UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING 2000 BOARD MEETING

REPOSITORY DESIGN and GEOCHEMISTRY

Monday, May 1, 2000

Bob Ruud Community Center
150 N. Highway 160
Pahrump, Nevada 89048
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BOARD MEMBERS PRESENT

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

SENIOR PROFESSIONAL STAFF

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Harold Ahagen,Expert - LKO
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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 and it's my pleasure to welcome you to this spring meeting of our Board. We've very pleased to be back in Pahrump. Ms. Devlin reminded me that it's been three years since we met here and we're very glad to be back. We enjoyed ourselves greatly while we were here last time and I'm sure we'll have another good meeting.

I'd like to call up Commissioner Ira "Red" Copass to provide a welcome to us.

COPASS: Thank you. Before I even get started on the speech, you were talking about Sally Devlin. She reminded me we have two stop lights in Pahrump now.

So, good morning, everybody. Welcome to Pahrump. We appreciate the fact that you took the supreme effort to come to Pahrump for this meeting, especially for people from Amargosa Valley who are going to be affected by Yucca Mountain and give them a chance to participate. And, by the way, this is a good-looking crowd. I realize some of you people had to go over the hump to Providence and, once again, welcome.

Most of you people probably know that we are about --we think, we are 29,000 people. We expect by the year 2010 to be around 60,000 or so. As you know, we are having growth problems and sometimes we take care of it and sometimes we don't. The Commissioners usually wind up looking like a bunch of idiots, but that's okay. In some cases, we are. So, why not?

One of our big things we are planning or trying to plan for out here in this valley and southern Nye County, especially, is the water. We're trying to keep a close tab on it. That's one of the big problems that we see in the future is water for southern Nevada. Now, as you well know, Nye County has been closely associated with the Federal Government. It has been for about 50 years on account of the Nevada Test Site. The fact of business is I remember it pretty well, too, because I'm old enough.

I'm not 21, anymore. The fact of business is I use my age to get by with a lot of things because, see, when you get to be old and you say or do the wrong thing, you just say, well, I'm too old to remember or I forgot.

But, anyhow, getting along with this little speech, what we're doing here says the nuclear project you are working on will have much more radioactivity associated with it than all of the above; the below ground weapon test conducted by Nye County plus the high-level being buried here, and it's going to be more than what it was when they set off all those bombs out there at the Nevada Test Site. So, what you're working with is something that's much more greater than what's already been there. What we're trying to do is to make sure that Nye County is kept in the circle and remembering that we're going to be here afterwards and we're still trying to keep this a nice, sedate community. And, we hope that you keep that in mind when you make the decisions as to what's going to happen down the line.

Once again, I want to thank you very much. I didn't read my speech. I kind of did it from the top of my head. I hope it was good enough. Thank you very much.

COHON: I suggested to Commissioner Copass that he give us his speech to be included in the record and he said he would do that.

Welcome, again. And, again, we're very pleased to be back here in Pahrump. Our Board meets generally three or four times a year. We usually meet in Nevada; often, in Las Vegas, and at least once a year, in one of the communities here in Nye County in which, of course, Yucca Mountain is located. We also try to meet in Washington, D.C. once a year. It's my pleasure to extend a special welcome to those from the state and, especially, from Nye County who can be with us today.

As most of you 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. DOE and charged it, in part, with developing repositories for the final disposal of the nation's spent nuclear fuel and high-level radioactive wastes from reprocessing. Five years later, in 1987, Congress amended that law to focus OCRWM's activities on the characterization of a single candidate site for final disposal, Yucca Mountain located on the western edge of the Nevada Test Site.

In those same 1987 amendments, Congress created the Nuclear Waste Technical Review 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 through Congressional testimony and reports. An example of our reports is our recently released summary report for 1999. It includes our findings, conclusions, and recommendations during all of last year. Copies will be available at the back table probably later on today when our shipment arrives from Las Vegas. It's already up on our website, however, and we encourage you to visit our website at www.nwtrb.gov, and you'll find, in fact, all of our publications and public letters, etcetera.

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

Now, I'd like to introduce you to members of the Board, all of whom serve on the Board in a part-time capacity. In my own case, I'm president of Carnegie-Mellon University in Pittsburgh. My technical expertise is environmental and water resources system analysis.

John Arendt--John, if you'll raise your hand, please--is a chemical engineer by training. After retired from Oak Ridge, he formed his own company. He specializes in many aspects of nuclear fuel cycle including standards and transportation. John chairs the Board's Panel on the Waste Management System.

Daniel Bullen is professor mechanical engineering at Iowa State University and he's wearing his colors today. That's not a Rorschach Test; that's an ISU Cyclone on Don's chest there. He's at Iowa State University where, in addition to being professor of mechanical engineering, he coordinates the university's nuclear engineering program. Dan's areas of expertise include nuclear waste management, performance assessment modeling, and materials science. He chairs both our Panel on Performance Assessment and our 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. She's a former Deputy Assistant Secretary in the Department of Interior. Previous to that, she was a scientist at the USGS. Her areas of expertise are in groundwater hydrology and she chairs the Board's Panel on Site Characterization.

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

Alberto Sagüés is distinguished professor of materials engineering in the Department of Civil Engineering at the University of South Florida in Tampa. Alberto is 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 in risk assessment and scientific team management. Jeff chairs our Panel on Environment, Regulations, and Quality Assurance.

Richard Parizek will be joining us later today. He's professor of hydrologic sciences at Penn State University and an expert in hydrogeology and environmental geology.

Our last member, Don Runnells, unfortunately, sends his regrets. He could not be here for health reasons. He's professor emeritus in the Department of Geological Sciences at the University of Colorado at Boulder. He's also vice-president of Shepherd Miller. His expertise is in geochemistry.

I know I speak for all of our Board when I tell you how pleased we are to be back in Pahrump. I say it myself, but I know they want me to say it, as well. They enjoy being here.

Many of you know and have worked with our staff who are displayed with sartorial elegance before you. I'd like to pick up, actually, on something the Commissioner said. He told us what a good-looking crowd we are and I took it as a compliment. The last time we were here, we all dressed in suits and ties and I think it was the Commissioner who said we haven't seen so many suits in Pahrump since somebody died. I forgot what it was. So, we decided to change that and you can see we've adopted something closer to natural garb.

Bill Barnard is not here. He's in the back carting the coffee for you. He is Executive Director of our Board. Mike Carroll is the deputy executive director. Mike, raise your hand, please? Unfortunately, Mike will be deputy executive director only for a few more weeks, at which time he'll move on to greater things within the U.S.

Government. He's becoming Assistant Inspector General for Management with the Agency for International Development. We wish Mike well and we will miss him sorely. Thank you, Mike, for all that you've done for the Board.

CARROLL: Thank you.

COHON: The Board is very pleased today that we have three guests with us from Sweden. Torsten Carlsson is Mayor of Oskarshamn in Sweden and you'll be meeting him later this morning when he speaks to us. With Mayor Carlsson today is Krister Hallberg, project manager for Oskarshamn's feasibility study on whether to volunteer as a possible repository site, and Harald Ahagen, expert consultant to Oskarshamn. In arranging this part of Mayor Carlsson's visit to the U.S., the Board hopes to assist him in his efforts to learn more about the political, regulatory, an site characterization processes for the Yucca Mountain site.

