long term that determine how the material behaves.

SAGÜÉS: Although, I would say if you that if you get into a condition whereby you're going to have a jet of hot water dripping on a container of this type for a couple hundred years, that's a situation that would give many corrosion engineers maybe good cause in thinking about what may happen under those conditions. And, I don't know if that could be dismissed so lightly.

VAN LUIK: Well, perhaps so, but don't forget that we have the titanium drip shield to absorb that first few hundred years of impact and to divert it. So, that's the reason that--I'm reporting what the model is doing here. It may be that in the scenario that you envision of hot dripping water on an actual waste package that that would be a slightly different story. I don't know.

SAGÜÉS: So, we'll see what the next presentations will bring then.

VAN LUIK: Yeah.

BULLEN: Okay. I think we've grilled Abe enough. We'll let him hand the microphone over to Bob. Well, until tomorrow; Abe will be back tomorrow for an encore presentation.

ANDREWS: Okay. We're going to walk through now in the next--how much time do we have, Dan? Half an hour, is that right?

BULLEN: Yes, Bob, about a half hour.

ANDREWS: Okay, good. Walk through the current status of the TSPA/SR and I'm going to treat this, more or less, as an introduction to the seven talks that follow. It's the seven talks that follow starting with Bo and ending with Kathy Gaither tomorrow morning that talk to the technical bases for the analyses and the models that feed into the TSPA. So, I'm going to give the overview now of all the individual components piece parts, if you will, of the TSPA/SR, walk through the preliminary results, try to give a sense for what does move the needle and what doesn't move the needle of those TSPA/SR results, give a preliminary sensitivity analysis, and the following talks, the following seven talks, will go into a little more individual sensitivity analyses for their individual component part.

I would be remiss if I stood up here and tried to represent this as all my work. There's an incredible team of very hard-working people who are still back there in Albuquerque and Las Vegas still working hard, still doing the runs, still analyzing the runs, still doing the plots, and still documenting the results and checking the results. A very talented team, that team has presented to you in other situations and, in fact, none of them are here. They're all still back there.

I'm going to walk through the process, you know, some of the attributes, put it into overall context, walk through the system parts, and then get into the results.

Starting with Slide 3, who was it? Dan, was it your quote or Leon's that talked about knobs or something to that effect. So, now, we have the TSPA wheel, as opposed to the waterfall diagrams that we had in the VA, for a lot of different reasons. One, we want to talk about the process of how a TSPA is created and talk about the individual piece parts, the individual component models that are required to feed into that TSPA.

We'll start with the upper right hand corner with the features, events, and processes. You know, if you've read and looked at draft Part 63 and even 197, it talks through the TSPA process, a well-known process applied internationally, applied on WHIP, and applied here in previous iterations. In previous iterations, we formally didn't do the screening of the features, events, and processes to determine those that need to be included in the models and analyses and those that, for whatever reason--perhaps it's a probability reason, perhaps it relates to salt and we don't have a salt site, perhaps it's a consequence kind of criteria--that particular feature, event, or process can be screened out of the models. Therefore, no need to include. So, therefore, the very first place anybody who wants to build some of the key underlying assumptions that are fed into the TSPA/SR model would be the family of 10 analyses model reports that describe the features, events, and processes relevant to that particular component and how it was screened in or out. And, if it is in, where is it in and how is it in? You know, what analyses model is that particular component part included in the TSPA? And, that stuff is summarized in the process model reports. There's a chapter, two usually, of each of the process model reports. It walks through for that part, you know, what features, events, and processes are in the TSPA/SR and which ones are out of the TSPA/SR.

We then have done a lumping of the component parts into those parts that relate to pretty high-probability expectation of likelihood of occurrence and we've called those the nominal scenario. So, all the individual component parts and models and analyses that we really think are likely to occur with their uncertainty are incorporated in that nominal scenario in Part 63, and in the NRC parlance, it's a scenario class, but that's just a little definition issue. For us, we'll call it a scenario in here. The other one are the low-probability, you know, close to the regulatory concern, 10^{-4}, 10^{-3}, over the 10,000 years. So, 10^{-8}, 10^{-7}, and you head the one main one this morning which is volcanism.

So, we have volcanism scenarios or scenario classes. We then have based on those scenarios each of the individual pieces of the system. We start with UZ flow and start, just as we did in the VA with how water moves through the system, how mass moves through the system, how energy moves through the system, how information in global sense moves through the system. Start with unsaturated zone flow. Bo will talk at depth about that later on, I think, immediately following me.

We then talk to the engineered barrier system environments. This includes the things that you guys were grilling Abe about just a little bit ago. What happens in the environments, in the drift, and around the drift? It affects seepage, it affects chemistry, it affects the stress state, it affects rockfall, etcetera. Ernie Hardin will talk to that one in greater detail.

We then have the waste package and drip shield degradation. Pasu Pasupathi will talk about that in greater detail.

The waste form degradation, once the package is degraded and the waste form, no matter what it is, whether it's a glass or commercial fuel or Naval fuel or a MOX kind of fuel, it will degrade. Christine Stockman will talk tomorrow morning about that in greater detail.

We then have transport first through the engineered barrier system and Ernie will talk about that. Then, through the unsaturated zone, Bo will talk to that and the model associated with that. Then, we get to the saturated zone and Bruce Robinson tomorrow will talk about that one. The biosphere, John Schmitt will talk about the conceptualization of the biosphere, the critical group water usage of the critical group, the assumptions there for the TSPA/SR.

And, finally, my volcanic scenario coming along here impacts all of the above. If I have that event occur, it's no longer what I think it is right now and how I think it's going to be extrapolated over the next 10,000 years. Other things happen, other processes occur, and Kathy Gaither will talk to those. We've already talked this morning about the probability aspects of the volcanic scenario; Kathy will talk about the consequence aspect of the volcanic scenario.

I will marry those two things when I get to the results and talk about the risk associated with that particular scenario.

We often decide we have this other one, human intrusion. And, as Abe already talked to you about, there's a number of different performance measures required in the draft regulations on both the individual does, expected annual dose, the groundwater protection, concentration in the groundwater at the point of compliance, and the human intrusion dose.

The next slide would put some of these same aspects in words. I was going to skip over that.

What I have in the following five or six are just kind of conceptual pictures, you know, to sort of orient yourself to where the individual speakers, the next seven speakers, are going to be. They're at their individual part of the system and everything gets integrated in that wheel that's in that total system model.

Starting first with the attributes, the attributes are the same as we had in the repository safety strategy. We have three which are the same as were in the VA. So, keeping water off the waste, the package life time itself, mobilization and transport, and finally the effects or potential effects of disruptive events.

The next slide walks through, just as was done in repository safety strategy, we have three and the VA. We've broken the system not only into nine process model reports, but into individual pieces that contribute to each of those process model reports. In the repository safety strategy Rev 3, they were called the factors, I believe. In repository safety strategy Rev 4 and in the TSPA/Sr being drafted right now, we're trying to relate them as they're more models. So, we're calling them process model factors.

Under each one of these process model factors are a family of the analyses model reports. The entire family, I believe, that TRB has been briefed on of 122 analyses model reports are feeding into each one of these. You know, for example, under this simple little bullet called waste package degradation, there's probably, Pasu, I don't know, 19 or 20 analyses model reports on the individual component parts that feed into that bullet. In in-drift physical chemical environments, you know, what is the environment in the drift? You know, what's the chemical environment, the hydrologic, thermal environment, the stress environment, the degradation of the rockfall environments? There's probably eight or 10 analyses model reports that provide the scientific bases and all of the assumptions that are tied to it and there's lack of some information that relate to the feeds into that--oops, well, this is kaput now. So, we need some more batteries.

Walking through the individual parts, we have those that Bo will talk about; you know, the water above the drift and how much water gets in the drift, those aspects of the total system. The next slide are the ones that Ernie is going to focus in on. You know, the environments inside the drift, what are those environments, what are the uncertainties in the environments, what are the conceptualizations and conservatisms, if any are included in those environments.

Pasu will then talk to the degradation of what happens inside the drift, the degradation of the engineered barriers that are there; both the drip shield and the package.

Christine has a lot to talk about. There's a lot of complex processes that occur inside the package, chemical processes, hydrologic processes, not so many mechanical, but chemical and hydrologic and thermal processes that occur inside the package before the waste is mobilized and available for transport. She will walk through some of those. Then, we have the transport back through the engineered barriers, back through the natural barriers, ultimately to the point of potential compliance which in the draft regulations is at 20km downgradient from the repository site.

Kevin talked a little bit about volcanism one this morning. This is a conceptualization of some of the processes occurring from the volcanic event. It can be volcanic events that interrupt and intercept the repository that degrade the packages and there can be volcanic events that lead to direct extrusive events through a volcanic conduit and then, you know, an ash deposition. Kathy will walk through the aspects of the consequences given the probability is sufficient to be of regulatory concern.

The next one is just a very schematic picture of what happens in the human intrusion scenario which is a requirement in 197 and 63 and 963. So, to give you a conceptual idea of what's going on.

Before I get to the results, I think it's very important--and I think the Board asked for this in Leon's opening comments excellently read by Dan--to talk to the uncertainty, and to the extent possible, some of the conservatism included in the TSPA/SR model. It's hard to do that in a few slides. It's probably even hard for the individual presenters that follow me in 10 or 15 more slides to give that adequate justice. Adequate justice is in the, unfortunately, I hate to tell you--in the 122 analyses model reports in which those assumptions are elucidated and discussed. The significance of those assumptions may not be elucidated and discussed in those. All the individual analysts or principle investigator or scientist or modeler is doing is saying these are the assumptions I have made because of perhaps the complexity, because of perhaps lack of data, or whatever. And, I think, they are reasonable because-- whatever their reason for because is. And, it's important to point out that all of that uncertainty and all the variability and all the conservatism if it's in there are housed in that family of analyses model reports.

Within each of those component models, as appropriate, the analysts then model or have decisions to make. In the face of uncertainty in virtually every one out there, there is some degree of uncertainty. That analyst or modeler made some decisions, some judgments that are in that analysis model report. Climate states, how many climate states fully capture the range of possible climate states? The assumption is three. It seems reasonable. They give a nice, strong basis for it in the analysis model report written by the survey of why three is adequate for the regulatory time period and what those values are and the uncertainty in those values.

How many infiltration states are appropriate? Well, the answer again came to be three, a nice round number; high, medium, and low. The bases for that again are in the analysis model report from the survey. What range of permeabilities are appropriate for evaluating seepage? You know, every aspect of the system. How far above the drift do you want to measure seepage before you evaluate how much could seep into the drift? Could it be 1m, could it have been 10m, somebody chose 5m, gave a basis for it in their analysis model report. Does it make a difference? I'm going to say, no, but can I show you a plot that shows that it makes no difference today? No, I can't, unfortunately. So, that degree of complexity, the degree of how the individual analysis model report incorporated the uncertainty or in some cases put some conservatism into the analysis because they really didn't know or to put a full PDF encompassing what they really felt from max to min, they had a hard time justifying. So, they went with a conservative assumption and there are some of those.

Although I can't do it justice in a few minutes, what I've done in the next three viewgraphs is walk through each of those component process model factors, which are correlated to the attributes of repository safety or attributes of the total system, and tried to give in a very kind of bird'seye view with a little check mark is uncertainty or variability in that particular component included in the TSPA. And, I also have a very simple, straightforward set of comments on aspects that relate to some detail of that particular part.

Let's take an example. Probably the example Abe just had on the board is as good an example as any with no check marks. You know, you might may, my gosh, you have no uncertainty in coupled processes and its effects on seepage?

No, that's not the case, at all. We have tremendous uncertainty. I think Bill's presentation and a discussion we just had a few minutes ago point to some of that uncertainty. We have conceptual uncertainty, we have parameter uncertainty, we have process uncertainty, we have scale uncertainty, we have time uncertainty, we have every uncertainty. We have thermodynamic uncertainty. We have every uncertainty you might want to have to say, well, what range of possible seepages could you get from all these complex coupled processes? So, in that particular instance, the analysis model report originators and there were probably two or three of them in that particular area chose to take a fairly simple and reasonable and also a little bit conservative assumption. They said let me just take the flux from my thermal-hydrology model which we have a 600 locations--you guys were presenting that all or, I guess, a subset. Not the whole Board was involved in that video conference, right; just a subset? Okay. A subset of you were exposed to all 600 columns in their glory. Spatial variability in thermal-hydrologic response, uncertainty in the thermal-hydrologic response, all of which gets folded up into a PDF of percolation as a function of space and time. Now, which is uncertain? All the analysts did is say, okay, for analysis purposes, I don't know whether the porosity changes 20 percent or 1 percent or 5 percent and nor do I really care because I'm going to take that flux well-above that zone and apply it to that seepage model.

Is it unreal? Oh, my gosh, it's unreal. Do you expect that to occur? No, I expect the drift scale test to say what fraction of flux above the repository actually does into the repository. Can I answer that question now with any degree of confidence or could that--it's not me, it's the analysis model report owner who has to do that; not me. And, the answer was, no, it can't. So, the easiest thing to do, the simplest thing to do and most appropriate thing to do was to take that conservative assumption and that's what was done. Do I know what the impact of it is? Do I know if it was .1m or 10m or double the flux or quadrupled the flux? Would that make a difference? No, I can't tell you the answer to that right now. My gut is it's no difference, but I can't show you a plot that shows no difference.

There's some other examples in here on the next page. Not to steal Christine's thunder any, but for the DOE spent nuclear fuel, a lot of uncertainty there. You know, some good data, a lot of good data collected at PNL on waste form characteristics and degradation characteristics, particular of the N-reactor fuel, but some conservative assumptions were made. A lot of complexity, tremendous variability; there's 250 something waste fuel types. So, it was just easier and more appropriate to bound the degradation rates of all the DOE fuels and to take no credit for any DOE fuel cladding. So, that's the assumption that's in TSPA/SR.

These individual ones, you can check me when Bo and Pasu present whether they're going to capture the same checks and comments. But, capture the uncertainty that's included i the TSPA and some of the conservatisms that are also included in the TSPA.

Let's go to the results. As Abe already pointed out, these are preliminary with the exception of VA. Those have been out for awhile, but we've put the same little caveat words down at the bottom.

Let's start with VA. There's a lot of results in TSPA/VA. The best comparable result to the ones I'm going to be showing you from here on out are in Figure 4-28. We had two different ways of showing doses in the TSPA/VA. If you remember, that was in the middle of summer of 1998 and NRC was in the process of preparing proposed Part 63. We had some indications from presentations; I think, some of those to the Board on the way of doing the calculation NRC expected to put forward in Part 63. So, we did the calculation that way once in the VA and it's that figure. I didn't give you the page number, but it's that figure in TSPA/VA. All the other plots of TSPA/VA are generated in a slightly different way of doing the dose plots. So, this is equivalent that I'm showing; 95th percentile, mean, medians, and 5th percentiles based on the TSPA/VA design, the TSPA/VA models, and assumptions and conservatisms if they were there. You know, if you want to zero in on a few numbers, you know, the mean of the dose out there at 80,000 or 90,000 years was 20mrem/yr or so per year; 95th percentile, of course, above that.