Some of our Board members have had the opportunity to visit Oskarshamn which is a small community located on the southeastern coast of Sweden. It's home to a number of nuclear facilities, including Sweden's central interim storage facility, a full-scale canister laboratory, three commercial power reactors, and an underground research laboratory. Oskarshamn is one of six municipalities in Sweden that have volunteered for the first phase of process aimed at picking a final repository site for that country's high-level wastes. Mayor Carlsson and Mr. Ahagen will be updating the Board and you on developments in the Swedish program, with particular emphasis on the decision-making processes put in place by Oskarshamn for the purpose of evaluating whether to proceed to the next phase of Sweden's site selection process. This should be very interesting and valuable for all of us.

I'd also like to acknowledge some others in the audience with us today. Lawrence Jacobsen, State Senator of Nevada, we're pleased you're here, Senator Jacobsen. Thank you.

JACOBSEN: Good morning.

COHON: Dr. Ivan Itkin, Director of OCRWM, from whom you'll be hearing later. Dr. Itkin. Dr. Russ Dyer, Director of the Yucca Mountain Project Office, waving his hand in the middle of the group there. And, George Dials, General Manager of the M&O. Thanks for being here, George.

Now, let me turn to our day's agenda which you've noticed is very full, as these agendas seem always to be. We will begin this morning with an overview presentation by Dr. Itkin who will update us on OCRWM's program and the Yucca Mountain Project, in general. He will be followed by Mayor Carlsson who will give us his perspectives from the perspective of potential hosts for the Swedish nuclear waste repository.

Our first technical session will focus on the repository and engineered barrier system design. Paige Russell will bring us up to date on design changes since the design was last presented to the Board about a year ago. Jean Younker will then discuss the effects of repository temperatures on the uncertainty associated with repository performance over the long-term. Ric Craun will complete the first session by presenting the results of a recent analysis of how varying repository operational parameters could affect repository temperature.

These latter two presentations by Dr. Younker and Mr. Craun are extremely important and I want to emphasize that. Let me take a moment to explain why so you're prepared for this and you have some context. Most of you are well-aware that the Board has for years expressed concern about the high degree of performance uncertainty associated with high repository temperatures, particularly rock temperatures above the boiling point of water. Furthermore, in the presence of liquid water, corrosion rates generally are higher at higher temperatures. Jean Younker will be describing an analysis that the Board hopes will address its long-term concerns. The upcoming presentation, hers, as well as the others, and the discussion that follows should be very interesting.

To complete the morning sessions, we'll have a public comment period, one of two today, and I'll be saying more about the public comment periods in a little while. Lunch will be somewhat late today for which we apologize, but by being late, we will avoid the rush in the many restaurants. So, you have a lot more restaurants, I noticed, than you did three years ago. So, maybe, it won't be so bad.

The afternoon sessions will focus on scientific updates. Abe van Luik will discuss some of the open issues in performance assessment and Mark Peters will give an update on the underground scientific program, particularly the cross-drift or the ECRB or some people like to call it the Board's drift. That's something of an inside joke. The last session of the day will be on geochemistry. First, we'll hear from Nye County. Then, we'll hear an update on the chlorine-36 situation.

The meeting will conclude with the second public comment period.

Now, let me say a few things about the opportunities we provided for public comment and interaction during the meeting. This is something that's extremely important to the Board and we try to give the public as many opportunities as possible to participate in our meetings. Before the meeting started this morning, Board members were pleased to have a chance to chat with many of the members of the public over coffee and thank you for those wonderful muffins, etcetera. This kind of informal interaction gives us an opportunity to get to know each other better and for you to express to us any thoughts or concerns you might not be willing to express in the more formal atmosphere of our meetings.

For today's two public comment periods, those wishing to comment should sign the public comment register at the check-in table where Linda Hyatt and Linda Coultry are stationed. 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 limit the amount of time we can give to remarks.

As an additional opportunity for questions and continuing something we've tried out successfully at some of our recent meetings, you can submit written questions to either Linda during the meeting. We'll make every effort to ask these questions. That is the chair of the meeting at the time will ask the question during the meeting itself, rather than waiting for the public comment period. We'll do that, however, only if time allows, which it may not in light of our very tight agenda. If that's the case, we'll 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 these written comments to either Linda.

Finally, I need to offer our usual disclaimer so that everybody is clear on the conduct of our meetings and what you're hearing and its significance. Our meetings are spontaneous by design. Discussions are not scripted events, despite the fact that I'm reading from a script here. That's the last time that's going to happen in terms of a Board member's remarks. Those of you who have attended our meetings before know the members of this Board do not hesitate to speak their minds. Let me emphasize that is 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, however, we will make that clear so that you'll know it. Otherwise, we're speaking as individuals.

Let me just mention one other important logistic matter. It's very important that you speak directly into the microphones and get close to them, especially those on the table and those standing up. They're for the members of the public and the members of the Board. Otherwise, people will not be able to hear you and our reporter will not be able to record your remarks.

Now, it is my pleasure to introduce our first speaker, Dr. Ivan Itkin, Director of OCRWM. A fellow Pittsburgher, Dr. Itkin came into the program last December after a long and distinguished career of public service in the state legislature in Pennsylvania. Before his election, Dr. Itkin worked on the Naval Nuclear Propulsion program at the Bettis Atomic Laboratory near Pittsburgh. Dr. Itkin has a doctoral degree in mathematics from 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. Dr. Itkin spoke to the Board in our January meeting and we're very pleased to welcome him back.

Dr. Itkin?

ITKIN: My only regret there, Jerry, is that I didn't get the message that we could come to Pahrump in a very casual dress manner. I would have preferred to be in your suit rather than mine. I hope that in the future my people from the DOE can remember that; come to the meeting and dress casually.

Well, thank you very much, Jerry. It's a pleasure for me to travel so many miles to visit with you. Jerry and I live in the same community in Pittsburgh, very close to one another, and I have to travel out to Nevada to visit with him. But, it's nice to see you on any occasion. And, it's also very nice to see the members of the Board here who I very much respect and are very gratified to have and be able to look over our shoulders, so to speak, and to be able to comment and to critique our work in a very constructive--and you have been--in a very constructive manner.

I would like today to update the Board on our recent progress and the near-term plans for the Civilian Radioactive Waste Management Program. I will also use my time to discuss some of the broader issues that affect the program, along with the issues raised in your recent correspondence. After my talk, there will be more detailed discussions on these issues as Dr. Cohon has mentioned and other topics that you have requested.

I'd first like to talk about our program's budget. Over the past three years, the program has received approximately $110 million less than the amount requested from the Congress. Because of these shortfalls, we have focused our efforts on the science and engineering activities most important for determining the suitability of the Yucca Mountain site for a geologic repository. This focus has taken into account the improved repository system from the design enhancements for the repository and waste packages. I would like to emphasize that even under restrictive budgetary climate, the program has aggressively addressed those issues most pertinent to understanding the uncertainties that could be associated with repository performance.

In spite of our efforts to focus the program, the budgetary shortfalls have had their consequences. The program has had to defer or reduce the scope of work required for licensing. Some of the work reduced in scope includes key elements of preclosure design and analysis, such as the integrated safety assessment required by the Nuclear Regulatory Commission. The benefits that could be obtained by further evolving the repository from the viability design to a modular design have been deferred. We can no longer continue to delay completion of this work and maintain our goal for submitting a license application to the NRC in 2002.

Our fiscal year 2001 budget request of $437.5 million is essential to complete the necessary work for defensible site recommendation. Significant components of our planning are additional design and engineering work and focused testing and analyses, both of which address recommendations from the Board. The FY 2001 request is a 25 percent increase over last year's budget authority. As I have testified before the Congress, if we do not receive the funding that we have requested, we will be forced to curtail our science and engineering work and potentially delaying site recommendation.

Our plans for FY 2001 reflect the evolution of Yucca Mountain Project's emphasis from comprehensive site characterization to focused scientific investigations and data synthesis, model validation, repository and waste package design, safety analysis, and documentation. Upon completion of site characterization, the program will shift its priorities to enhancing and refining repository design features and to developing the remaining information required for licensing.

Our plans are described in Revision 3 of the Civilian Radioactive Waste Management Program Plan released in March. This revision takes into account the programmatic changes since the publication of the viability assessment including the substantial budget shortfalls in FY 1999 and FY 2000. I believe, copies of the plan were provided to all the Board members.

I would like to add that the FY 2001 budget request includes $10 million for a cooperative agreement between the Department and the University and Community College System of Nevada for performing scientific and engineering research. We hope that this agreement which started in FY 1999 and lasts into FY 2002 will continue to foster cooperative working relationships between government and academic researchers.

And, now, I'd like to turn to legislation. As you know, Congress passed Senate Bill 1287, the Nuclear Waste Police Amendments Act of 2000, and sent it to the President in April. If enacted, the bill would authorize acceptance of spent fuel at the repository surface facilities after the NRC issues a construction authorization for the repository. The bill would set a milestone of January 31, 2006, for NRC to decide whether to issue the construction authorization. The bill would not allow the Environmental Protection Agency to promulgate radiation protection standards for the Yucca Mountain site before June 1 of next year, 2001. Before promulgation, the NRC and the National Academy of Sciences would each submit a report to Congress on the proposed standards.

The President vetoed S-1287 for reasons that the Administration has consistently cited before. The Administration opposes legislation that would undermine EPA's existing authority to establish standards for a repository at Yucca Mountain. The bill that the President vetoed does nothing either to advance the scientific understanding of the Yucca Mountain site or to increase the public's confidence in a siting decision. The Administration continues to believe that the overriding goal of the Federal Government's high-level waste policy should be to establish a permanent geological repository. The Administration remains fully committed to completing the scientific investigations necessary to make an objective, science-based determination on the suitability of Yucca Mountain as a site of a permanent geologic repository.

Now, I will briefly discuss some of the issues that you have raised in your recent correspondence. Since January, we have received three letters from the Board and the summary report on your 1999 activities. We appreciate your timely and constructive feedback on our activities. We recognize the important independent oversight role that the Board plays in the program. I look forward to working towards a common understanding of these issues and our approach to resolving them.

Our recent discussions and correspondence continue to stress the notion of uncertainty and its consequences with decisions regarding the suitability of the site. The issue of uncertainty has always been an important factor in reaching a decision on a repository, which involves assessing performance over many thousands of years. Through our scientific investigations, we have assembled the technical knowledge necessary to support analyses of repository performance and to develop site-specific repository designs and operational concepts.

These efforts have also led to the development of state-of-the-art analytical tools needed to determine the significance of uncertainty. Our analyses seek both to quantify the degree of uncertainty and to evaluate the significance of that degree of uncertainty to the overall performance of the repository system. And, this approach ensures that relevant issues are thoroughly evaluated and provides the context necessary for decision-making on issues, such as the appropriate operating mode for the repository.

Our current repository design concept and its operational mode were selected after a thorough evaluation of alternatives, as suggested by the Board. The Board noted that the selective design concept showed much progress when compared with the design concept in the viability assessment. As the Board is aware, the repository design process involves the definition of both the physical characteristics of the engineered system and its operational parameters. Our design process has produced a robust design concept that offers a great deal of operational flexibility by allowing us to make adjustments in the period of ventilation, in the amount of fuel staging and fuel loading into the waste packages, and in waste package spacing. The current design concept retains the flexibility to implement either an above-boiling or below-boiling thermal load. This design flexibility permits us to refine the operational parameters of the repository as we gain a greater understanding of the uncertainties associated with the thermal loading.

The Board has stated that repository operation at below-boiling temperatures would reduce uncertainties in assessing performance and, in particular, those associated with the complexity of coupled processes. The Board also suggested that reduced uncertainties would increase the confidence in a site suitability determination by improving confidence in the scientific basis for the determination. We recognize the interdependence between the thermal characteristics of the repository operating mode and the uncertainty in the analyses of water movement in the surrounding water. We have considered and will continue to consider this relationship in the evolution of our design and operational concepts.

To further reduce uncertainty, the Board has recommended that we evaluate our current design concept at below-boiling temperatures. Our evolutionary design process is responding to the Board's recommendation in a thorough and controlled manner. With the analytical tools that we have developed, we are evaluating the key operational parameters and refining our operational concepts to mitigate to the extent practical the impacts of uncertainties of concern to the Board, while accommodating the other constraints on the program.

For example, we have evolved the design by removing backfill to lower fuel pin temperatures, thereby reducing the uncertainties associated with long-term fuel pin integrity. We believe that this design and its operational flexibility effectively balance the uncertainties in repository performance analyses with other programmatic considerations, such as public and worker safety, intergenerational equity, and cost.

The program's ongoing evaluation is focused on the operational parameters that could further reduce temperatures. Those parameters are being assessed to evaluate their impacts on both the uncertainty in performance analyses and on other programmatic considerations. We recognize that the Board is very interested in this effort and have supported a number of related interactions over the past several months.

I urge that we explore the flexibility of the current robust design concept thoroughly and, in particular, its options for managing temperature conditions. A decision on whether or not to proceed with a repository should be met with prudent consideration of all the relevant aspects. The program has put forth a flexible repository design that balances all the technical and programmatic considerations. And, this approach will permit future generations to evaluate actual repository performance, learn from the operations and monitoring, and close the facility when appropriate. A repository that is flexible to future changes in priority and reversible in the event that the National policy changes, is one way to address concerns regarding the need for additional information due to uncertainty.

Now, let me address the status of development of the regulatory framework for Yucca Mountain. Finalizing this site-specific regulatory framework is central to determining the suitability of the Yucca Mountain site for development as a repository.

NRC and EPA proposed their site-specific regulations last year. The public comment periods for these draft regulations have ended. We understand that both NRC and EPA are now working to complete their final regulations.

To align ourselves with the NRC and EPA site-specific regulations, last year the Department proposed its guidelines for determining Yucca Mountain site suitability. We held two public hearings in Nevada on the proposed suitability guidelines, and the public comment period has ended. We, too, are working to address public comments, including those of the Board, and to complete the final rule.

In determining site suitability, a concern of the both the Board and the Department is understanding and communicating the uncertainties about performance assessment. The consideration of uncertainty will be a key component of the determination. The Department has stated that the determination of site suitability is largely an estimate that a repository at Yucca Mountain could meet applicable radiation protection standards, as set by the EPA and implemented by the NRC. To make this estimate, we will not only present the performance assessment results, but we must account for the uncertainties and variabilities in parameter values and provide the technical basis for them. This estimate will also take into account other factors, such as the analyses of multiple barriers.