The next plot is the first of the nominal scenario class TSPA/SR results. I've done two things--actually, three things differently. One is to show each of the individual realizations that resulted in a dose consequence to that critical group. That's all those thin little lines that are kind of hidden behind the thick colored lines. The other thing we've done is still show the 95th, mean, median, and 5th to give, you know, the audience a range for what kind of range of possible outcomes are we talking about? You know, what did all that uncertainty and all of those check marks that were on the previous slides, what was--when you propagate them through the system, what is the impact on dose consequence and you can see--

BULLEN: Just a quick question in clarification since this came up. How do you get the mean greater than the 95th percentile? Is it heavily weighted on that 5 percent?

ANDREWS: Yep.

BULLEN: It really is then. So, those 5 percent really drive everything that's--

ANDREWS: Yeah. Suppose you had 95 zeros and 5 nonzeros.

BULLEN: Okay.

ANDREWS: Your mean is still non-zero and it's very close to that, you know, 95th percentile. In fact, in many cases, the mean is higher than the 95th percentile. So, low-probability events or features are driving the mean. And, when we get to volcanism, we'll see a very low-probability event driving the mean of the dose response.

BULLEN: Okay, thank you.

ANDREWS: This is 300 realizations. Was that question? Just as a clarification point, yeah.

Let's talk about the next slide. Slide 21 talks to the nuclides--well, maybe I should have stuck. Well, I can do it on here; that's okay. The mean of the dose response at that 100,000 years--remember before it was 20mrem/yr or so--now, it's 60 or 70, something like that, mrem/yr at 100,000 years. This plot, I'm sorry the colors didn't come in as well on the screen, but hopefully they're a little better in your handout. It illustrates another, you know, conclusion that was reached in the VA. That at earlier times--and early, of course, is point of some--to hear talk of all the geologists as early as 70,000 or 80,000 years and for others, maybe early, that's way out there in time. But, at earlier times, the dose is dominated by the highly soluble, poorly retarded, in fact, hardly retarded at all, nuclides like iodine and technetium. Same as in the VA; iodine, technetium are dominating the dose for the first tens of thousands of years. After that time, it's the less soluble slightly retarded, but not completely immobile nuclides; in particular, neptunium 237 and the colloidally transported plutonium. These plutoniums, the 239 and we have the other plutoniums in there, too, but this is the dominant one, are being transported colloidally through the system. So, again, it's iodine and technetium early-on, neptunium and plutonium later one just as in the VA.

The next one just to--because, you know, some people might have said, boy, that curve still is kind of rising, you know, to the right of the 100,000 year plot. What happens as you go later out in time? And, you see the dose is still rising for these preliminary analyses and peaking at a few hundred mrem/yr for the mean out at a few hundred thousand years. That's not that different than the VA. The VA had, I don't know what it was, 200 or 300mrem/yr at 300,000 years being driven by a glacial climate change that occurred at that particular time.

This is an interesting plot because there's several interesting things to pull off of here. One is--you know, if I pick a particular time slice and let's pick the one at 100,000 years because I put the dashed line at 100,000 years, the spread, the total variance of the possible outcomes is probably six or seven orders of magnitude, you know, of potential dose. It's a huge potential variability of does attributed at that time period. As you go a little bit further out in time, you know, out here at 200,000 or 300,000 years when now most of the packages have failed, you're driven by two or three things. You're driven by neptunium solubility, you're driven by how much water got in that drift, and how much water got out of that drift. That's about what you're driven by at peak. So, the variability, the uncertainty has gone from seven orders of magnitude at 100,000 years to three orders of magnitude, roughly, and maybe a little less than that, even; two orders of magnitude at 300,000 years.

The nuclides shown on the next slide is all neptunium, you know, at the peak or most of it is neptunium. Some of the thoriums are coming in, some of the plutoniums do come in, but it is dominated by neptunium.

The next slide, 24, everything up until now has been nominal. So that what's likely to occur with probabilities close to 1, like .9999, that kind of number, now we come to the one that was talked about this morning, probability of occurrence of about 1.6×10^{-8} per year or 1.6×10^{-4} over the 10,000 year time of regulatory concern. And, we have two things going on here. Kathy, I think, when she presents it tomorrow is going to break it out a little bit better. This is more of an introduction for her talk than to go into the details. The two things going on is over the first, you know, 7,000, 8,000, 10,000 years were dominated by the eruptive scenario event. So, the volcanic event and true to the repository and continued on to the surface, created a little ash cone or cone of some dimensions and ash cloud which was transported 20 kilometers to the south.

I should point out I think I have in my list of assumptions on that three slides. There was a lot of complexity about which way does the wind blow? Which way does the wind blow when the volcanic event occurs? Well, who knows? You have a wind rose, it's a reasonable wind rose. You could factor that wind rose into your analysis, but this particular set of calculations just says the wind blows south. The wind blows south. So, that event occurred and the wind blew south and the ash is sitting out there 20 kilometers and is respirable and is incorporated in the soil and incorporated in the crops and it's breathed, etcetera. So, that's the first, you know, 7,000 to 8,000 you're dominated by that scenario.

After that, you're dominated by the intrusive scenarios. The event occurred. It disrupted the package, disrupted the drip shield, disrupted the cladding because the temperature of that event, Kathy will tell you tomorrow, is I don't know what, 1200 degrees C. My package wasn't designed for 1200 degrees C to maintain its function, even though Hugh is going to say, no, it can still behave fine and there's a lot of uncertainty associated with that. The assumption here is when it saw that 1200 degrees C, the package no longer performed. It's gone and the cladding is gone and the drip shield is gone.

WILLIAMS: Just a quick clarification. Why is this so much less than the nominal scenarios? Why is this number so much less than the nominal scenarios? It seems to be less than a milligram compared to much bigger than that.

ANDREWS: Yeah, the main thing that's going on here, this dose rate already has factored in the probability of that event occurring. So, it's factored in the $1.6x10^{-8}$ per year or the $1.6x10^{-4}$ over 10,000 years. By whatever probability was sampled. As Kevin showed you, there's a PDF of probability. There's uncertainty on the probability of this thing occurring. That uncertainty is in that model. So, that PDF is being sampled. So, you get this kind of dose response; you know, the smooth curves being the eruptive event, the more coarse curves being a randomly timing of the intrusive event.

If we go to Slide 25, we see the effects of essentially combining the two scenario classes, the disruptive events with the low-probability and the nominal features, events, and processes with the probability of close to 1, but not exactly 1. So, you see the mean of the curve does extend, you know, to prior to 10,000 years. That curve is the mean of the curve driven by those low-probability disruptive events. This one, Dan, going back to your question, I mean, this one is far exceeding the 95th percentile. 95th percentile is close to zero; in fact, it is less than zero--not less than zero, it is zero; you can't be less than zero. It is zero at 10,000 years, but the mean is significantly above that because you're driven by a low-probability, high-consequence event.

Okay. That's just one part of TSPA showing a few curves and, you know, some squiggly lines, but that's only the first part. I think the Board has been instrumental and pushing, and I think correctly so, to understand fundamentally what drove it. In Leon's opening remarks read by Dan, it was what moved the needle or what knobs moved things? They're doing a lot of different things, you know, to try to evaluate that within the context of the models and analyses that are incorporated in the TSPA/SR.

First, we're doing just normal statistical analyses and a lot of different ways of doing those statistical analyses; simple regression type analyses and more sophisticated analyses to look at what drove the extremes of the distribution. Those are very illuminating. To understand what drove the top 10 percentile to be that top 10 percentile, it's called classification analyses and there's regression that can be done after that. So, you can attack this thing from different angles to try to understand within the parameter space that you have--and there's 240 or so parameters that are uncertain and being sampled in these distributions. What is making it contribute to the variance and what drives the highs and lows of distribution?

Another thing that we're doing and you'll see some plots of these by the individual presenters are sensitivity analyses very analogous to what we did in the VA, except now every time we do any analysis, we're doing multiple realization analysis. We're doing it in the way Part 63 asked us to do it, not in the way we did it in the VA. So, it's the expected value of the output, not the expected value of the input. It's the expected value of the dose consequence or dose risk, not the expected value of input parameters that drove it. It's a key distinction. It just adds a little computational burden. We don't have 1200 processors like Bill does, but we have enough processors to do this efficiently over the time frame that we've been allotted.

Then, we get to some barrier importance analyses, very elucidating, to understand the individual barrier contribution, you know, to total system. How much does the overall UZ flow, not just an individual part of it, but the overall UZ flow, how much did that barrier contribute to overall system performance whether you degrade it or whether you enhance it. You know, what kind of range of things do you have? And, some of these will be discussed in the individual process model talks that follow starting with Bo and going on. I want to give those guys a little out right from the get-go. They have plots in there and they're TSPA plots of what moves the needle. They saw the plots when we saw the plots which was two weeks ago or a week ago. Well, we have Bo his a little late, but everybody else had them quite a while ago. So, understanding what is exactly causing the move of a needle, you know, the subsystem contributions to moving a needle, we haven't done all those analyses yet. I mean, as Abe pointed out, this is work-in-progress trying to give you the benefit of that work-in-progress, but the work is not done. So, if you get hard on Bo, I'll stand up and try to defend him as he maybe doesn't understand exactly the curves. And, we maybe not have fully analyzed some of the curves and there's still other analyses going on. You know, we're neutralizing things, we're still evaluating significance of individual component barriers.

The last type of barrier importance analyses has been reserved for a special talk tomorrow right after lunch, I think, Dennis, right, on the current status of the repository safety strategy Rev 4. And, just as in Rev 3 and a little bit in Rev 2, a barrier neutralization analyses have been used to try to elucidate what's driving the system and what's important to the overall system response. Dennis will talk to that.

One example of the regression analyses are taking those 300 curves that we had for the nominal performance and doing simple regression analyses on them. When we do that, the first five parameters pop up as explained, the spread, the variance of the results. Of those five, four of them relate to the package and each one of those four relates to what's going on at the weld and the degradation of the weld. It's the stress profile at the well and the corrosion rate of the base metal in the vicinity of that weld.

With that kind of information, we then go in and do--and I'm going to have an example of how we do a barrier importance analysis. There's uncertainty in every one of those aspects and more, you know, associated with the package. So, we go into those particular component parts that drive the uncertainty and the degradation of the package and uncertainty in the rest of the system, too, but let's just focus on one example which is the package example, and we look at the 95th and 5th percentiles of those distributions and then rerun, you know, the whole model. When we do that, as an example, we get Slide 29. That black line is the mean of the curve that I showed you before. The red line shows what happens if I choose these things at their 95th percentile which is at the worst end of their distribution--not at the worst, it's towards the worst end. And, if I went to the 5th percentile, I'd have no packages failing. So, if I really reduce the uncertainty in the stress profiles and some of those other parameters that relate to the package, I'd have no packages from the models, from the analyses model reports that are incorporated in the TSPA, no packages failing in the first 100,000 years. Kind of the Swedish concept.

Okay. The next slide just talks to some of the barrier importance analyses that will be presented in the following talks. And, because the list was changing, this might not be the final list that actually is going to be presented, but they kind of give you an idea of the types of analyses that will follow to help to explain what drove the system response.

Abe already talked to the technical improvements on Slide 32 and the process improvements on Slide 33. I just want to reiterate on Slide 34 that it's a work-in-progress. Clearly, we haven't stressed everything in the system yet. Every issue or uncertainty that we can evaluate probably has not been evaluated yet. It might be in there, but we haven't maybe evaluated the significance of it. I mean, that's work still to be done. But, I think that's probably as good a point as I need to stop and entertain any questions.

BULLEN: Thank you, Bob. We'll start out with questions from the Board. I'll start with Jerry.

COHON: There's a lot to digest here and it's also complicated enough that it's hard sometimes to put into intelligible statements just what it is I'm trying to get at. So, please, be patient with me as I try to get to the core issues.

First of all, I guess, I have a question about nomenclature. All of your horsetail diagrams have as their vertical axis dose rate, but you really mean some kind of expected value of dose rate, don't you?

ANDREWS: The mean curve on there--I forget what color we made that; black, in the end, I think--the mean curve is the expected value of the dose.

COHON: Dose, okay. Let me get specific. Let's talk about the volcanic scenario. Is that dose rate in the same sense that you use dose rate for the nominal case?

ANDREWS: Yeah.

COHON: Even though it's weighted by the probability of the occurrence of a volcano?

ANDREWS: Yeah, because in a nominal, I took those curves and the correct mathematically down to the fourth decimal point, would be to take those nominal curves multiply them by .9999.

COHON: All right. Well, that's another question.

ANDREWS: As opposed to 1.

COHON: Oh, okay. Well, let me go back. I would never use nominal in that way. I mean, nominal means, to me, and we're getting I think partially into nomenclature and I'm not sure it's a technical issue, but nominal means to me in some sense an expected outcome, a normal outcome. Doesn't it mean that to you?

ANDREWS: I think it means--to me, it means it's my expectations of the models, the highest probability models. But, each one of those has uncertainty. That's the whole family of horsetail.

COHON: Right. No, no, no, wait a minute. The nominal case, I'm assuming, means you take your 200 and some odd parameters and each one has a nominal value. Is that what that means?

ANDREWS: No, not in this--

COHON: No?

ANDREWS: No.

COHON: No, of course not, because you're sampling from a distribution.

ANDREWS: I'm sampling from that distribution. I mean, I could call that one case, you know, the expected value of the input, the expected value of my models and parameters.

COHON: Yeah. No, you're right. Okay, good. I just clarified or we clarified together one of my confusions. The sense that that's nominal is only nominal in that it's not volcanic, right?

ANDREWS: That's correct.

COHON: I don't like nominal. I really think it's a bad word. I just do.

ANDREWS: I appreciate--I mean, it may be--

COHON: I'm not arguing with what you're doing; I'm arguing with what you're calling it.

ANDREWS: The semantic, okay. Can you think of a better--we tried base case in the VA and base case left people kind of queasy, too.

COHON: All right. But, you see my point about how nominal is interpreted?

ANDREWS: Yeah.

COHON: Dan, I've just got one or two more.

BULLEN: Go ahead, that's fine. We've got a lot of time.

MR. COHON: Oh, yeah? Okay. I'll keep floundering around here. Why 300 realizations?