I now want to address our plans to complete the Final Environmental Impact Statement. During the 199-day public comment period which ended last February 28, we conducted 21 hearings throughout the country to solicit comments on the Draft EIS. More than 2700 individuals attended those hearing and more than 700 provided comments. The total number of comments received at the hearings, in writing, and by e-mail exceeds 10,600, and parenthetically, I'm told that's approaching 11,000, as we speak. Among those are comments from the Board. We are presently analyzing the comments, preparing responses to be documented in the Comment Response Document and continuing development 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 is on developing the Site Recommendation Consideration Report and supporting documentation. We continue to gather and analyze relevant site characterization data, some of which you will hear about later today. We are completing another major iteration of the total system performance assessment. Although the SRCR is not specifically required by the Nuclear Waste Policy Act, we are planning to issue it late this year. After the issuance of the SRCR, we plan to hold public hearings in the vicinity of Yucca Mountain to inform the public of a possible site recommendation. We will solicit comments from the public, and the States, Native American Tribes, and the NRC. The program will then focus its efforts on updating the technical basis for a site recommendation. This process will provide comments and updated information for the Secretary's consideration in deciding whether to recommend the site to the President.

I would like to address one other issue, the re-competition of our Management and Operating contract, which will expire in February 2001. In January, I informed the Board about our decision to re-compete the M&O contract and that is consistent with Departmental policy and Congressional appropriation intent. In February, we asked for comments on a draft request for proposals and we held a presolicitation conference. After reviewing the comments and revising the draft, we published a formal request for proposals on March 30, 2000. Those proposals are due by June 8, 2000. After evaluating the proposals and awarding a contract, there will be contract transition and phase-in periods. We have targeted the transition to begin in November of 2000, but we may begin, if we're able to, as early as August. The new contract focuses on design and licensing work scope and will require a contractor with strong postclosure performance assessment and preclosure integrated safety analysis capabilities. The work scope will permit the successful offeror to continue to use the national laboratories and the U.S. Geological Survey. We are carefully managing our current scientific and engineering activities to ensure that the timing of the re-competition does not significantly affect our primary objectives for this year.

In conclusion, we are nearing a point where the scientific information will be adequate to determine whether a repository for spent fuel and high-level waste at Yucca Mountain could be operated, monitored, and closed while protecting the health and safety of current and future generations and the environment. Approximately, $3.5 billion has been committed to the work at Yucca Mountain. After almost 18 years of site characterization and design work, we are very close to making that suitability determination.

We are now developing the documentation to present the technical basis to the stakeholders. Comments from the Board on the SRCR and the underlying technical work will be essential. My goal is to ensure that the technical basis is portrayed in such a way that it provides the necessary information to answer the questions of our stakeholders, including the Board; gains the confidence of the public; and provides a sound, scientific basis for decision-making.

Thank you very much for the opportunity to share my views with you today and I'll be happy to address any questions that you may have at this time. Thanks, Jared.

COHON: Thank you, Ivan. Just hang on, sir. Let me just review our procedures for public comment. There will be a public comment period at the end of this morning's session. If you have a question you want to pose now, no, please, you're not going to do it now. But, you can write it down and, if you'll give it to the people at the back desk, we will try to fit it in. Okay? Otherwise, you have to wait until the public comment period.

Questions from the Board? Oh, you might get lucky.

BULLEN: Bullen, Board. Ivan, I was very pleased to hear that you addressed all the issues associated with the letters that we've been sending over the course of the past months. I'm also pleased that there's a flexibility in the design associated with hot versus cold operation. But, I was a little intrigued by the fact that you mentioned the reversibility in the event of a National policy change. I guess, I'd like you to comment on in doing the flexibility analysis and the reversibility, how would that reversibility be paid for? Is there money set aside in the budget or if the National policy change did occur, then basically the national government would have to come up with the money to facilitate the change?

ITKIN: Yes. We're not factoring retrievability in part of our cost analysis, but we hold that as an option that in order to ensure the public's confidence, the national government can and it has the will to do what's necessary to protect the public and the environment. I see this program as something that must maintain flexibility in our design, that we will never be 100 percent certain as the work will happen in 10,000 years. Therefore, we have to be mindful, as we move along in the process, that we should allow as the design progresses to be able to modify the design as we go into a post-licensing emplacement.

I believe strongly that the way this program needs to be accomplished, if we get the go-ahead, is by doing a modular design so that we will do things in stages. We will monitor in stages. We will test in stages. We will offer confirmatory or not-confirmatory information and we can then adjust the design as we move forward into the emplacement program. And if, for whatever reason, whether it be for changes in National policy, we've got the materials that are now emplaced, found a significant utilization, and there's a public will now to extract these materials from the repository, we should be in a position to be able to retrieve them. Or, in the event that beyond our ability to plan, a situation develops where there isn't an ecological problem and we feel it's important now to remove materials that we will then have the capability of doing that.

One of the bases of this type of geology is that it isn't like salt where once you put stuff inside, it all falls down on top of you. We will be able to go in over a reasonable period of time and remove. So, I'm offering that as a sense of security to the public who are concerned about the what ifs. And, we can't be certain, but what we do is provide for a thoughtful approach because there will be uncertainty.

NELSON: Nelson, Board. I note your comment about if the funding level requested is not received, then the project would be forced to curtail science and engineering work. I wonder if there is consideration be given to priorities, what would be curtailed in this possible event?

ITKIN: We're asking for $437.5 million. We believe that if we receive that amount, we can provide for an acceptable level, a good level of scientific and technical work to be able to make a good decision on site suitability. If we get somewhat less, we may--we will probably still continue to work on scientific and development work for site suitability, but what we may have to do is delay some of the work necessary for prelicensing. So, if we get a significant reduction in our funding request, we may postpone licensing as much as nine months to a year's time. Which means that--since most of this stuff occurs in series, that if we delay our license application by a year, and therefore, we delay the NRC in making it's ruling on the construction, we delay emplacement which we have committed to begin in 2010 by a year. This has profound financial implications because--and, this is something that I'm trying to impress to the members of Congress--that for a few tens of millions of dollars and that's what we're talking about, we could end up delaying this for a year and incurring approximately $400 million in additional costs because, as you may be aware--and most of you, I think, are aware--is that we have been responsible for removing the assigned contracts, removing fuel from power plant on-site and storage facilities, January of 1998. And, every time we delay, we are under an--we believe we'll be under a Federal obligation. You can probably characterize that in paying rent.

So, it's almost like we're building a repository, a home for the nuclear fuel, spent fuel, at the same time as we're living and paying rent at these repositories which does not make sense, which is very inefficient from a cost point of view. And, in trying to get a handle of it, for $10 million or $20 million, we could end spend up spending 400 million. And if, for example, we are forced to, because of the potential of the prior three years of delaying a lot of our preclosure work--we've been concentrating on postclosure on site suitability--we could end up, you know, being more than a year; it could be three years or four years. And, if that were to occur, of course, it would have profound implications in terms of cost to us and also to the concerns, you know, in and around these reactor sites around the country.