ANDREWS: We did 100, 300, and 500 and the means are not dissimilar; they're all on top of each other. They're not exactly on top of each other, but statistically they're on top of each other. So, we chose 300 as the most representative. And, also, 300 was the most meaningful for getting statistical regression output. When we were running 100, even though the mean was stable from the total system perspective, the mean was stable. Doing the regression analyses was giving us spurious statistical regressions. So, 300 was giving very meaningful regressions and 500 was giving the same results as 300. So, 300 became computationally efficient, yet sufficient for our purposes.

COHON: Could we go to #15? This is your table with the check marks which I think is going to be very useful. But, actually, what I want to talk about is, in particular, the issue with regard to the coupled effects on seepage. Now, I want to make sure I understand this. When you choose the percolation flux at 5m above the crown of the drift, are you then assuming all of that enters the drift?

ANDREWS: No.

COHON: Oh.

ANDREWS: No, that's--

COHON: What are you doing with it?

ANDREWS: I take that flux which is now a certain number of millimeters per year of average water which is a function of time because I've heated the system, I apply that to the seepage model.

COHON: Okay.

ANDREWS: And, the seepage model has fracture characteristics and uncertainty and variability and fracture characteristics like permeability and suctions and things like that--

COHON: It's just isn't in coupled processes.

ANDREWS: Not coupled, yeah.

COHON: Okay, fine. I'm done. Thanks.

BULLEN: Priscilla Nelson?

NELSON: No.

BULLEN: Oh, no? Priscilla Nelson?

NELSON: My questions are too stupid.

BULLEN: No, there are no stupid questions.

NELSON: Yeah, there are. Okay. This was a joint question. I need help. On Page 28, I've been trying to grapple with this barrier importance analysis and understand what uncertainty importance factor is. But, on 28, when you have a case of a mean being graded in the 95th percentile, what do you do then when you're doing this analysis? Do you use the mean?

ANDREWS: Let's see, on any of my input--this is an input to TSPA. I'm not sure in my input distributions I have a mean that's greater than my 95th percentile. I do not believe we do, but I should go check. It is possible that the distribution is so long distributed that the mean is greater than the 95th percentile. I don't think I have any of those, but I will ask the folks back in Las Vegas and

Albuquerque whether that occurs. I don't think it does.

NELSON: But, if you did, you would use the mean instead of the 95th?

ANDREWS: No, we probably would have said go use the 95th percentile. In the methodology that we chose, we said let's go with the 95th percentile. I probably should check that, though. Good point.

NELSON: Okay. Can you tell me on the preceding slide, tell me again what is the uncertainty importance factor as you've calculated it there?

ANDREWS: This is--what we're trying to do is describe what drove the total variance on the output where the variance in this case was the 100--well, we did it at each successive time after 40,000 years. So, I had a total distribution of dose at each time slice; 40,000, 60,000, 80,000, 100,000 years. So, I have total variance. Now, I'm trying to explain what parameter variance helps explain that total dose variance the best.

RUNNELLS: They don't have to add up to 1?

ANDREWS: No.

RUNNELLS: This is Priscilla's question. They don't have to add up to 1?

ANDREWS: No, that isn't what we're--the first case is other parameters that didn't pass a certain screen that are not plotted; you know, kind of in the noise here. When you're in the noise, you don't know whether it's real noise and means something and you should look at it as meaning something or whether it's just statistical noise.

BULLEN: I have a couple follow-on. Do you want to go to just the immediate previous slide, #26, and maybe we can talk a little semantics here to sort of straighten my mind out. I understand a sensitivity analysis where you can set individual parameter to the 5th or the 95th percentile and then take a look at the response of the total system to that calculation. When you do a barrier importance analysis or a barrier neutralization analysis, I guess, the question that I have for you is then are you picking, for example, all 5th percentiles that would mean that you're driving it all in one direction or are there cases where the 5th percentile of one and the 95th percentile of another counteract each other and so you kind of ended up with a 50 percentile, anyway?

ANDREWS: Yeah. What we have to do is look at each parameter and say which one? Is it the 5th percentile that's worse or is the 95th percentile that's worse? Sometimes, it's 95th, 95th, 5th, 95th that you're combining to get the worse performance. It just depends on distribution.

BULLEN: Okay.

ANDREWS: On the neutralization, you're going outside of those statistical distributions to begin with. You are outside the bounds of the zero to 100 percentile.

BULLEN: And, maybe Dennis will explain this tomorrow when we talk about RSS Rev 4, but when you talk about a barrier neutralization, is the barrier completely removed or is it--how do you handle a barrier neutralization or will that be better explained tomorrow?

ANDREWS: Oh, I think, Dennis might go through that tomorrow. We're removing the function of it; we may not be removing it physically, but the function of it.

BULLEN: Okay. That again will be something we'll look forward to because sometimes we have difficulty grasping those concepts.

Knopman and then Wong?

KNOPMAN: Bob, on 15, 16, and 17, I just want to make sure I understand the significance of a check mark. If there's a check mark in the column for quantified uncertainty or quantified variability, that means if not used someone else could give us the order of magnitude or the range of uncertainty surrounding a particular parameter or set of parameters that describe the--that's somehow associated with that process model. Is that right?

ANDREWS: That's correct.

KNOPMAN: So, we can do that with you. We could get a whole--we could fill in these blanks?

ANDREWS: Yeah. One of the things I put in the backup was kind of a list also by process model factor of the key--

it's not the complete set--but the key input parameters and the uncertainty is at the parameter level, you know. That little check mark is just kind of a rollup, you know, simplifications, you know, shorthand. But, the key is down at the parameter level in developing the PDFs or the variability down there.

KNOPMAN: All right.

ANDREWS: So, yeah, in the AMRs or the individual presenters as they come up, you know, could go into which ones of these were uncertain and which aspect of the parameter was an uncertain input parameter.

KNOPMAN: So, these are all parameter uncertainties as posed--these charts are not intended to try to represent any model uncertainty?

ANDREWS: Well, you know, now, we're going to get into a little semantics. The infiltration--as far as PA is concerned, it's incorporated as a parameter. If you say what underlies that distribution--you know, climate is as good an example as any--you might say really was uncertainty that he had, Rick Forester of the Survey had, in his model, in his representation, but as far as its incorporation in TSPA, it becomes parameterized. The probability is X of this climate state, Y of this climate state. So, parameterize, you know, in the abstraction.

WONG: Priscilla felt bad about her question. So, my question is intended to make you look good.

A number of Board meetings ago, Mark Nutt gave a presentation on GOLDSIM (phonetic). It was portrayed at that time that GOLDSIM was designed to be a simplified model for the public to use or maybe even for me to use to understand what's going on. And, yet, in here, I see you've used GOLDSIM to do the calculations. So, has GOLDSIM increased in its importance or complexity or have you decided to choose a simpler route?

ANDREWS: No, I think--let's make a distinction, I think, between GOLDSIM, the piece of software that we are using and a lot of other people are using; WIPP is using it now a little bit, the Spanish are using it, the French are using it. The GOLDSIM software can be as complex as the science dictates or requires. It can be very simple; you know, a simple response surface kind of representation or it can be very complex with a lot of complex models that are being called. In this particular application for TSPA/SR, it is pretty complete. Those 122 AMRs which are supporting this thing are kind of fed in through about 30 or 40 AMRs that are the final leads into TSPA. In order to honor those 122, it had to be a fairly complete and, in fact, fairly complex integrated system. All those little arrows of how information flows ended up being fairly complex. Each one of those component parts could be boiled down to a more simpler representation. You know, SZ transport that Bruce will talk to you about tomorrow is a fairly complicated representation. UZ transport is probably even more complicated that Bo will talk to you. It could have been dramatically simplified and significantly reduced the computational burden, if you will, but in so doing, you would have cut the length a little bit and become a little less traceable back to the science that underpins it.

So, each application in this one because one of the main goals to that traceability back to the science, back to the data, back to the quality status of the data, was crucially important, you know, for TSPA/SR. So, it ended up being a fairly complex TSPA model. But, it can be simplified to the one that Mark Nutt showed you guys, I don't know, six or eight months ago.

WONG: Thanks.

ANDREWS: And, maybe, they will do that again, you know, this fall or next spring or something. I don't know what the plans exactly are.

SAGÜÉS: Yes. I'm trying to understand a little bit #27 which we have alluded to once. So, that's sort of a sensitivity to a given--and you're using the results of this to identify which parameters should be better characterized or what is the objective of this, first of all?

ANDREWS: Well, the principal objective was to figure out what drove the total spread of dose outcomes. What drove that six or seven orders of magnitude? What parameters that are uncertain were most driving it?

SAGÜÉS: With what purpose? To maybe change the design of the repository or of the waste package or whatever to make that--

ANDREWS: No, just to understand the significance of that particular--of the whole system, what significantly drove the performance.

SAGÜÉS: Okay. But, presumably, by understanding the significance, then you can do something about it?

ANDREWS: Yes, that's true.

SAGÜÉS: Okay.

ANDREWS: So, it is a feedback to the waste package folks and there were discussions about stresses at the welds and--

SAGÜÉS: So, this will allow people to tweak the design or whatever considerably or at least to find out what they can indicate in more detail to--okay. So, now, having said that, I was just trying to figure out the formula that was used or the question that was used to trace those curves. And, I figure what you do is you sit--for example, you sit at 100,000 years and then you see the whole spread of dose rates that you have and then you calculate, I don't know, some standard deviation of that or some such--and then, you go to the one parameter; for example, the Alloy-22 outer median general corrosion rate and you go ahead and you take that one and you see what is its evaluation and it's sigma, for example. Then, you would be doing like the ratio of the two sigmas and then you would--

ANDREWS: Essentially.

SAGÜÉS: I see.

ANDREWS: It's a little more statistics than even that though. That's essentially--

SAGÜÉS: Uh-huh. Now, the problem with that is certainly doing what the equivalent of what they were calling small sigma analysis. Like, what I'm saying is that sensitivity may depend on the absolute value of those things. For example, if there is more information that shows that the corrosion rate for Alloy-22 now is two orders of magnitude greater than what was before anticipated, now maybe there may be a much lesser number now, but it's importance may paradoxically become smaller because maybe it now has avoided dispersion.

ANDREWS: That's true.

SAGÜÉS: So, this is a very relative kind of--

ANDREWS: Yep, it's--that's why I started out--I mean, it's true. It is relative and it's relative based on, you know, the analyses and model reports that are directly feeding into the TSPA. Those distributions that are in there which--and there are distributions for stress dates and corrosion rates and MIC factors, et cetera, in the analyses model reports are in here; you know, not evaluating outside of those bounds. In the neutralization, we do evaluate outside those bounds, but for these, I'm sticking with the bounds that are in the input PDFs that have been given.

SAGÜÉS: Right. Like a forward sort of linear, if you will, then I think that this would have a much more absolute meaning, but if--

ANDREWS: Yes.

SAGÜÉS: --then the whole picture may change even with all the same problems?

ANDREWS: Yeah.

SAGÜÉS: Okay.

ANDREWS: I mean, an excellent point, you know, on this particular stress date. In the supporting AMRs, there are I believe three or four--I don't know if, Pasu, you're going to talk about this. There's different numbers. There's different models essentially of the uncertainty of the stress date right now in the supporting AMRs. So, in the TSPA/SR, those are treated as totally separate runs. I'm only showing you one here, but we have a whole family of alterative models for stress dates at the welds and the impact of those alternative models on the TSPA results.

COHON: Bob, I just wanted to emphasize something that you said earlier in your presentation about the role of judgment by modelers in developing the parameter distributions. As a demonstration of that, I'd like to look at 29, just briefly. First, I want to make sure I understand its implications. Do I read the graded case curve to--can I infer from that that no package fails before 8200 years or something like that?

ANDREWS: It's a little before that because the natural system has a few thousand years. So, it's, I don't know, probably 6,000 or 7,000 years, something like that.

COHON: And, when does the first package fail in the enhanced case which is off this curve because it's after 100,000 years?

ANDREWS: It's after 100,000. I don't know.

COHON: You don't know.

ANDREWS: We couldn't get that. I haven't looked at that result.

COHON: And, I can interpret this as--I'm tempted to infer from this that I've got an underlying waste package life distribution that says that there's an equal probability in my view of a package failing before 7,000 years and no package failing until after 100,000 plus years; it might be 300,000 years for all we know.

ANDREWS: That is statistically correct and that is a technically correct statement, but a bit misleading probably.

COHON: Oh, well, why?

ANDREWS: Because I go back to the previous slide--John, if you can go back to 28. We fixed in this case seven parameters at either their good or their bad.

COHON: Oh, good. Okay. Got it.

ANDREWS: The probability of hitting those seven parameters out at those two tails is $.05^7$ or whatever.

COHON: Is a much lower probability, right.

ANDREWS: Somebody with a calculator--a small probability. So, half of those ends are outside--

COHON: So, you only have a waste package-like parameter or distribution. We've got several other parameters distributions when taken together.

ANDREWS: When take together, yeah. Does that help?

COHON: All right, thanks.

ANDREWS: Okay.

BULLEN: Any more questions from Board members?

(No response.)

BULLEN: Board advisors? Rod Ewing?

EWING: I'd, at first, like to get some sense of the scale of the total calculation. So, I just have some quick questions. What's the total number of input parameters for this analysis?

ANDREWS: That are sampled?

EWING: No, just total input parameters?

ANDREWS: Oh, I don't know. 500, something like that. In parameter types, it's probably--

EWING: Fixed values sampled, all the input?

ANDREWS: The sample ones are those 240, right.

EWING: Right.

ANDREWS: The face values are probably, I don't know, 200 or 300. So, the total is 600, 700.

EWING: For the WIPP, I think it was 1500. So--

ANDREWS: We have a much simpler system than WIPP.

EWING: All right. So, 300 and then for those parameters sampled over a range, several hundred?

ANDREWS: Yeah.

EWING: Right? And, how many individual models in the total models of subsystems or their--

ANDREWS: I think you can--those little circles or the little lines here, I think, are probably as good a way of disparatizing it. So, it's probably on the order of 30.

EWING: Right. That was 25, but then you mentioned 122 analyses models reports. Are those separate?

ANDREWS: Well, those are--the 122 includes those 30 that are the final inputs to TSPA, but it also includes the other 90, if you will, that are process model and analyses understandings of each of the individual component parts.

EWING: So, just to be sure I understand, there's maybe 300 or 400 input parameters. Of those, approximately, half are sampled over a range?

ANDREWS: Yeah.

EWING: And, what percentage of the 300 or 400, do you think, are ultimately based on expert opinion? Is that a sampling over a range or--

ANDREWS: Let's break out this--we have to define what we mean by expert opinion probably here. Those that are formally elicited expert opinions are generally confined to the probabilistic volcanic hazard and the probabilistic seismic hazard. So, there's a seismic effect here and a volcanic effect here. Those where the analysts or modeler applied, in addition to data, applied some judgment, you know, to those data either extended the bounds or added some conservatism, I'm just going to take a--maybe I shouldn't even take a guess. You know, there's some judgment in all of them, but I'm not sure this judgment--who was that, Leon? No, Leon's talking again. There is some judgment in all of them, but I think the number that don't have--and, I think all of them have some data. It may not be project-specific data, but analog kind of information, but there's judgments--

EWING: Sure, right. Well, even expert elicitations are based on data to some extent. So, would it be fair to say that approximately half are sampled and that sampling over a range involves some judgment either by the analysts or expert elicitation?

ANDREWS: Yeah, okay. I think so.

EWING: And, changing from that, could we look at Figure 22? Right, thank you. You know, in your presentation, the variation between the 95th and 5th percentile, you equated that with, let's say, a qualitative measure of uncertainty. Right?

ANDREWS: Of the dose.

EWING: Of the dose, right. And, the fascinating thing to me to consider is that the uncertainty decreases with time if you think of it that way and that could be because certain radionuclides decay and are no longer important, certain processes are important or aren't over different periods of time, but is that a fair assessment of the uncertainty in the following sense? Actually, if you take a series of models, sampling over variables and extrapolate through time, in all of my experience the uncertainty should increase as a function of time.

ANDREWS: If all of those models were important, that would be the case. I mean, at the peak--and this is something we observed in the VA. It's not been new in this observation. But, at the peak, there's very few parameters, very few models that are really affecting the peak.

EWING: But, let's--you know, you have to educate me. Let me take a simple example, the weather. You have a higher probability of getting tomorrow's weather right than a thousand years from now. In other words, the uncertainty increases dramatically with time because the coupling between the variables and the range of variables that you may eventually get given a longer period of time, that range widens, right? So, why don't you see that kind of uncertainty in this analysis? Why doesn't it increase?

ANDREWS: Well, suppose weather was driven by the probability you were in El Nino. That was the main driver on weather, nothing else really--or which way the wind was blowing. If the wind was blowing from the west and yesterday was sunny and you're in Michigan it's probably going to be sunny tomorrow. Pretty high probability.

EWING: Right.

ANDREWS: That's what we have here. We have one or few parameters that are driving that peak. Abe wants to--do you want to add anything, Abe?

EWING: Just to continue, the decrease in uncertainty is in this analysis a result of fewer parameters being important in the models as a function of time?

ANDREWS: Yeah.

VAN LUIK: The only thing I wanted to add to the discussion was to say that one of Bob's slides made it very clear that he is showing the results of calculations that evaluate quantified uncertainties. There are also unquantified uncertainties and I'll get into that a little bit tomorrow. So, the unquantified uncertainties could actually give you a different spread on the outcome in the long-term. They could.

EWING: Well, I would maintain quantified or unquantified projected over time, I expect to see the uncertainty increase.

VAN LUIK: Except, as Bob said, the processes that are highly uncertain go away after a certain time and what's left then is the natural system variability without the additive of the waste package variability at the bottom.

BULLEN: I'm going to exercise some chairman's prerogative and give John Kessler the last question. Then, we're going to break so we can get done before 9:00 Eastern Time.

KESSLER: Bob, on this same figure and on 24 which, I guess, is the one before 10,000, at roughly the peak there where the uncertainties do decrease again, you'd mentioned that in some cases you substituted conservatisms for poorly understood uncertainty ranges. I'm assuming, therefore, that you're saying that there aren't any non-conservatisms in the current SR model. Yes?

ANDREWS: Uh-huh.

KESSLER: Okay. Okay. And then, the next question would be are there any significant conservatisms that are affecting the magnitude of that peak, as well as--and then, the same on 24, what are the major conservatisms that are affecting the pre-10,000 results for the volcanism, presumably?

ANDREWS: Okay. That's an excellent question. I'm glad you had a chance to ask that because I probably should have gone into that as I was going through it. On 23 on the nominal case on the conservatisms that are--oops, 22, sorry--that are affecting the peak and this is something observed also in the VA, you know, the solubilities or secondary phases that can form when the fuel is altered and the degradation characteristics of those secondary phase, secondary uranium phases, can be a very significant contributor to long-term, in fact, peak dose performance because the neptunium solubility is not this which is different than the VA, but is significantly lower than that because of the secondary phases. In the VA when we did those secondary phase analyses, it was essentially reducing the peak by about a factor of between 10 and 30. I mean, 10 seems kind of like the best estimate number. On those secondary phases, it's one of the things I have in the table on the spent fuel degradation. The secondary phases are not included in the nominal TSPA/SR model and that is a conservatism. You know, it's about that factor of 10 or so conservatism.

This is also driven by the amount of water that seeps in, and therefore, contacts the waste at those particular times. The amount of water that seeps in is quite uncertain. It's variable and uncertain. There are some--and I don't know if Bo is going to talk to it, but some recent indications that would indicate that perhaps we're a little conservative on the seepage representation that we've incorporated in here.

So, you know, those two aspects is really, because it's driven by neptunium and driven by a solubility-limited release, it's impacted by those two parameters, those two component piece parts.

On the volcanism one, the conservatism more lies in the degradation characteristics of the engineered barrier once the event occurs. Right now, there's no credit. Once the event occurs, there is no credit taken for any of the engineered barriers for the package, for the drip shield, or for the cladding.

That's a fairly, you know, conservative assumption. But, most of the other assumptions that really impact peak either volcanic peak or nominal or base case peak, they don't impact as much. I mean, so these three or four things are dominating conservatism driving the peak doses.

BULLEN: Thank you, Bob. In the event of time, I think we're going to have to wrap this session up. Before I close, I guess, I should point out that maybe my speech reading is an indication of why we should elect the speech writers instead of the politicians when they give their speeches.

I will call a break right now and I would like everyone back here in 12 minutes which is going to put us at 4:40. We're going to be out of here by 6:10 which is 9:10 Eastern Time.

(Whereupon, a brief recess was taken.)

BULLEN: I actually waited to make these remarks because I want the Board members to hear them explicitly as a reminder of how the last three sessions of the day or three presentations of the day are going to go. These presentations are met to cover a lot of details. We have a very limited amount of time available which means we're not going to get out of here until 9:15 Eastern Time. So, I want to limit the questions from the Board members and perhaps from the experts if we have time to questions of clarification only. So, the three presentations today and the four additional presentations tomorrow will be followed up with a panel discussion at which point we can go into more details. But, I wanted to give each of the presenters 30 minutes as an opportunity to present the details that we've asked them to present.

Our first presentation is by Bo Bodvarsson from Lawrence Berkley National Laboratory. He's the lead for the Yucca Mountain Project and the Nuclear Waste Program for the

U.S. Sciences Division at LBNL. His research includes geothermal reservoir engineering and nuclear waste disposal. He also is the lead for the unsaturated zone. Bo has a new microphone coming right up.

BODVARSSON: I'm going to talk about the unsaturated zone flow and transport models and I'm going to try to use this thing here. Next slide, please?

And, these are the some of the models I'm going to talk about. Then, I'm going to put them on the side here also so that you can see them when I go from one to the other and you don't be too confused. There's a lot of models here starting with the climate infiltration models. Then, we go into flow models and the thermal effects on flow. Then, I'm going to go into seepage models and then the thermal effect from seepage and thermal-hydrological-chemical effect on seepage, and finally end up with the transport models. All of those feed TSPA. Next slide, please?

So, these models--this is a slide from Bob Andrews. This kind of lists the model that we use for TSPA. The climate models provides climate states and the timing and net infiltration, infiltration rates, unsaturated zone flow, flow fields, coupled effect, percolation flux affected by thermal effects, seepage into emplacement drifts, seepage flux, percolation flux, functional location, waste type, time, and climate, coupled effects on seepage. This is a very popular topic with the seepage flux and seepage fraction as a function of percolation flux. And then, how it's affected by thermal effects. Next slide?

Final model is the transport models and they are used directly also in TSPA and these are the main things that we use, main parameters. They use the flow fields from the flow models, they use fracture apertures and spacings, Kd because you use the Kd approach, matrix diffusion, colloid parameters. Next one?

So, let's start with the first model. That first model is two models, climate and infiltration. I'm going to go through them fairly quickly. I'm going to tell you for all of them the objectives of these models. I'm going to tell you what they'll be used for, what the results are, then the uncertainties and some of the important factors that dominate the results of these models.

Start with the climate, we look at climate for 10,000 years and then after that for 100,000 or longer, the estimate mean, upper and lower bounds. Just like precipitation just like Bob mentioned, that provides input to the infiltration model. Then, infiltration model uses all these processes, surface processes and near-surface processes, to estimate spatially-distributed time-averaged estimates of net infiltration. And, that, of course, is used in the UZ flow and transport model as a boundary condition on the surface. Next slide, please?

The main assumptions are a climate analysis based on examining paleoclimate records and the climate is cyclical with several alternating glacial and interglacial periods. Infiltration assumptions, model infiltration through root-zone only and then it uses a simplified "bucket-model" which is a fairly large assumption. You see here in this graph some of the main issues regarding climates, the location of some of the sites used for climate estimates, for glacial transition, monsoon and modern and then some of the results; same with the infiltration model. Next slide, please?

Some of the results for the climate, we have modern, monsoon, and glacial transition. These are the time durations and these are the mean precipitation rates based on the climate AMR of Joel Forester. Infiltration based on these climate states, you have mean infiltration rates, modern 4.6, monsoon 12.2, and glacial transition 17.8mm/yr. Next slide?

Uncertainty, of course, there is a lot of uncertainties in all these models as everybody understands. The uncertainty climate is not directly used in TSPA, but used through infiltration and the effect of UZ flow model. There is substantial uncertainty in climate changes, the time periods, and of course, the magnitudes of precipitation. Infiltration is included indirectly as a boundary condition.

Monte Carlo simulations by varying important parameters in infiltration models are used to get the variability of this model. Of course, the bucket model approximation is an important uncertainty. And then, the weight climate scenario histogram which is shown here for sampling in TSPA simulations. Next one?

Next one is UZ flow. So, we have done climate, we have done infiltration. They feed the UZ flow as a boundary condition on the surface and now we want to calculate the flow of water through the mountain and through the saturated zone at the bottom. Objectives of the UZ flow model certainly is to integrate all the available data into a comprehensive 3-D model. It is to develop submodels and quantify the flow of water, flow of gases, and flow of heat in the unsaturated zone, and provide, of course, TSPA with three-dimensional steady-state flow fields. It should be mentioned here that UZ flow is not abstracted very much. We basically take directly the three-dimensional flow fields and use them directly in total system performance assessment.

You see here some of the main issues regarding UZ flow with regard to variation in percolation flux, PTn effects, flow through vitric zones, lateral diversion, perched water issues, and things of that sort. Next slide, please?

Major assumptions. We use dual-permeability model with Darcy's law and Richards' equation. We use the geology based on the geological framework model. That, of course, is approximated, the geological data, from all of the boreholes and the tunnels. We assume that the ambient unsaturated flow can be approximated by isothermal steady-state flow fields. Steady-state flow fields, we don't use transient flow fields. And, perched water occurrence is due to permeability barrier effect is another assumption. This is the mountain, this is the extent of the model, this is the model as it is currently with the repository horizon right here. Next slide, please?

Results. Some of the results from this model are we've done a lot of calibrations against a bunch of different datasets including pneumatics, saturations, temperatures, et cetera. We have developed several submodels to look specifically at big important issues like faults and PTn in Calico Hills in perched waters. We then calculate percolation fluxes at the repository and these are used for the seepage model, the variability and percolation flux at the repositories, and fracture and matrix components of flow based on different climate states. Most important factors/ conservatisms/optimisms are the surface net infiltration rates are very important, obviously. The heterogeneity of the hydrogeologic system is very important, as has been pointed out by various members of the Board. And, we are finding more and more the importance of faults that the

results, for example, of flow and transports are very much dominated by assumptions made regarding properties of faults.
Next slide, please?

Now, we go from the flow model now to the thermal effects on flow which is basically we take the basis three-dimensional flow model and we add heat to it and then we compare the flow patterns in the three-dimensional model with and without the effect of the repository, the effect of the repository heat. So, objectives of this one generally is to evaluate the effects of heat on liquid and gas distributions to see if we need to modify the three-dimensional flow fields to take into account the effect of heat. To evaluate global large-scale temperature changes such as how much does the temperature of the perched water rise, what are the effects of boiling on the perched water, how much does the Calico Hills and the water table rise in temperatures and things like that. Here on the right hand side, you have a location at the repository. This is typical temperature sequences, time sequences. You start off with the geothermal gradient and at times zero, you put in the heat load, the thermal load ventilation, and it starts to heat up, starts to heat up, it starts to heat up more. It boils around here. Then, it starts to cool down more and more. And, you see, of course, maximum temperature around 96 or a little bit higher. In low infiltration areas, you'll get peaks that's significantly higher than the boiling point for water getting up to 120 degrees Centigrade at the drift walls, something like that.

What you see here then is over all of a bunch of different drifts, these kilometers and we have a bunch of drift here. This is a cross-section and it shows how the temperature rises and increases to a boiling temperature except at the fuel location with infiltration rate so small that it's not sufficient to cause continuous boiling, but rises above that to super heated conditions.

Assumptions, uniform heat distribution at the repository, ventilation removes heat only, constant flow properties in layers, no hysteresis effect. Hysteresis may be very important for thermal loading issues because you change the saturation and matrix block very significantly. Fixed temperature at the ground surface, this is a good approximation, and one kilometer into the saturated zone. You have to model this below the water level, obviously, water table. The modeling approach is supported by geothermal analogs and drifts scale heater test. Next slide, please?

Results shows the following two-phase zone around the drifts is generally confined to 10m to 20m. Temperature can get higher than 96 up to 120 degrees close to low infiltration areas. Temperature at the midpoint of the pillars between drifts is 80 to 85 degrees; 70 to 75 degrees in the Calico Hills and there we worry about zeolites and the effect of temperature on the sorptive capabilities of zeolites; and, it rises to 67 to 70 degrees at the water table. Liquid flux and the high fracture permeabilities, this is a very important point here. High fracture permeabilities allow for easy and rapid drainage in pillars between drifts, and I will come a little bit more to that a little bit later. Liquid flux towards drift may exceed a very large value, as has been discussed before, because when you dry out the soil around the drift, you create tremendous capillary suction or water coming towards the drift and that can generate very large fluxes. Of course, very little or any of that is going to go into the drift; it's all going to be vaporized.

Factors that may impact predictions of works like this is lateral variation or properties between layers, focusing and channelizing of flow, changes in long-term distribution of surface infiltration and effects of climate, and certainly the fact that on a mountain scale thermohydrology you do not have a large-scale test that can give you confidence in a large-scale model like that. The largest test we have is the drift scale model over 80 meters. Here, we are talking about a mountain scale thermohydrology model. So, the only confidence builder you can get is really geothermal analogue studies.