COHON: A quick followup question to Priscilla's. If you do not get your budget, would you expect that that could result in delay in the SRCR, that site recommendation with the SRCR?

ITKIN: No, we do not believe that will affect the SRCR. The SRCR will be basically put to bed under the current year funding.

COHON: Seeing no other questions from the Board, I can see the top of your head, Debra, but no question? Let me just ask one question that came from the public. Grant Hedlow, H-E-D-L-O-W, would like to ask the following. He noted your observation or your proposal to work more closely with the colleges and universities of Nevada. He doesn't say it, but I assume you're being commended for that. He's wondering if you're reaching out to other people outside of the program, technical experts outside of the program, especially those who are involved already in related technical matters working not for the Government, but for the private sector?

ITKIN: Well, we are reaching out to working with the scientific and technical people in Nevada. We have approached the universities. We now are doing a number of scientific and technical studies, as the Board is aware of, with the University of Nevada-Las Vegas. We are continuing trying to foster that, but beyond just site suitability, I believe that Nevada has a history of working with nuclear technology and nuclear energy and has a closeness in proximity that, for example, Yucca Mountain, if it was constructed, could be a working laboratory on international matters dealing with waste disposal. In fact, you know, we are not the only country that has a concern about what do you do with nuclear waste? Every country and there are scores of them around the world that generate power through nuclear reactors and also have in certain cases defense related wastes and nuclear waste generated and have a concern and a need now to find a way of dealing with waste disposal. And, now, they are looking to us, the United States Government, as a world leader in this regard, and since Yucca could be one of the first of such a repository, it might allow for international collaboration here in Nevada dealing with on a global perspective the treatment of nuclear waste. So, we're encouraging--we're going out and trying to encourage the technical community within Nevada to become more involved in these matters.

COHON: Great, thank you. That's an excellent lead in to our next presentation. Ivan, thank you very, very much for your presentation.

ITKIN: You're quite welcome. Thank you.

COHON: Let me call up now our friends from Sweden. We look forward to hearing your perspectives. We'll start with a presentation from Harald Ahagen.

AHAGEN: Hello. Thank you, Mr. Chairman, ladies and gentlemen. I've been asked to give a very brief introduction to the status of the Swedish program before Mayor Carlsson gets into the actual work in Oskarshamn. I'm an expert advisor to the municipality.

I'll go into three topics mainly. The

organization or the construction of the Swedish program, very simplified, the program is organized around three parts of legislation. There's a Nuclear Act which is the core of the legislative work that gives the industry the responsibility for managing the waste. So, different from the United States, it's the producing industry that has the responsibility. It gives the authority to the Swedish Nuclear Inspectorate which is equal to NRC to review the compliance with this legislation and set criteria. The Nuclear Act also includes a three-year review cycle that has proven to be a very effective tool to provide dialogue with the different parties related to the program. Torsten will go more into that from a Inspectorate perspective.

We also have the Radiation Protection Act and in this matter it gives authority to SSI, the Swedish Radiation Protection Institute to set and implement the criteria which is similar to what EPA is doing here. SSI has recently issued specific criteria for nuclear waste management just a year ago. So, we are, I think, a little bit ahead there with fixed and set criteria.

We also have the Financing Act that regulates the financing of the final disposal system. The industry has requested or has to provide a planning report every year that is being reviewed by SKI and they recommend a certain fee to the government, the government sets the fee annually, but then is paid out of each kilowatt/hour. The foundation is administrated with a separate board and government. It's now even invested partially in stocks.

Next picture, please? The disposal concept, it's often referred to as the KBS-3 multi-barrier geological repository. It relies mainly on four barriers with heavy emphasis on the engineered barriers for performance assessment. It's the spent fuel, itself. It's a coupled canister with a cast iron insert. It's a highly compacted bentonite surrounding the canisters and the bentonite across backfill in the tunnels and Swedish crystalline rock at about 1500 feet. That's low permeability, low frequency on major fracture zones, reducing conditions, less than 210 degrees fahrenheit at the surface of the canister, no valuable minerals in the surrounding rock, no--required after closure unless an institutional decision is made to do so. But, technically, it should not be required.

Next picture, please? Siting. We are in the middle of a siting process. The current and final siting process was initiated in 1993. The program has been working on developing the concept and preparing for siting since 1976. The plans are divided into three phases. The first phase, feasibility studies, is a study of existing geological and technical and institutional information to provide bases for selection of two candidate sites. This phase has been going on since 1993. It includes today six volunteer municipalities. Two municipalities have been going through feasibility study. They have had referendums and they have exited the program. The feasibility reports are now being finished. Our report, Oskarshamn's, is already on the table. The final reports from the other municipalities will come this spring. And, industry, through SKB, Swedish Nuclear Waste Management Company, will make their decision in December and issue the two sites they have selected.

We will then enter into a process that has been unclear in the past where we, a couple of years ago, provided a proposal to government or a requirement to government, whichever you put it, that we need to have it clear a decision step going from feasibility to site investigations. That is now included in something that's called R&D 98 complimentary reporting. SKB will put all these documents simultaneously on the table in December. It includes a full performance assessment study and that is all that is actually out and is currently being reviewed shared by Dr. Margaret Federlein from NRC. It will be criteria for site selection. It will be a full site characterization program and all this package will go into SKI for technical review.

This decision legally is nonexistent. It is formally a matter between the industry and municipalities. And, we've said that will put a very unfair burden on municipality to take technical decisions. We would then be the one that will accept the method and accept the basis for the decision to select the site out of the six they've been looking at. We have said it must be the government's and authorities role to provide policy statements and scientific reviews on the method and review the quality on the bases for selection of those two sites. If everything works, the decision-making process from December will take about two years and the final decision will be a council decision then to accept or reject the selection in about two years.

Then, they will enter into site investigation. That includes the drillings and very extensive testing. And, that will take four to six years, I would guess. So, about eight years from now, there will be one site that will be subject to a shaft and a pilot repository.

I think I'll stop there and save the rest of the time for the actual work we're doing presented by Mayor Carlsson.

COHON: Thank you. Mayor Carlsson, before you start, may I ask that if you want to have a private conversation, please go outside of the hall. Hello? May I ask for you to step outside if you want to have a conversation? The acoustics are such that it carries up here. Thank you very much.

Mayor Carlsson?

CARLSSON: Mr. Chairman, ladies and gentlemen, it's a pleasure for me to be here and talk about my whole municipality, Oskarshamn, and to the title of my paper, The Political and Public Perspective on Radioactive Waste Management. My name is Torsten Carlsson and I'm the mayor of Oskarshamn since 12 years ago.

The Oskarshamn Municipality with 26,500 inhabitants is located in the Swedish southeast coast. It's far away, you know. The municipality economy is strong and the employment is high. In the local municipality, we have 13,000 jobs and the largest employers are the truck factory, SCANIA, with 1700 employees and the Nuclear Power Company with 1100 employees.

Oskarshamn is hosting three reactor blocks. The first reactor went on line in 1972, the second started in '74, the third, '85. These three reactors produce 10 percent of Swedish total electric power consumption. We are also hosting the CLAB facility, the interim storage for spent fuel; the Aspo Hard Rock laboratory for underground research and disposal technologies, the canister laboratory where the industry is developing welding technology for the copper canister. Since 1995, Oskarshamn is also one of the six municipalities studied for a possible final repository for spent fuel.