What you see here on the right hand side is selected five drifts and this is a rather complicated time history of fluxes between the pillars. The reason it's so complicated is because during this period not only are we boiling water and remobilizing it and moving it around, we are also changing the climate state going to better and better climates that results in higher and higher fluxes. Now, if you look at this carefully and look at the AMR that backs this up, you will probably reach the same conclusion that TSPA and us process modelers reached and that is the thermal effects on flow are very small on a large-scale global sense. There is more effect from seepage, but less on large-scale, three-dimensional flow in the mountain. Therefore, it was neglected in this TSPA/SR. Next slide, please?

Now, we are going into the seepage route here into the seepage model that, of course, uses the calibrated properties models that calibrates all the properties in the mountain, uses infiltration scenarios in the flow models, but it's concentrated on predicting how much water will seep into the drifts. As you can see here on the right hand side, this is a highly heterogeneous model and Priscilla's question was very good. We need very much when we look at flow and transport properties, we need to look at the variability and heterogeneity in the formation which is extremely important.

And, I think this model is truthful to or obeys all of the air injection data around the niches, and is calibrated and constrained and based on those datasets. If then it's calibrated against all the seepage data where we put water above the drift which is included in this model and calibrate how much goes into the drift, that is the fraction of water that enters the drift as a function of the total amount applied.

The objectives then are certainly to determine the fraction of waste packages affected by seepage. This is an important parameter for PA. Determine seepage flux at the inner locations as a functional--percolation, flux, climate, and based on different other things. Major assumption is that we use the heterogeneous fracture continuum. We use flow focusing; that is to say we assume that there's not uniform flow at different locations, but flow tends to focus, to be more conservative. We assume no way of operation or condensation within the drifts after post-closure which is a conservative assumption. We include explicitly partial drift collapse. We also include the effect of permeability enhancement due to mechanical changes in permeability next to the drift because that comes out of the calibration exercises. And, we have large variability in parameter uncertainty because, mainly, we have only tested seepage in the middle non-lithophysal. We have not tested in the lower lithophysal 75 percent of the rock which we are doing now or the other deeper units. So, we have a large variability in uncertainty. Next slide, please?

Results. The seepage-relevant parameters, like I said, are determined by seepage experiments called mix experiments and we calculate seepage fraction and seepage flux for a large range of parameters. Let me explain this a little bit. We cannot measure seepage as a function of all possible percolation fluxes because the flow through the mountain is very slow. It will take us years and years and years to do mix experiments at low fluxes. We, therefore, do them at higher fluxes, calibrate, and then extrapolate the models to give us response surfaces, a figure like this that Mike Wilson of Sandia developed which shows percolation flux and a seepage fraction or a seepage rate, a mere two per year, as a function of this very important parameter, permeability of the fracture system divided by Jack Bailey's favorite constant, the Von Knuckten factor (phonetic). So, this is a very important plot that I'll come back to a little bit later.

The factors that must control this behavior are percolation flux, the assumed focusing effect that we assume to be conservative, the effective capillary strength of fractures, and the fracture permeability. Let me just tell you something now about this. Mark Peters in his presentation this morning told you that the effective permeability of the lower lithophysal which is the main repository rock is an order to an order and a half higher than that of the middle non-lithophysal. You also saw from his picture that the dye around the borehole went in a circular fashion and not in a vertical fashion like it does in the middle non-lithophysal. What does that mean? That means that when it goes vertically down, as it does in the middle non-lithophysal in our studies in the ESF, that process is gravity driven. The capillary suction in the fractures is not sufficient to overcome gravity. It wants to float down. The gravity forces exceed capillary forces. The dye results that we see in the lower lithophysal show much stronger capillary forces because there are much more smaller fractures that are interconnected with much more suction and that overcomes total gravity forces.

Why is that good? That is good because that means that this one knuckten-alpha value which is the measure of the capillary strength of this medium is approximately, based on our estimation, an order of magnitude higher for that medium than it is for the middle non-lithophysal. So, that makes this term two orders of magnitude higher than it is now for the middle non-lithophysal which goes into the zero seepage rate here by two orders of magnitude. So, this is very promising results from a very limited study. And, I was going to warn you about that. One caveat about that is that we haven't done enough seepage studies that, even though the fracture permeability data are very promising, the alpha parameter is very promising, we do not know yet what the lithophysal cavities are going to do to the overall seepage. So, that's what we are testing for now.

Conservatism/optimism. We use, we believe, conservative parameter values and I think Bob Andrews mentioned that before. We mentioned this before. We ignore ventilation/evaporation effect which is conservative. WE ignore in-drift condensation. That may be optimistic. Next slide, please?

Now, we are going into the thermal-hydrological-chemical. This is a slide that Leon asked me to add about again these 5m above the drift so that the Board can practice on all of us and give us all a hard time. This is my little version about this. The percolation flux 5m above the drift is used in the seepage model and nobody has shown this curve. This is actually how it looks like. This is the 100m that shows a lot of percolation flux. Then, with time it goes to higher and higher values. The 5m above the drift is a fairly operative value, as you have mentioned. We just recently did studies that are plotted out; .2m, I think it was, 1m, 3m, 5m, 7m, 9m to kind of bracket it, and as far as I'm concerned, this is fairly conservative with respect to that.

But, the points that the Board makes and I tend to agree with and I may not be popular for this and that is the following. You take this pulse of water from a homogeneous model. You don't take it from a heterogeneous variable property model that is based on actual data from the science. Therefore, how can you justify that you're conservative? What is the basis for this? I think that's a very valid point.

What we are doing now is to do a stochastic variability in a permeability field that shows hereafter 100 years with stochastic variabilities and permeabilities on the right hand side here looking at this effect and how much it can be decreased. So far as everybody has guessed, there is no seepage with about three or four realizations. We need to do a lot more with this.

Another point I want to make here and that's the following. You look at the scale here and the fact of the matter is that based on this model results, you never get saturated conditions in the fractures except at greater localized conditions because the permeability is so high you--basically, right when it increases mobility of the water, the model just flows down. It just flows down very rapidly in the fracture system. Then, whatever is above here and condenses, it wants to go down again, but it can't, gets revaporized, mobilized, and then eventually flows laterally.

And, I think we ought to show when the study is done is that we're using Monte Carlo simulation and stochastic variability and that this may not be very much of an issue. Next slide, please?

NELSON: Wait. Can you just say what SL is?

BODVARSSON: Saturation of liquid in the fractures. This is the liquid saturation in the fracture medium. Zero means that this is all dry. There is no single drop of water in the fractures next to the drift. This means that there is enhanced water in the fractures. The ambient in this unit, you have different colors and different units initially. The ambient saturation of the fracture is roughly 3 or 4 percent and it increases to some 10 percent that allows it to flow laterally and most of the drainage occurs on 10m away from the drift based on these realizations.

SAGÜÉS: But, what is the drift? What is the wide rectangular feature in the--

BODVARSSON: That's the drift. This is the drift. But, you look at the aspect ratio. The aspect ratio is very bad. So, this figure it not very good. But, basically, it is a 5m drift just like a regular 5m drift.

SAGÜÉS: Okay. Is it simulated by a square cross-section?

BODVARSSON: Yeah, it's simulated by a square cross-section which is very conservative, too, because that allows water to go in it easier, but it has no effect because of the boiling zone around here in this case.

Any other question on that to clarify that?

NELSON: Is that result for non-lith?

BODVARSSON: This is the result based on the--no, it's not from non-lith. This is the middle non-lithophysal because we have most of the permeabilities there. When we get to permeabilities from the lower lith, then we will use that. Good question.

NELSON: Okay. In which case, there is matrix porosity which can be saturated or closer to saturation outside. This is just the fracture saturation?

BODVARSSON: Yeah, yeah, yeah. You're absolutely right. There could be water in the matrix around here that is not fully dried off, but the simulations show that most of the water around here, even in the matrix block, is also dried off because of boiling.

Next slide? Now, I'm going to go--sorry about that. Now, we're going to go into THC model. As you know, Bill Glassley made a presentation of this kind of model before. The objectives of this model is to predict the chemistry of water and gases that will seep into a drift and evaluate changes in hydrological properties due to mineral precipitation/dissolution and this is what Bill discussed a lot in his talk. Calibrate/validate model using the chemical evolution of the drift scale test. So, we are calibrating this test against the result of the drift scale test and then we predict what we get and use for PA.

Dual permeability assumptions, initial water chemistry, geochemical systems considered is adequate for the ambient system. And, that goes to the question of how many minerals do you need to use? This is just some of the main features of the model that is shown here. Next slide, please?

You have seen these results before. Mark Peters showed them. We are quite pleased that the model is able to represent all of the changes in chemistry in the drift scale model in a very reasonable fashion without calibration and I want to emphasize this. There is no calibration here. This is a prediction without any calibration. And, it predicts very well the increases in CO_2 content and the decreases in pH that agrees with the observed data.

The main results from this model, predictions over the long-term, were as follows. pH will vary between 7.5 and 9 on the average. We will not see a lot of salts close to the drift wall like has been hypothesized and simply because the water is so dilute, you don't have enough salt in the water to accumulate next to the drift even though you boil the heck out of the water. There's just not enough salts in the waters, very dilute water.

The porosity and permeability changes over the first 10,000 years are small and the effects on flow fields is minimal. I want to say a few words about this. I really feel strongly that the fracture porosity is a key to thermal-hydrological-chemical processes. I feel more strongly that it's more key than the mineral assemblages that are used or the kinetic data. And, why do I say that? We get the best results from this model using "a simplified mineralogy" with just the essential clays in the feldspars and zeolites present. When we go into more complex 25 minerals rather than the 10 or 12 used as the base minerals, the comparison is not as good to the data. And, why is that? My view is that the limited knowledge about thermal chemical, thermodynamic processes of multi-species medium are very limited and I recall one experiment on silica precipitation done at Menlo Park. They thought it was pure silica in the water and they ran it through a core and the core sealed up like crazy much sooner than they expected. After a huge amount of work, they figured what caused it was a tiny, tiny concentration of aluminum in the water. Now, this just is a very simple system. Anyway, I'm generally in favor of simplified models. Next slide?

Conservatisms/optimisms. Initial water chemistry, of course, we feel is rather conservative because we use more concentration water than is actually present. We also think that perhaps the permeability and porosity changes could be underestimated because of more localized mineral precipitation and dissolution. That's where we want to exercise this model against the lab experiment like I told the Board before. Next slide, please?

Finally, UZ transport, this one right here. Objectives, develop a model to investigate radionuclides transport and this shows--this is a mountain scale model. This shows at the water table after some time the radionuclide presence. And, you see when you look at these figures and you look at the AMR very strong effects on faults. So, they really dominate the behavior of the radionuclide transport models to some extent. Flow components, the same as the UZ flow model. Governing equation is already known. Next one?

Result/important factors. Faults dominate, matrix diffusion and sorption are very important. The plutonium 239 decay chain products are very important. They need to be considered in TSPA and I think they are. The plutonium 239 goes to uranium 235 and something else. And, uranium 235 has a long half-life, and therefore, they need to be considered. Colloids transport could be important. It is very hard to say if they are or not because it depends so much on the filtration process and what you assume regarding the size of the colloids and things like that.

And, finally, the current PA transport model may be very conservative. I want to mention that a little bit. There are two figures here. This figure shows the transport of technetium which is a conservative species for the three climates; the upper bound, lower bound, and the mean climate. You can see the breakthrough curves at the water table based on these different climates. This lower one compares the current PA model which is shown here, the FEM particle tracker model to another partical tracker. There are very large effects here due to different assumptions regarding dual-permeability versus dual-porosity case. We think this is fairly conservative and we are now working with PA to try to fix this, to make it less conservative. Next slide?

Now, I'm done with what I know and now I'm going to talk about the TSPA results which I don't know anything about. That should be interesting. I'm going to talk about the sensitivity studies to this 95 percent and 5 percent cases. Dan's question was very good, what does it all mean? Priscilla's question was very good, what does it all mean? And, sometimes, I don't understand it well enough, sometimes. But, I'm going to go through it anyway.

Let's take infiltration barrier sensitivity analysis first and this is two cases, degraded barrier and enhanced barrier. And, this is simple enough; I can understand this. We take in one case high infiltration case throughout the model and lower infiltration case throughout the model in the other case. That's fairly simple.

So, let's see the results. This is what you see. This is Bob's TSPA curves and what this shows us is that even if we make it a lot wetter with very high infiltration, it really doesn't get any worse. If you make it a lot drier, it makes substantial difference on the order of order of magnitude at 100,000 years. Why is that? One explanation is that the mean infiltration for this case is about 18mm or 19mm/yr, and the high one is something like 38. So, it's a factor of 2. But, the low one is only about 2 which is a factor of 10 times less. Therefore, it should make a lot more difference since it's 10 times less than 2 times more. Does that make sense to you? Now, it makes sense to me to a certain extent. Now, let's go into this a little bit more.

Certainly, these releases here at the early time must be technetium and iodide. Those are the only ones coming in at an early time, but what happens here, I'm not really sure about because this should then actually not becomes so important anymore because you will see on a subsequent slide that the retarding radionuclides don't get improved at all. It only seems to be the non-retarding ones. So, I can't explain that to a large extent, but we'll learn more about this later on. But, there is significant improvements there. Next slide, please?

Now, we go into the seepage barrier sensitivity, degraded barrier, and enhanced barrier. This again is the 95th percentile that is chosen for a couple of factors here. One is the flow focusing factor and the other one is that graph you saw, Mike Wilson's graph, on the uncertainty analysis and seepage as a function of percolation flux and the K over alpha variability. Let me tell you what I know about this or understand about this and then Bob can correct me and all the rest of you.

That is when you do this 95 percent and 5 percent analysis, there are several things that enter the picture. (A), if you see a lot of difference in the curves, that must be an important parameter, right? (B), if you see a lot of differences in the curve, this could only also reflect the uncertainty in that variable. If 5 percent and 95 percent is pretty much the same and there is no uncertainty, the curves would overlap, right? Number 3, it also depends on how that model is used with respect to other models, conceptual models in TSPA. Because if you're very conservative with respect to one model so the effect of this model doesn't count, then that must be also looked at. So, there are several complicating factors here.

So, let me go into the next one which is--

COHON: Bo?

BODVARSSON: Yeah?

COHON: I'm sorry. What is the 95th percentile seepage flux? Do you happen to know the number?

BODVARSSON: No. Let me answer it this way. I think that in the repository block about 90 percent of the waste packages don't see seeps, about 10 percent will see seeps. And, I guess, that's in the area of the high percolation flux at the crest. You'll correct me if I'm wrong, Bob. That must be the area where you have high infiltration, we attempt to focus flow, and we get a lot of seepage. A lot of the packages will not see seeps under this scenario.