During the first half of this century, large industrial facilities did not meet much opposition. Industry was equal to prosperous future with opportunities. After the '60s, a majority of siting decisions were still taken behind closed doors. It was then announced publicly and when "surprising" opposition arose, the decision was defended. This is often referred to as the DAD phenomena; decide, announce, and defend. Initially, information was seen as a solution. Also, this strategy failed because it was still we and them and no sharing of values or participation by the concerned people in the decision-making process.

After adversity and failed projects, complete openness and participation by the public has evolved as a new concept. Complete openness and room for active participation has, however, still not been fully accepted and is still seen as a treat. Nuclear waste repositories are probably one of the most controversial siting project we are currently facing. It's a problem everybody wants to see solved, but elsewhere. The model of complete openness and participation was fully adopted by myself and my colleague politicians in Oskarshamn as the governing method when participating in studies for eventual siting of nuclear waste facilities. Consider that the initial phase of the siting process from a political perspective will last, at least, four electoral periods before we even have a formalized licensing application.

As Mr. Ahagen just told you, the reactor owners every third year shall present their plans for research and development. The Swedish Nuclear Act has formed the basis for a national dialogue on how we shall take care of our spent nuclear fuel. That has been very positive. In the R&D-plan 1992, the nuclear industry proposed siting of the planned encapsulation plant of spent fuel to Oskarshamn. The proposal forced the political leadership in Oskarshamn to discuss and determine the role and the participation of a municipality in the Nuclear Waste Program. The municipality role needed to be defined in relation to the other parties, mainly the nuclear industry and the licensing authorities.

During our international review, internal review of SKBs, R&D-plan '92, the political foundation for the work in Oskarshamn was laid. The main components were requests for Environment Impact Assessment, the EIA process to be initiated early; a defined and clear decision-making process; a systems approach to various components of the final disposal system; openness and clarity in all information and communication from all parties; economical resources to cover the municipality participation. The municipality's review of the R&D-plan '92, our policy first write-out was sent to Stockholm with an unanimous council vote and the content had a large impact, in particular, on the company, SKB, and the SKI and SSI. Initially, the government did avoid to take any firm national stand on the nuclear waste issue, but we and other municipalities involved in the program have strongly insisted that the government must be clear in its policies. This is not a municipality responsibility. During the first two years, we have seen an improvement in this respect. With the municipality veto in my back pocket, I think it was wise of all parties involved to listen to our terms and comments.

In 1994, we initiated an EIA forum with participants from SKB, SKI, SSI, and the Kalmar County and the municipality. The county Lt. Governor shares the forum and the county also provides the secretary. To date, 31 meetings have been held by the forum. Forum activities are completion of the EIA work for extension of the CLAB facility, a scoping report for the encapsulation plant, initiation of a scoping process for the proposed geological repository. In 1995, SKB sent a request to Oskarshamn where they wanted to carry out a feasibility study for a deep geological repository. All six current feasibility studies in Sweden are conducted after approval by each municipality, a volunteer process. After one year of internal discussions, municipality discussions, the municipality council approved the feasibility study with certain conditions. The municipality then formed its own organization with 40 participants in six groups to follow SKB's work and to make sure that all relevant issues were addressed by SKB. The study was formally initiated in August '97 and completed by SKB in June '99. The Draft Final Report has been subject to an extensive review and the municipality working groups initiated an extensive dialogue with the public.

The municipality policy developed in 1992 in cooperation by all seven political parties represented in the municipality council can be described by the five key elements. First, an active municipality participation and municipality proposed for siting of a nuclear waste facility can take one of the following procedures; object, be passive, be active. Oskarshamn has taken the decision to be active. This decision is supported by all political parties, also those against the participation in the project. Oskarshamn has a particular situation and the spent nuclear fuel from all the Swedish reactors will be stored in the CLAB facility. If no solution or site is found, the fuel will remain in this temporary facility. For us, the nuclear waste cannot simply be voted away.

We strongly believe that active participation contributes to a better program. The industry and the licensing authorities may have numerous experts in natural science that are understanding of public reactions and what forms the local society is limited. The local political leadership and the public themselves are far more suited to evaluate their current and the future needs. Only through active participation can this knowledge be shared by the other parties and included in the overall basis for future decisions. The active participation taken by the political leadership has resulted in an increased respect for the political system in general. A passive approach is not an alternative.

Second, forcing clear roles of the key parties, industry, competent authorities, municipality, and government, in the decision-making process. One of the factors identified earlier in the process was that the parties must act clearly in their roles. In short, we have defined the following roles for the participating parties. The government must be clear in its policies in order to give legal status to the program. The industry has the responsibility by law to develop proposals for disposal methods and siting. The licensing authorities are the independent experts who review and approve or disapprove the proposals put forward by the industry. Very important, they also have the role to aid the municipality throughout the process from review of plans to various results presented. An authority approach where they are waiting on the sidelines until the license application is available is not acceptable and puts unfair burden on the municipality to take technical decisions.

The public are the experts on the local conditions and how they like to form the future.

Third, the Environmental Impact Assessment, EIA, as a tool for local participation and real influence. We have selected the EIA as the overall method for an organized participation in the program. The EIA legislative framework allow us to work together with industry and the licensing authorities in order to develop the best possible basis for the decision to come. The actual decisions are then taken independently by each party. The EIA framework also contributes to documentation of the work and a clear track record how various questions have been treated throughout the scoping process. The fact that the county provides the neutral chairman and secretary puts further emphasis on a wellstructured and transparent process. Both the industry and the licensing authorities are a strongly supported organization of the EIA work as implemented by us.

Four, complete openness and broad participation, democracy in practice. Real public participation is probably the most difficult issue when it comes to a practical implementation. Numerous projects have had ambitions to include the public, but the public do not show up. Why? We have heard that the public does not have an opinion, that the public do not have time and interest, that the public do not trust the political system, that the public cannot influence, etcetera, etcetera. We argue that the public definitely has very clear opinions. We know from our project that the clear decision-making process is of utmost importance. People must understand what phase we are in, what the results is going to be from this phase, what the next phase is going to be, how the decision will be taken before the next phase.

We suggest that there are two particular factors that are of ample importance in engaging the public. If you want to communicate with the public, you must come to them. When you come to the public, you must have clear information, clear questions, and be prepared to seriously--seriously--address their questions and concerns. The Oskarshamn municipality has, for example, therefore demanded that the feasibility study shall result in well-defined sites where the repository surface facility and cites where the site investigation can start in the form of deep drillings. It has not always been clear to the industry why we demand such concrete results.

And, fifth, engagement of neighbors in the dialogue. The interest and sometimes fear about the final repository is not only limited to the directly concerned municipality. It also has may regional aspects. The administrative board are, therefore, of limited importance. We have decided from the start that this type of program must be seen in a regional context.

The regional efforts are taking place on two levels. On the first level, the county administration has taken a leading role in the making sure that all the county municipalities have direct information about the program. On the second level, Oskarshamn has identified the six direct neighbors as target municipalities for a closer dialogue. Each one of the municipalities council in the six neighbor municipalities have received direct information from Oskarshamn on how we work and how the questions and concerns can be included in the program.