So, I can't really answer it. So, my guess would be and again, Bob, you'll correct me, if you take 5 percent and 95 percent seeps, I would say the 5 percent would be that perhaps 5 percent of the waste package will see seep and maybe 15 percent for the 95 percent case? Because I think what we are seeing here is not really the variability in how many respects to you see seep. That's my problem in one extent. It's more what do these factors show?

So, sorry. Do you want to explain it a little bit better, Bob?

ANDREWS: I don't know. We'd have to look at the actual intermediate result distribution which is I think what your question was. How much do you really change the seepage or the seepage fraction when you change these input parameters. And, that's the kind of intermediate result. And, as I said earlier, we're starting to look at these and these kinds of questions of, okay, this is the change. Bo is going to show you the change in dose, but what led to all of the steps that led to that change of dose are still being analyzed right now.

COHON: Right, right.

BODVARSSON: Next slide, please? Now, this show the three cases. One is then the degraded barrier which it shows here and enhanced seepage barrier which is shown here. To me, this is fairly small changes. If I were to explain them, I would do it in the following way and again Bob will correct me. I'll explain in the following way.

If 90 percent of the waste packages show no seeps and then you vary the flow focusing factor or something on this graph, so maybe going from 10 percent to 5 percent and 50 percent, you are not really evaluating how important seepage is. You are evaluating what is the uncertainty of the seepage for the conditions that you have. To evaluate the importance of seepage, you must seep on all waste packages because that tells you the effectiveness of the seepage barrier, if I understand this analysis correctly.

Therefore, I would conclude in my ignorant way that the reason you don't show anything here is because you're not increasing a lot of the waste packages that seep; you are basically having fewer seeps which is the red curve because of the flow focusing factors and more waste packages seeing seeps in this blue one. And, if you have more water coming in, you are worse off is my explanation.

Why doesn't it make a bigger difference? There are several other possible reasons. One is that seepage--the waste package corrosion rates do not depend on seepage. They don't depend on seepage, at all. The waste mobilization and waste form degradation doesn't depend on seepage. That depends on the relative humidity and the pH depends on seepage, but the waste mobilization does not depend on seepage.

The transport in the invert and this comes back to other conceptual models. The transport in the invert is diffusion dominated even if you have seepage because of very conservative diffusion approximation that have diffusion at the bottom of the drift as a zero concentration boundary and, therefore, you have huge diffusion. So, whatever seeps doesn't affect the EPS, as Ernie will show you a little bit later on, because it is all diffusion dominated. Those are some of the reasons. I don't know. You want to--I don't know. That's what my knowledge is. Next one?

What I proposed to PA actually was to do a case where you actually put seepage into all of the drifts so you can see the effect of seepage. UZ transport barrier sensitivity analysis and this again we did the 5 percent and

95 percent. 5 percent were Kds, 95 percent were Kcs to maximize, therefore, the colloid transport. 5 percent for matrix diffusion or 95 percent for fracture apertures. All of this is bad for transports; all of this is good for transports. So, your question before, Dan, was that if it is 5 percent and if it is bad, you make that with the 95 percent. Next slide?

What this shows here, if I understand this correctly again, is that you have in the degraded case, you have americium colloids giving you a high dose initially, but since the half time is only 7,000 years, it disappears after some time, and then you go back to the baseline. And then, later on, because the Kds are lower, you get a degraded UZ transport barrier. But, like I told you before, when you look at the infiltration, we see no improvement in transport, at all. Even if you enhance the use of transport by increasing Kds or getting rid of colloids, there's no effect. Partly, the reason for that is, I think, the very conservative, perhaps, transport model that we use that could shift these curves considerably down when we use a more realistic transport model. The PA is currently working. That is up there, Bob? I don't know. Next one?

Now, I guess we do both of them, degraded UZ transport and degraded infiltration barrier. So, I assume that we take high and low value infiltration and assume that we take high or low values of Kds. We throw out the colloids or we add the colloids and off we go. Right? Next slide?

Now, that shows very similar things to what we showed before. If you remember correctly, lower infiltration gave us better performance, but it didn't affect transport. And, you add them together, again, you have better performance because the lower infiltration, the transport doesn't do anything for you. Same thing with the red curve here. If you remember correctly, increasing infiltration didn't make it any worse, but again having colloids and reducing Kds did to some extent early-on. That's what this curve is. So, it's basically to me a super position of two effects that is clearly unrelated.

I guess, that's it. Any more?

BULLEN: Thank you, Bo. Now, at the risk of asking this question, just for clarification purposes only, are there any questions from the Board only?

SAGÜÉS: Okay. I'm sorry, but I want to go to #16, please. Okay, thank you. First of all, the time scale there, where is--the zero years will correspond to the moment in which the repository is closed or is--it looks like the curve starts at exactly 50 years. Is that--

BODVARSSON: Oh, no. It's--

SAGÜÉS: Yeah, when does the time scale--

BODVARSSON: The time scale starts at zero, right here.

SAGÜÉS: But, what is zero, the moment in which emplacement has finished and the--

BODVARSSON: Is that right, Bob? This is a TSPA curve. I guess, zero would be the time when you close--or you start the ventilation. Right? You start by putting the waste emplacement times zero, right?

ANDREWS: Yeah, and then ventilation is a 50 period and then ventilation is turned off.

BODVARSSON: Right.

ANDREWS: So, I think things start after ventilation is turned off.

SAGÜÉS: Starts after ventilation. But, then, that's when the drifts begin to warm up, correct?

BODVARSSON: No. Well, they are still warming up here, but you know, because of ventilation, you remove a lot of heat, but they are still warming up.

SAGÜÉS: But, there's not a lot of--I mean, there is a lot of water displacement in that period. Indeed, there would be water being removed from--

BODVARSSON: Right. It's because there's a lot of heat removed. So, basically, a lot of heat starts to enter the picture right here. You're right.

SAGÜÉS: Okay. So, now, then you close the drifts and this begins to heat up now and then you stir moving water out from the surrounding of the drifts?

BODVARSSON: Yeah, of 5m. This is a 5m location above the drift.

SAGÜÉS: Uh-huh. Now, does that mean you still--does that flow downwards then?

BODVARSSON: Yeah, yeah. Here is the drift. It is capillary driven towards the drift, yes.

SAGÜÉS: Uh-huh. So, even though the drifts are beginning to get real hot, you still want to have all the time in net downward flux? You never drive water upward from the drift?

BODVARSSON: No, no, no. It's because what happens when you dry out around the drift, you dry out the rock. So, the capillary suction becomes more and more and more. And, therefore, water is flowing towards the drift. Is always going towards the drift at all times. The gas phase is always going away from the drift at all times because the gas pressure increases. But, basically, the water flux is always towards the drift because of capillary suction.

SAGÜÉS: Uh-huh. So, this is going to be very rudimentary. So then, the heat doesn't dry the environment around it?

BODVARSSON: No, what happens is this is the flux. This is the flux here, okay; 5m above the drift.

SAGÜÉS: Yeah.

BODVARSSON: What really happens is it goes in here and will all be vaporized. It moves up, it moves laterally. Some will go back down again, vaporized again, but eventually all flow around the pillar.

SAGÜÉS: And, that evaporation boundaries, they are pretty close to the--I mean, if I were to go only like one foot over the surface, then in that case, I wouldn't--

BODVARSSON: No water. You see no water. You see no water there.

SAGÜÉS: No water. So, what is the break point?

BODVARSSON: This will--what?

SAGÜÉS: What is the break point, 1m, 2m?

BODVARSSON: Well, it depends on time because the boiling sort of moves outwards with time. And, you start with any location. Like 1m above, you have capillary suction of water going towards the drift starting earlier around 5m. Then, the boiling point moves out. Then, at 5m that we're showing here, it is this location. Closer to the drift would be here. Further away from the drift, it would be here.

SAGÜÉS: But look, for example, at the heated drift test. Right now--I don't know certainly where we are right now, but we are pretty--we're dry for a couple of meters over the drift now, right, or not?

BODVARSSON: Yeah.

SAGÜÉS: But then, the net flux of water, I guess--the next flux of water is up, not down?

BODVARSSON: No, it's always towards the drift. You're talking about drift scale test or--

SAGÜÉS: Yeah.

BODVARSSON: Yeah. See, what happens is this. You know, water moves towards the drift, but steam after it boils moves away. That steam condenses here further away. So, maybe, that is what you are thinking about is that when the water is boiled, steam moves away, moves outwards here, and it condenses and forms water here that then wants to flow towards the drift again. Is that what you're thinking about?

SAGÜÉS: Yes. And, I'm just trying to figure--so, forget about the seep flow and just get whatever--let's get number of moles of water per unit time per unit area net. Is it moving down or moving up?

BODVARSSON: It's moving down here, it's moving sideways here, it's moving up here all towards the drift. The net component if you take the whole system, net component order is there. This is where the water flows. But, locally, flows like that; steam flows out, condenses, some of it goes like that, some of it wants to move again, and steam vaporizes, moves off and condenses again, and eventually a global effect is this. The small scale effect occurs close to the boiling region.

SPEAKER: When you're saying water, though, you mean--

BODVARSSON: Oh, you mean, liquid--

SAGÜÉS: Water as--liquid plus gas, yeah. The overall flow of water is--

BODVARSSON: Oh, you mean liquid and gas?

SAGÜÉS: Yes?

BODVARSSON: Just the water compound?

SAGÜÉS: Yes.

BODVARSSON: Oh, the water compound--the net effect of water compound is away. Steam flow is more than liquid flow.

SAGÜÉS: Okay. So, you're saying that there is a liquid--there is a liquid flow of water downwards, molecular water, and that water goes even in areas where we're above boiling, where dynamically--no, that doesn't make sense.

BODVARSSON: It goes into a hotter area. It boils off and then steam goes out.

SAGÜÉS: Okay. And then, some of the water makes it all the way down to the drift even though the drift is, for example, 200--

BODVARSSON: No, I don't think any other water will get going through drift.

BULLEN: I'm going to exercise some chair prerogative here and take this conversation offline and you guys can do this outside the room. We need to finish the last two presentations and I'll ask one last time are there any questions of clarification?

(No response.)

BULLEN: Seeing none, thank you both. By the way, Bo, you will be back tomorrow and you'll have your viewgraphs. When we have the panel discussion, we can delve into this in a little more detail and perhaps you could talk offline with Alberto or other Board members.

Right now, I will just introduce Dr. Ernie Hardin and state that he's going to talk about engineered barrier system supporting models and analyses and leave it at that.

HARDIN: I have the privilege of presenting to you work done by some 15 or 20 people, too numerous to mention, but I'll try to call out some of the contributors as we go through the material.

This is sort of a table of contents of this presentation. I'm going to talk a little bit about how we predict relative humidity and seepage during the thermal period, how we predict water composition in the drift, touch on flow loads and breaches in the drip shield and waste packages, talk about EBS radionuclide transport. That would be primarily the diffusion barrier. And, give you a brief overview of the TSPA abstractions that are used to control the in-drift environment, and finally, touch on FEPs. We have the opportunity to talk about two of them here. And, after that, we'll talk a little bit about uncertainty analyses and I have a couple of Bob Andrews' sensitivity slides. What I do not have time to talk to you about today are microbial effects in the drifts, effects of introduced materials, such as cementitious materials that would be used in ground support, and the production of colloidal iron in the drifts from the use of iron ground support.

I'm going to leave this slide up here and go to the next one. This is one of Bob's slides, as well. And, these are TSPA input parameters that I'm going to speak to you here. I should point out that some of these are variables and vary over a specified range in the implementation of the TSPA run, the probabilistic run. And, some of them are, for example, fraction of the drip shield that is wet is set to 1. So, they come in different flavors. Next slide, please?

This is another slide which gives the invert diffusion model or the EBS transport model input parameters. Next slide, please?

Okay. So, now, I'm going to jump right to how we predict temperature and relative humidity in the drifts. The Board has been briefed on the multi-scale thermohydrology model. I recall Tom Buschek did this in 1998. That model basically combines 3-D, 1-D, and 2-D models with the purpose of representing heat flow due to thermal conduction in the rock on a three-dimensional large scale. In addition, it represents the heat transfer coupling effects from multiple waste packages and then it brings in the thermohydrology effect by using 2-D, TH model here. Now, the approach is roughly analogous. I certainly don't have time to explain the details of this approach here, but I can tell you that it is roughly analogous to a product solution and heat transfer theory where you have a one-dimensional solution which you actually multiply by a two-dimensional to give you a three-dimensional solution. And, I think that analogy is, more or less, correct for the thermal part for the hydrology part. Next slide, please?

For the hydrologic effects, those are limited to two dimensions in the current implementation of the multi-scale model. And, we are currently working on a three-dimensional TH model to incorporate in this process. Okay, next slide, please?

These are some representative results from the multi-scale model. These are 170 different temperature histories representing average surface temperature on the waste package. These 170 happen to have been chosen from 610 all together based on the range of infiltration at those 170 locations. So, in the very long-term then, the infiltration is in the range of 10 to 20mm/yr for these locations. And, the mean of those is plotted in red. Okay. Next slide?

Now, Leon Reiter also asked me to talk about thermally perturbed seepage and I'll take a crack at it. The multi-scale TH model prediction for liquid flux 5m above the drift is used. It's used to key into an algorithm developed by Bo and his collegues at LBL. This is Mike Wilson's of Sandia plot from his abstraction AMR. Basically, what we're doing is we're calculating the flux and we're entering a curve like this with that flux value to calculate seepage fraction. Now, seepage fraction is defined here as proportion of waste package locations that will be exposed to seepage. The min, max, and peak correspond to the limits and the peak of the probabilistic distribution function that's used in TSPA to represent the k bar over alpha parameter that Bo was talking about earlier. Bottom line is we're calculating a percolation flux in the rock 5m above the drift. We're drawing an analogy between that and the ambient percolation flux in this sense and we're using this model which is based on the development of the seepage model that Bo described earlier. Next slide, please?

Okay. So, somebody asked earlier what are the thermally perturbed fluxes that you would get and we're talking about fluxes here at the 5m horizon. Using this approach, you get fluxes in the range from 4mm to 120mm/yr. Those are averages and there's some additional variability that's associated with waste package to waste package differences in the thermal hydrology. For comparison, the ambient percolation flux during the present day period--that's zero to 600 years from present--ranges from 0.7 to 38mm/yr. So, this approach then is generating something like a three or four fault increase at the upward end.

Okay. What does the approach does not do and Bo has pointed this out is it does not include the effects of dryout within 5m of the drift opening and that is true. However, it also does not accommodate the fact that at certain times, very early during the post-closure thermal-hydrologic history, you may have a flux that's greater than the flux calculated at 5m. So, on balance, we think that this approach is reasonable and conservative. I would also point out that the extent of dryout around the drift openings depends. As Bo pointed out, it depends on the time. So, we develop a dryout zone and then over time, over thousands of years, it gradually diminishes back onto the drift wall. That's for a typical case. But, if you take a high infiltration location that has high flux and you're looking at the repository edge or even in the corner of the repository, then you get somewhat cooler temperatures. In effect, you may not develop a dryout zone that's bigger than a fraction of a meter. So, in that sense, dryout zone is not an issue. Enough said about that.

The point here that I'd like to make to you is that, after 600 years which is when we have our climate change and also corresponds to significant diminution of the thermal output of the waste packages, is that this approach to estimating thermal perturbed seepage generates results which are indistinguishable from the background approach where you would use the ambient percolation flux in the seepage model. So, after a couple of hundred years, no difference. And, Bo stated this differently, but what we're saying here is that the effect on the TSPA dose rate model from this several hundred year approximation is pretty slim.

Next slide, please? This is a calculation done using this approach. It's based on a 2-D thermohydrologic model. The model inputs represent the geographic center of the repository and we're using the mean infiltration here. What we're comparing here is the downward liquid flux at a horizon 5m above the crown of the drift and one 0.2m above the crown. You see in very early time at this location--this is a pretty typical location--we're going to get some movement of water close to the drift according to this model. After 600 years of so that the curves pretty much track the background percolation flux.

Next slide, please? Okay. Switch gears and talk about water composition. The available models for calculating and predicting water composition have determined the approach that we use in process modeling in support of the TSPA. These approaches are available to us. We have an empirical approach which we use for very dry conditions. I would point out to you that the little bullet at the top there--I need the pointer. Thank you. Okay. This bullet belongs here, my mistake. So, the empirical approach, we use for very dry conditions as certain salts deliquesce at a low humidity. And, we really don't have predictive models that tell us what, for example, sodium nitrate does in a saturated brine at a humidity of 55 percent when the strength of sodium nitrate in solution might be 15 molal. We don't have real good models for that. What we can do is we can say, look, we know the constituent salts that are present in the environment, we know when they start to deliquesce. We can treat them independently and look at the first one that deliquesces and that would be the nitrates, and then as others deliquesce, other components gradually come into solution. We'll get to a point as relative humidity increases where they've all basically dissolved into solution. You now have a brine that contains all the components. That point is selected as 85 percent relative humidity which is in reasonable agreement with other things that we know about the relationship between molality of these salts and relative humidity.

At that point, the approach kicks in a Pitzer Formulation which is an approximate model that is supported at the dilute end by some laboratory evaporation tests. At the concentrated end, we compare that model to tabulated solubilities for pure salts. The Pitzer model takes you right on up in relative humidity towards 100 percent. And, as you get up to relative humidity above 98 percent, it's possible to use the more familiar Debye-Huckel family of models. We do not do that in the PA because the approach--the Pitzer approach is adequate as an approximate model for the purpose at hand.

Okay. So, the output from this process that I've described consists of pH, ionic strength, chloride concentration as a surrogate for all the soluble components that develop as relative humidity increases in the presence of salts. Those are predicted over ranges of temperature, RH, and P_{CO2} which is the partial pressure of CO_2. The approach allows you to look at the effects if you have salts in the environment, no seepage. Just a little bit of salts, let's say you evaporate some pore water on the surface of the drip shield, you never see seepage, but it's exposed to the relative humidity. The approach allows you to look at that, as well as what happens if there is seepage during that period. The seepage comes in, part of it evaporates, it interacts with the salts.

Next slide, please? And this is an example of results output from the Pitzer model. This is based on Paul Mariner's (phonetic) calculations, his AMR, and this shows what happens to the pH of a sodium bicarbonate water with the composition of J-13 water as Rh increases from 85 percent to above 99 percent. So, pH is basically dropping towards neutral as RH increases. The approach is used to generate response surfaces that are then used to look up tables when the GOLDSIM model is run.

Next slide, please? Switching gears again, what kinds of breaches or holes can we develop in the drip shield or the waste package? We recognize that water can occur in different ways in the EBS. For example, humidity can interact with solid surfaces and you can develop very thin films of water. Those are not thought to be significant for advective flow; however, they may be--once a breach forms in the waste package, a breach of any type, that water vapor can get inside the package and then interact with the surfaces and you can get a thin layer of water. We're talking angstroms of water. That water can support the molecular diffusion of solutes from the waste form along a circuitous path that would result in a release. That is accommodated in the TSPA.

In addition, a stress corrosion crack would be a very small thin crack on the order of hundreds of microns in aperture. A stress corrosion crack in either the waste package proximal to one of the closure welds or possibly on the surface of a drip shield damaged by rockfall could behave in that way, could allow water vapor to penetrate the waste package. In addition, both the waste package and the drip shield are allowed to undergo general corrosion and in the model that eventually results in something called patch through which you can have capillary flow or droplet flow in the case of seepage.

Next slide, please? Okay. Now, we're going to talk a little bit about the EBS transport model. The EBS transport model is basically a one-dimensional advective, dispersive, diffusive model, represents a one-dimensional vertical pathway from the surface of the invert to the rock below. We do not take credit for radionuclide sorption in the invert. We have undertaken several studies in the past to look at the possibility of doing that; perhaps, even engineering radionuclide getters into the invert. We have elected not to do that.

So, in the current design, conceptual design, there is no sorption of either colloids or solutes, but there is advection. So, you have enough water in the invert. If the water content is high enough then under the impetus of gravity, you can have a flux which represents the velocity that is able to transport those nuclides from the surface to the rock. Now, if that velocity is very low and this would occur if the invert were relatively dry, then that flux would be vanishing small and you would not get releases except that you can have molecular diffusion through traces of water in the invert material and that molecular diffusion could result in a calculable release. Now, when the invert is very dry, you need to know what the molecular diffusion coefficient is. That process, we refer to as our invert diffusion model. It's one-dimensional and it relies on experimental characterization of diffusion coefficients. That's done using electrical analogue. We take granular material, such as gravels or crushed tuff, and we put them in a centrifuge, acclimate them to known hydrologic conditions, volumetric water content, measure the electrical conductance of the sample, and then equate that through a classical thermodynamic formula into solute diffusion.

Now, this plot here shows the experimental data support for the invert diffusion model. The data points were all generated by Jim Konka (phonetic) and Judith Wright (phonetic). These are published data. We see that the diffusion coefficient here is normalized to the self-diffusion coefficient of water. Basically, we have a power loss that's no surprise because behavior has been known to oil/fuel geophysicists for years; I think it's Archie's Law. The red plot is a classical form of Archie's Law over-plotted on these data. So, basically, for TSPA, what we're doing is we're taking the--we're using these experimental data to support a conservative fit down to 1.5 percent volumetric water content which gives us a normalized diffusion coefficient in this range.

Next slide, please? Okay. This is the overview of

TSPA abstractions for the in-drift chemical environment. What we have included is interaction between aqueous and gas phases, primarily CO_2 because CO_2 has an important effect on the pH. The evaporation of seepage, the evaporative concentration of it is taken into account, as well as potential condensation effect through the TH model which gives us an input to the calculation of water composition. We allow salts to form and dissolve in the drift. We calculate ionic strength which can be used to infer the stability of colloids; that is as ionic strength increases, colloid stability decreases. That has not been done before in previous TSPA approaches in quite this way. Finally, we have our EBS radionuclide transport model.

We had excluded influences on the bulk chemical environment for microbial effects, from cement-water interactions, and from corrosion products. Each one of those is associated with a series of arguments that I can only refer to at the moment.

Next slide, please? This is sort of a depiction of that. Multi-scale model gives us flux above the drift, gives us T and RH conditions at all points of interest within the drift. We get the seepage model used in conjunction with that boundary condition and is used to calculate actual seepage flow into the drift during the thermal period and then after the thermal period. The THC, the drift scale THC model that Bo described which I'm not really going to talk about is used to describe the composition of water at the drift wall and that's used as the incoming composition. So, we now know the flow rate and the composition of that water and the THC model also gives you P_{CO2}, but you have the necessary conditions to calculate water composition, evolution of water in the drift.

Okay. Next slide, please? Several of these things that are talked about represent improvements over past implementations of TSPA.

Next slide, please? Switching gears a little bit here and talking about FEPs, features, events, and processes, that need to be considered in performance assessment, for condensation under the drip shield, we have reached a bottom line that condensation could occur if the invert becomes wet enough. The method that we use to do this is approximate. We used NUFT which is a porous medium simulator. We calibrated to analytical solutions to represent the air space between the drip shield and the waste package. We then put those pseudo properties into a model, a TH model, and incorporated all the percolation conditions and the infiltration boundary conditions and rock properties and so forth. We looked for evolution of humidity under the drip shield that could lead to condensation and, yes, it is possible. However, I think we can conclude from this exercise that with the invert remaining unsaturated that the vapor pressure there will be rather low, but we will get a vapor pressure lowering effect because the water is at a negative potential and the end result of that is that condensation under the drop shield is rather unlikely. It is not taken into account explicitly in the PA.

Next slide, please? Another area I'd like to talk about here is rockfall. The approach that we've used to address rockfall is key block theory. This photograph shows a key block that was observed in the cross-drift. It's a block bounded by fractures with certain orientations that allows it to fall out of the drift wall under the force of gravity. There have been a great many data collected on fracture frequency and orientation which gives you the ability to predict when conditions like this could occur.

Next slide, please? This is a summary of those predictions. They include changes in the outline or the profile of the drift opening. The plot here shows a CDF cumulative distribution plot of block size. The analysis has been extended to seismic conditions by incorporating seismic acceleration in addition to gravity, which of course changed direction, as well as the resulting magnitude. In addition, the fracture data are available for different units. So, we have an idea of the frequency of rockfall that can be expected in the different areas of the repository.

Next slide, please? Okay. I'm about to boldly go where I've never been before and talk about the EBS transport sensitivity calculation.

Next slide? This is a comparison of the base case or the nominal scenario with a degraded EBS barrier and an enhanced one. The graded EBS barrier is defined as we show over here. We've used a different invert diffusion model. Instead of using the one based on the data, we've gone to a first order fit. In addition, the solubilities for plutonium are pegged at the 95th percentile of the distribution. The chemistry in the invert is assumed to be the same as it is inside the waste package which means that during a certain period of time when the waste package internals are first corroding, the pH is a little lower and neptunium solubility is higher. And, maximized colloid stability and maximized at the 95th percentile, the distribution coefficient for radionuclide sorption onto colloids. That gives you the red curve over here.

The enhanced barrier uses a very low value of the diffusion coefficient which would plot near the bottom of the plot that I showed you previously. And, it also uses the converse of the solubility and colloid stability and chemistry conditions that I talked about. So, bottom line is this behavior right here appears to be highly favorable to performance and, in fact, it is. It requires a diffusion coefficient on the order of $10^{-11} cm^2/sec$. We have no measured any coefficients that low. That's partly because when you get down to transport behavior of that nature, very slow transport, it's difficult to observe.

Next slide, please? This sensitivity study is for backfill and these two curves represent the same--I believe, they represent the same nominal scenario result except that for the backfill case, we've used a previous set of multi-scale thermohydrology model runs which incorporate backfill. But, the seepage is the same, virtually everything else about the model is the same. And so, that means that the effects here differ primarily in the temperature at the waste package surface for the first few thousand years. Not that the curves are really that different, okay? I think you could question whether these are significantly different results. So, given the way that the TSPA is set up, I think what's happening here is that differences between these results are limited probably to temperature sensitive cladding performance.

Okay. Next slide, please? Last slide, summary of major points. Temperature and relative humidity are the master variables in this approach to predicting the in-drift chemical environment. Temperature and relative humidity are fairly straightforward to predict. Relative humidity is a pervasive measure of the environment inside the drift. That is the gas phase is highly communicative. So, we use these in a way--combine them with chemical modeling in a way that allows us to predict water compositions given a reference composition as J-13 water or a chloride sulfate bore water. In addition, we can calculate water composition at various places within the drift. We've combined inputs from various other models. This is an integration of models for the purpose of calculating response in the drift.

And, a final point about water compositions, I would expect the water compositions will be heterogeneous, both spatially and temporally. Temporally as the hydrologic boundary conditions change and as the thermal perturbation changes; and spatially because we have spatial heterogeneity in the mountain. We have identified different compositions for waters; bicarbonate water, chloride sulfate water. Both are considered in this approach.

That's all I have.

BULLEN: Thank you, Dr. Hardin, especially for keeping it to 26 minutes. That was outstanding. Now, I'm going to allow just one from Dr. Parizek, a clarifying question, and we'll invite you back tomorrow to discuss whether or not backfill has an effect and we'll ask you to bring your slides and we'll investigate that further at the panel discussion.

Dr. Parizek?

PARIZEK: Brines and Bodvarsson says the dilute, dilute,

dilute water. I hear it from brines to dilute water. So, I just want to know where the brines are and it's with small amounts or what. But, that's just a--you can answer that tomorrow. It just seems inconsistent with what Bo mentioned earlier.

HARDIN: Sure. Yeah, I could take Bo's dilute brines and bring them into the drift and evaporate them. That results in--

PARIZEK: On waste packages then?

HARDIN: Yeah, on the drip shield or in the invert.

BULLEN: Thank you very much.

Our final presentation of today is on waste package and drip shield degradation by Pasu Pasupathi and we'll just start without further adieu. Pasu?

PASUPATHI: What I should say is most of the past models we've been talking about, one by Joe Farmer and his Livermore gang and the abstraction models were done up by Jung Lee (phonetic) and the PA team. You know, if Joe Farmer were here, he would be worried with the project. He would be standing there making the presentation; instead, you got me.

First slide, please? Okay. This is another one of Bob Andrews' slides. It simply shows the different attributes and process model factors for the drip shield and the waste degradation. We've got a bunch of these model parameters. For example, in the drip shield, there's a hydrogen induced cracking initiation threshold, hydrogen concentration profile thresholds, a profile that gives you the critical hydrogen concentration before the hydrogen induced cracking occurs. And, in the case of waste package, you have the parameters for the size of material, manufacturing defect flaws, stress and stress intensity factor profiles, and SCC initiation threshold, SCC crack growth rate.

Next slide, please? This presentation is going to touch on TSPA, VA, and how much improvement or how many models that have been added in the TSPA/SR. So, the design for the waste TSPA/VA has changed considerably. We used to have a carbon steel outer corrosion allowance barrier. That's been eliminated in the SR design. Alloy-22 used to be the inner barrier. It is still there, but now it's moved to the outer barrier position. Instead of the carbon steel for structural material, we have 315 stainless as the nuclear grade stainless steel inside Alloy-22 shell. And then, we added drip shield made of titanium.

Next slide, please? This has been projected lots of times before. One thing I want to point out is these arrows here are all misplaced. This is really the inner barrier and this is the outer barrier. Somewhere along the line when the transfer of files happens, you know, the arrows are not in the right places. But, anyway, the inner barrier is Alloy-22. It's got just one single lid on both ends.