The Oskarshamn's model for public involvement, as described above, can be summarized in the following seven points. Openness and participation, everything on the table, and real influence. Real influence, that's important. The EIA process, development of basis for a decision by parties together, decisions independently. The council as a reference group. The competent elected officials responsible to us, the voters. The public, a resource. Concrete bonds and clear study results are a prerequisite for public engagement and influence. The environmental groups, early source, really--really, they are real resource. Their members and experts give us valuable contributions. Stretching of SKB to clear answers. Legal competence; so, we ask the difficult questions. We ask until we get clear answers. And, if we don't get clear answers, they get data to go further together with us. The competent authorities, our experts. The authorities visibly throughout the process, our decision after statement by the competent authorities.

The Oskarshamn model has, so far, worked extremely well as a tool to achieve openness and public participation. The municipality involvement has been successful in several aspects. For example, it has been possible to influence the program to a large extent to meet certain municipality conditions and to ensure the local perspective. The local competence has increased to a considerable degree. Activities generated by the working groups with a total of 40 members have led to a large number of contacts with various organizations, schools, mass media, individuals in the general public and interest groups.

For the future, the licensing authorities and the Government must further clarify the view of a disposal method. We can no longer discuss method and site in parallel. We have proposed a plan for how this should be done that the authorities and the Government has now accepted. Out of the current six feasibility studies, two municipalities will be selected for site investigations. The result of the work, so far, and the final report from the feasibility study will form the basis for how our municipality will decide about the next phase. Site investigations, if the questions come.

Together with my political colleagues in Oskarshamn, I am well-prepared to address these questions. Thank you for your attention.

COHON: Thank you very much, Mayor Carlsson and Mr. Ahagen. We appreciate that very much.

Are there questions from the Board?

KNOPMAN: Thank you, Mayor Carlsson. It was an excellent presentation. I wonder if you could tell us a little bit about the terms in which the CLAB facility, that's the interim storage facility in Oskarshamn, was sited in Oskarshamn? You alluded to that imperative of needing to come to some decision about the final disposition of the wastes, in part, because Oskarshamn has all of the--just about all of the spent fuel of Sweden already in your municipality. Could you just talk about how that plays into the--what the terms were of having the CLAB facility in Oskarshamn in the first place and how that effects your work now?

CARLSSON: Oh, it's not as it has been most other places in the world. The DAD phenomena in the beginning, and the people, they didn't know so much about it and they trusted the industry and the Government people and the authorities, of course. And, the industry tell that the waste, it will be a bottle. You can handle it. It's nothing to discuss and so on. And, therefore, there have been more--we have had a hard jump to go further with the discussions we have had the last two years because people's minds and the memory of how the discussion was for 20 years ago, 25 years ago, when besides the CLAB facility came, it was different, but when we discussed the ASPO Laboratory, there was another discussion, much quieter and much more open. But, you see it has taken us about eight years. I have been a member of discussion with SKI for more than 10 years and it was in the start of the 90's. It's taken us about 10 years to come together, the industry, the authorities, the community, the region people, and we have had one goal and that goal are to take the best way--the best way to take care of the wastes on the nuclear plants. We have the same goal and that was not the situation in the '60s and '70s and '80s. And, I have had the opportunity to be mayor for 12 years and I have been a politician since--many, many years in my community. I have seen in the background how we don't--because if we do it the wrong way, the people never accept that we didn't listen to them. They'd never accept--if they don't feel that they have a real influence over the situation in my community, and if I will be mayor in the future, I must listen to the public. I am the voice of them. And, it's hard to get the understanding in the Government to work it the same way.

COHON: Dan Bullen for the last question?

BULLEN: Mayor Carlsson, thank you again very much for an excellent presentation, but I was intrigued by a comment that you made that with the municipality veto in your back pocket, you had the opportunity to influence SKB and the interests that they undertook. When in the decision-making process does the municipality veto expire? When is the decision final and your municipality has bought in and then can no longer say they have a veto anymore?

CARLSSON: It's only in the environmental situation.
BULLEN: Okay.
CARLSSON: The environmental situation, we can say it's not allowable. But, not about the waste situation where the Government could say to take care of it.
BULLEN: But, in the time frame that Harald talked about, when you come down to two sites and then finally to one site, when you get to the two sites, is there still an opportunity for the municipality to veto it?
AHAGEN: Formerly, the veto comes in when it comes and takes the decision to accept the site characterizations because they have now been defined as a nuclear facility. So, it will be after site characterization before vetoes.
BULLEN: Thank you.
COHON: Priscilla Nelson did such a good job of pleading that she gets the actual last question.
NELSON: And, this actually came from the community. They're interested in getting some relative measure, the volume or the weight of the waste that you're facing so they can put it in the perspective of how many metric tons are under consideration for storage at Yucca Mountain. Can you give us a weight or tonnage or--
CARLSSON: It's 8,000 tons in all if all the units are running until they are technical in the end. 8,000

tons.

NELSON: It's about 10 percent?

COHON: Yeah, roughly, 1/10 of what we--yeah.

NELSON: Thanks.

COHON: Thank you again, Mayor Carlsson and Mr. Ahagen. That was excellent; very, very valuable.

We can turn now to our first technical session and Dan Bullen, Board member, will be chairing that session. Dan?

BULLEN: Thank you, Chairman Cohon.

In the next morning session which I see that we're beginning without a break, we have our first talk as we press the endorse of the audience here. We're going to actually hear from Paige Russell who is going to give us an update on the design of the subsurface facilities and engineered barrier systems. And, the Board will be very interested to learn and to listen about the design evolution and the flexibility, as noted by Dr. Itkin earlier this morning.

Our second presentation of the morning is going to be by Dr. Jean Younker who will speak to us about repository temperatures and the impact on and uncertainty in performance assessment predictions and again the Board will be very interested in understanding the ability of the performance assessment to describe the coupled processes that are so difficult to handle in a hot repository.

Our third presentation this morning will be by Mr. Ric Craun who will talk about the variations in the operations to effect repository temperatures and again this goes back to addressing the issue of flexibility in the design, as noted by Dr. Itkin.

Our first presentation will be made by Paige Russell and she'll talk to us about design and subsurface facilities and EDS. Paige?

RUSSELL: Hi, my name is Paige Russell and I hope you can hear. I can't speak. I could speak if they could give me something, but at three months pregnant, they make you suffer through everything. So, Michael Anderson has been kind enough to step in for me. He'll be giving the presentation. He's a member of our waste package design team. He'll be happy to answer your questions, as will some other members of our design team that are here with us today. Excuse me.

BULLEN: Thank you very much, Paige. And, in fact, we will just save al the hard questions for you and then you can respond in writing, right?

RUSSELL: Dr. Bullen actually scared the voice out of me.

BULLEN: Thank you.

ANDERSON: As Paige said, my name is Michael Anderson. I'm the manager of waste package design. Today, I've come to talk with you in Paige's stead about changes to the subsurface design and waste package design that have occurred since the last time you were briefed on that back in June of 1999.

There have been several changes to the subsurface design focusing on changing in the total drift length excavated and the drift orientation. This came about because of changes in disposal scenarios that required a larger footprint to be evaluated. Probably the most notable one is removal of backfill. We'll talk about that at some length during the presentation. Placement of the ventilation intakes. This came about for two reasons, one of which was to put the ventilation intakes in the footprint and also to accommodate greater ventilation efficiency. And, finally, as far as subsurface, we'll talk about drip shield and the drip shield emplacement gantry which, I believe, you haven't seen before.