The next slide, please? This is the current SR 21-PWR waste package configuration. You have the outer barrier with Alloy 22, the stainless steel inner shell and there are two--there's a stainless steel lids and there are two lids on Alloy-22, the one on the inner lid and outer lid. These are on the top. On the bottom, we have only a single lid of Alloy-22.

As I mentioned earlier, because of these design changes, we have added a lot of new degradation models. The TSPA/SR now includes stress corrosion cracking model and that includes the effects of manufacturing flaws and we have added aging and phase stability effects, microbiologically influenced corrosion effects, and the potential for radiolysis. And, in addition, we have looked at the bounding environmental condition on the waste package and drip shield.

The TSPA/VA because of the corrosion allowance material, we have included the general and localized corrosion that included humid air corrosion. The corrosion rates were based on published data. In the TSPA/SR, we do not have any corrosion zone material.

This applies to general and localized corrosion model applied both to drip shield and waste package outer barrier of Alloy-22 barrier. In the TSPA/VA, we have the general and localized corrosion. We have in the SR dry oxidation, humid air corrosion, and aqueous phase corrosion. In the VA, we looked at the range of water chemistry in the crevice, but assumed the worst case to be due to the Ferric chloride formation. Whereas in the case of SR, we have environment on the surface based on the evaporative concentration which Ernie Hardin talked about. The other item is the model parameters and corrosion rates were based on expert elicitation and published data. These date were not particularly relevant to the repository conditions. In the SR, we have primarily the experimental data from the long-term corrosion test facility and the short-term cyclic polarization data. In addition, we do use published data as a corroborative purpose.

The models that we have for all of the degradation models and source corrosion and general and localized corrosion, all of them get fed into a code called WAPDEG, which is the waste package degradation code, and that conceptually treats the waste package. It divides it into about 1,000 patches. We have different patches, different conditions for different patches. For example, here's dripping water. All of these patches see drips. The associated welds are identified separately and this one is for the closure welds. And, here the pH and the chloride of the water contacting the waste package and drip shield are coming out of the EBS chemical environment model abstraction.

Then, what's coming to the drift is from the UZ T&H models.

Again, continuing with this general and localized corrosion of the drip shield and Alloy-22, in the VA, we have the same situation with 1000 patches per package and also in the SR. We do not have a drip shield in the VA. This plot here as relates to this one, the maximum general corrosion rate, we have--we are assuming for Alloy-22 is .73 microns per year. This is the upper bound of the two year data and this is the curve that you get when you plot the CDF for the two year data. This is from the long-term corrosion test facility. For titanium drip shield, the general corrosion rate is .325 microns per year and this is also the upper bound of the measured data.

Again, continuing with this general and localized corrosion of drip shield and Alloy-22, this plot here is a different way of saying what Ernie Hardin was talking about. This particular point here is 120 degrees C, 50 percent humidity. That is the highest temperature, we believe, you can sustain an aqueous film on a salt deposit or uprooted geometry. And, this is based on the deliquescing point of sodium nitrate. As you go up in relative humidity and lower in temperature, other salts come into the picture, you get into the chlorides and sulfates dissolving along with the sodium nitrate.

I mentioned the .073 as the highest general corrosion rate we are assuming in the TSPA. Added on top of that are a whole bunch of factors and this is one of them. This is to account for the silica deposits we have seen on surfaces and some of the coupons we take out--took out of the long-term corrosion test facility. The deposits were analyzed and looked at and then we are assuming a uniform deposit based on the thickness, the density, and all that. It translates to about .063 microns per year bias as a maximum. So, we use zero to .063 as a distribution added on to the general corrosion rate.

The other two bullets relate to the localized corrosion aspects. We have taken out samples from the long-term corrosion crevice samples and--well, in the crevice sample, we have seen no evidence of localized corrosion. These test media in the long-term corrosion test facility go from 10x, 100x, and 1000x J-13. pH range is from 2.7 to about 9 or 9.5. In addition, we did cyclic polarization tests on both Alloy-22 and titanium on four different media at 60 degrees C and 90 degrees C and we still don't see any localized corrosion.

BULLEN: Pasu, would you just clarify, the .073 is added to the .063?

PASUPATHI: .063 is added to .073, right.

BULLEN: And so, you actually get a corrosion rate that's conservative--silica up to .13?

PASUPATHI: Yes, right.

BULLEN: Okay. Thank you.

PASUPATHI: Okay. The next one is MIC. There was no consideration of MIC in the TSPA/VA. In the SR, we have--we evaluate MIC with electrochemical techniques. The samples were tested in the sterile and inoculated test media, J-13 based. I think they were based on simulated concentrate water. That's about 1000 x J-13. Based on the short-term tests that corrosion rate and angstrom factor, 1 to 2 was determined. So, this enhancement package is also applied to the general corrosion rate. We're continuing to work with this. Long-term tests are going on with the different media and so we'll update the results as we go along. And, titanium is still considered to be immune to MIC.

The radiolysis effect, we did no consider radiolysis effect in TSPA/VA. Here again, we have done short-term cyclic polarization tests, added hydrogen peroxide to the test media up to 72 ppm. After that, it seemed to stabilize. So, there was no point in adding more. The corrosion potentials were measured and based on this, we concluded that radiolysis does not change the corrosion rate significant. So, it's been screened out of TSPA at this time.

Aging and phase stability, then we did not consider aging and phase stability in TSPA/VA. In the SR, we have a fairly extensive program ongoing on aging and phase stability. We are using some of the samples that have been aged by Haynes (phonetic) as a corroborative measure. We have our own samples going through the aging process. We have only limited data at this time, but based on that, we have the functional relationship between temperature and the fraction of grain boundary coverage where the precipitation occurs due to again. The limited data shows that aging and phase stability will not be important if the surface temperature of the waste package stays below 260 degrees C. Again, as I mentioned, this is based on the base metal in the annealed condition and we're continuing work with core work and welded samples.

Based on the data that we have, this was again a short-term cyclic polarization test. We determined an enhancement factor of 2.5 is appropriate for again. This is a fully aged sample. These two photographs show the effect of aging. This is at about 650 degrees C, 100 hours of aging; and this is the same temperature, 1000 hours of aging. So, you can see the difference in the amount of the invert precipitation. We are assuming no aging effect on titanium.

Early failure, this is a significant difference in this particular model. In TSPA/VA, it was assumed that one waste package failure, 1000 years for base case. Probabilistic case of 1 to 10 was assumed. The upper bound was based on British pressure vessel data, 17 defect-related failure in 20,000 and so it comes out at about 8.5 for 10,000. The lower bound was based on a conservative interpretation of Midland reactor vessel. So, this gives like $6x10^{-6}$ per waste package. And, the time of occurrence of these failures was pretty much arbitrary.

Whereas in the case of TSPA/SR, we have a review of early failure literature on welded metallic containers. This included tin cans and fuel rods and pressure vessels and cesium capsules and every kind of welded material you could think of. And, for the types of defect that can occur and subset applicable to the waste package. You know, not all of them are, but a couple. So, for each defect type, estimate probability of occurrence per waste package and the consequences of these defects. And, manufacturing and handling induced errors and defects, these are human factors induced. Defects were also assumed. This particular model, we believe, is much more defensible and more applicable to the waste package in repository conditions.

As I mentioned, in addition to the weld defects, we consider a lot of other things, such as improper heat treatment and the surface contamination. This assumes somebody uses to clean up the waste package surface and then leaves contaminant in there and then any handling damage, thermal mis-load of waste package, drip shield emplacement, all of these things were included and our conclusion was that only weld flaws have the potential to lead to early failures through the SCC. So, these flaw sizes and size distributions were included in the SCC model.

This is another big one now, a big change from TSPA/VA. We recognize that, always recognize, the SCC incredible mechanism for Alloy-22 under certain conditions. It was not analyzed in the VA because we did not have data or the models to do that. The SR, we have two different models. One based on the stress intensity factor threshold and the other one based on what we call the--diffusion model. This model was selected in the TSPA/SR because it is much more defensible and it assumes the stress to show for initiation of SCC crack. This again, as I mentioned earlier, the SCC model for Alloy-22 includes manufacturing defects present in the closure lid welds. We did not include the SCC model for titanium in the SR in the early version of our AMR mainly because we are backfilling the design and titanium was protected from rockfall by backfill. And, we had planned on annealing all of the titanium welded structures prior to installations. But now that the backfill is not in the design, we have a model to look for titanium stress corrosion packing based on rockfall into the stresses and that is being put into the SR model.

So, the recent tests that the project had done both at General Electric Center and in Livermore show that Alloy-22 and titanium are both susceptible to SCC. So, looking at the three parameters we need for SCC, the environment, the susceptibility, and stresses, we would that we don't have a whole lot of control over environment. At least, the material is shown to be susceptible. So, the only thing we can do is to deal with the stresses. So, stress mitigation is the planned approach right now to eliminate or delay SCC.

And, we also have added a second lid to the design to give us additional margin. So, this is what the schematic looks like. This is the outer lid and this particular area of the weld will be induction annealed to relieve the stresses. This one is the inner lid and this particular one is a Philip weld and stress will be mitigated by laser peening. And this inside is the 316 nuclear grade weld and we are not planning to do anything with that.

Okay. We're looking at several sets of conditions for the outer closure lid. I don't have--I have similar curves for the inner lid, but in the interest of time, I just put in only these two. This particular curve shows--this is the hoop stress distribution. And, when you look at the stress mitigated layer, this one is--the positive stress intensity factor goes around about 12mm. What this says is that you must corrode through 12mm of material before SCC cracks propagate. And, we believe the hoop stress is the dominant stress driving radial SCC crack. The radial stresses do cause crack, but they cause circumferential crack of the cracks by the time the stress distribution--by the time the cracks grow part wall, the stresses are not conducive to propagation of these cracks. So, they don't go through wall.

Next slide, please? This one is assuming a different set of stress distribution uncertainties. Here, we're looking at 10 percent, 15 percent, 30 percent uncertainty band about the stress distribution in the material. And, this is the stress intensity factor associated with that. Again, when you look at the conservative case of 30 percent bound, the positive stress intensity factor is around 4mm. So, the decreased minimum thickness of the compressive zone to about 6mm to before you can--you have to corrode to that much.

Okay. Next slide, please? These are waste package lifetime. This is the integrated model that WAPDEG puts all of its current degradation modes into that. And, in the case of TSPA/VA, the first failure of the waste package was due to localized corrosion of the two barriers. Assuming the high pH on carbon steel and then Ferric chloride concentration of the crevice and going through a localized corrosion, the number was about 2700 years. In the SR, we have significant model enhancement incorporated and the first waste package failure is a conservative estimate of about 11,000 years. This is based on 100 realizations and looking at the worst case of 100 realizations.

Next slide, please? Let me do what Ernie did. I'll put this up on the viewgraph machine and we can compare. These are the degraded drip shield barrier sensitivity. These are the 95th percentile for the degraded barrier and the 5th for uncertainty-variability partition. So, in other words, these are assuming the worst case for the degraded condition and then the best or best estimated case for the 5th percentile cases and the favorable conditions for the enhanced barrier. There's not a whole lot of difference in here, as you can see. If there are any more questions, Bob probably can answer better than I can.

Next one, please? Okay. Again, this is the degraded barrier and the enhanced barrier, the same situation. Some of these have beaten to death already. I think, Bob has addressed most of these things. But, the key thing is to look at the 5th percentile manufacturing defect probability and then uncertainty-variability partitioning and the 5th percentile of Alloy-22 corrosion, 5th percentile of Alloy-22 inner stress profile indices. All of these things are in the favorable condition. These are all in the most unfavorable condition.

Next slide, please? So, again, you see this. The failure time for waste packages come down quite a bit and I think Bob mentioned 6,000 years or so. Yeah. But, the other thing I wanted to point out is that even with all of these early failures occurring, you're not exceeding even 10mrem for about 30,000 years or so. So, that's what it really--

COHON: Excuse me, I just want to ask you a question while this is up. Two or three slides ago, didn't you just say that the earliest possible failure is 11,000 years.

PASUPATHI: Yes, right. Right.

COHON: So, how do you reconcile the--

PASUPATHI: The two differences--the WAPDEG runs based on 11,000 years are a stochasic approach. This is artificially forcing everything to the worst condition or the best condition. Isn't that right, Bob? That's the way it was explained to me. In other words, in the WAPDEG cases, you take about 100 realizations and take--

COHON: No, but wait a minute, wait a minute. You're staying inside all of your distributions. You're not going outside of them.

PASUPATHI: Right.

COHON: If I understood what you said before, you said the earliest possible failure is 11,000 years.

PASUPATHI: When you do a stochastic analysis using WAPDEG, some of them may be not so unfavorable, some parameters. So, when you run 100 realizations, the left-most curve is what you're looking at.

COHON: So, when you said earliest possible, you meant in the context of this model?

PASUPATHI: Yes, exactly. Stochastic was the force fit worst case. Am I saying it right?

ANDREWS: I think that's right, Pasu. I mean, this is forcing that at a very low-probability of occurrence. I mean, I think you asked the same or similar question before. You know, if I take this .05 to the whatever, 7th power, this is well outside our distribution of what we expect to occur, but it's--you know, trying to force a system to barrier failure.

PASUPATHI: I think that's all I have.

BULLEN: Thank you, Pasu. And, we'll just entertain one or two quick questions. I see Alberto has his hand up. For clarification only, please?

SAGÜÉS: Yeah, a clarification.

BULLEN: Does this one have to go out in the hall, too? Go ahead, Alberto?

SAGÜÉS: Do I understand then that localized corrosion is--other than for stress corrosion cracking is completely out, is not a concern?

PASUPATHI: Right. Right.

SAGÜÉS: It is not going to happen.

PASUPATHI: Currently, it is not. Yes, right. In fact, our failures occur by stress corrosion only at this time.

SAGÜÉS: Thank you.

BULLEN: John?

KESSLER: Maybe, it was just the same question. I just want to make sure I confirm that the failure is occurring at the closure welds?

PASUPATHI: Right.

KESSLER: And, that where would--would general corrosion occur anywhere in the center of the package any time--

PASUPATHI: Yes, it will continue to occur so long as the conditions are right. If there's water, film, and whatever, it will continue to occur, but the rate is so slow that, you know, this particular failure scenario dominates.

KESSLER: Okay. So, it's much, much later before you start getting failures over the body of the container?

PASUPATHI: Right. In fact, if you apply .073 plus the bias and whatever other factors you do, still it's lower than what --.

BULLEN: I would like to thank all the speakers today and also to thank the audience for their adherence and tenacity to stay around for the duration. I would also invite everyone to take a look at the handouts that we have to ask their questions tomorrow during our panel discussion. Now, I'd like to turn it back over to our Chairman who will close today's session.

COHON: We're adjourned. We reconvene tomorrow at 8:30.

(Whereupon, at 6:30 p.m., the proceedings were recessed.)

I N D E X