Regarding the EBS, we'll talk about changes to the waste package, in particular, those which address stress corrosion cracking and the final closure weld. We'll talk about changes in the drip shield from the last time you saw it. And, finally, we'll talk about the emplacement pallet which, I believe, was not briefed in the last presentation.

Insofar as changes to the drifts, the eight non-emplacement drifts for ventilation and operational standby have been moved between the drifts, as opposed to outside of the drift footprint. Intake shafts has also been located within the emplacement area. The motivation for these changes has largely been to simplify the design and construction of the repository. Of greater note is reorientation of the drifts to improve the stability and also the expansion of the upper block to provide additional contingency on the north end.

I might call your attention to the backup slides. There are two backup slides, one of which shows the orientation in June of 1999 and then a new slide which shows the orientation at present. You'll see there is a shift there. The basis for that has been additional boreholes to better understand the major fracture networks in the mountain and the reorientation results in greater stability of the drift walls.

Another issues has been preclosure ventilation was increased from $10m^3/s$ to $15m^3/s$ cubic meters per second. That's increased the ventilation of the net heat removal in the repository drifts to about 70 percent for 50 years preclosure ventilation. That also helped motivate the changes in the intake shafts in order to accommodate that increase in air flow.

Removal of backfill was an evolutionary event. Early-on in the license application and design process, it was assumed that candidate backfill materials would have thermal conductivities about .66W/m K. Subsequently, with changes in candidate materials and testing of other candidate materials, it was found that those actual conductivities were much lower, on the range of .15 to .30W/m K. Evaluations of the peak cladding temperature for design basis packages showed that there was no margin to the cladding creep-rupture screening criteria of 350 degrees C. With removal of the backfill, we now have ample margin to that cladding limit. Another added advantage of removal of backfill is it does simplify the operations of the repository.

As far as moving the shafts within the footprint, you might want to know how we're going to deal with closing those up. The shafts themselves will be backfilled with minded rock from our excavation below the plug and before the surface. Those exhaust shafts will be connected below the emplacement level of the repository which means that any water that finds its way into them will end up below the repository horizon, as is the case with the exhaust shaft. The goal of these design features is to preclude water entering into the because repository horizon, at least entrance of surface water through those mined features and also manmade gravity flow paths below the shaft seals.

The next slide shows a somewhat better--or a schematic of these things. As you can see, this is an intake shaft with a sump region. This shaft that it empties into is an empty drift and is used as a distribution system. It distributes to the major drifts along the end and then is ducted into the individual drifts. The exhausting area is taken off the center of the drifts into this exhaust main which is then connected to these exhaust shafts and then exhausted to the surface through the exhaust fans that provide the driving force.

The drip shied placement system is the concept very similar to that being used for other gantries, not only those used to emplace the waste packages, but also goes for performance confirmation and drift inspection during the preclosure period and so it's got the same kind of redundancy and capabilities as those gantry systems.

The next slide shows an example of the gantry and operation. You can see here, here's a line of waste packages. It's hard to see, but there is the drip shield itself. The gantry moves along the tracks that are used for emplacement and inspection. You can see they're staged out here past the end of the drift.

Moving on, changes to the engineered barrier system since the June meeting, there's been some substantial changes in the waste package design since EDA II. The original design had skirts which had handling holes in them into which trunnions were placed. What we've done as a result of our addressing the stress corrosion packing and final closure weld heat treatment is that we've shortened those skirts and changed the lifting feature to a trunnion ring system which we'll see in a subsequent slide.

Another change has been the addition of a second alloy 22 closure lid for final closure and this has to do with demonstrating margin to stress corrosion cracking which we'll address subsequently.

There have been some changes in the drip shield, also. In the June presentation, you saw corrugated drip shield design because of considerations about separation of that due to vibrations or rockfalls and other operational issues. That's been changed to a smooth surface drip shield which we'll see in a subsequent slide.

And, finally, the requirements to place the waste packages 10 centimeters apart from one another led to the introduction of emplacement pallet which is used to place the waste package in the transporter and then subsequently emplace the waste package in the drift.

This is an isometric exploded view of the 21 PWR absorber plate waste package. We see here this is a new alloy 22 lid that's been introduced. Also, there have been changes which we'll see subsequently to the outermost lid which is now the outer shell extended closure lid. In addition, we've gotten rid of those holes in the skirt and shortening the skirt and we now have a trunnion collar sleeve in which we attach these trunnion collars which are subsequently used in the surface facility to maneuver the waste package.

Well, what's the basis for these changes we made to the waste package? The driving force for most of these changes has been either emplacement requirements or the need to treat the final closure welds for mitigation of stress corrosion cracking. The final closure weld was moved to the lip of the waste package and, if you will, the waste package to facilitate heat treating by induction annealing. Also, because of that and we'll talk about this shortly, we had to add a second lid in order to obtain sufficient protection against rust corrosion cracking. Before the lifting holes were replaced by the trunnion ring collar, this was in order to facilitate handling on the surface facility.

As a result of material science considerations and testing results, we believe that stress corrosion cracking in the final closure weld is not credible for stresses less than 20 percent of yield. The particular stress we're interested in is hoop stress in the final closure weld. We reduced this stress in two ways. One is that we have induction annealing of the final closure weld or that outer alloy 22 closure weld, and the second is laser peening of the inner alloy 22 closure well. We don't do induction annealing on the inner alloy 22 closure lid because of feasibility considerations. As a result of corrosion considerations, we believe that achievement in depth of the depth of 6.5mm for induction of heating in the outermost lid and then finally 2 to 3mm of laser peening in that new second closure lid, we will prevent failure in the weld region for at least 10,000 years and, in fact, we believe much longer than that.

The final closure weld configuration is a bit complicated. This is a cross-section which shows the various parts of the waste package near the final closure weld. In here in the green part are the--the internal structure of the waste package. The yellow is the stainless steel shell and you can see this other yellow part is a stainless steel closure lid. The brown represents the alloy 22 barrier shell. The blue represents the flat closure lid. Then, finally, the red represents the outer extended closure lid. As you can see, there are three welds. There's the inner closure lid weld, the outer shell flat closure lid weld, and then finally the outermost weld that seals the package.

The process whereby this is done is that this lid is placed on the inner shell and then the internals are inverted with argon, the top is flooded with argon, and then the stainless steel is welded. Subsequently, the argon is withdrawn from the internals and that is backfilled with helium; subsequently, the flat closure lid is put on. It is welded, laser peened, and inspected. The final closure lid is put on. It is welded and then induction heaters are placed all around the final closure weld location, it's induction annealed, and then there's final inspections on this closure weld.

As far as the trunnion handling, I must say at the outset that we don't have a--we've been studying how to attach the trunnion collar itself to the waste package and we haven't come up with a final conclusion yet. Some of the candidate ways are to have bolts or to have some sort of a clamp mechanism. But, nonetheless, this illustrates how the trunnion collar is used or is attached to the waste package at each end. We can see that it's attached around each end to facilitate handling. When the waste package was brought into the surface facility, it's put on its bottom end so the open end is upward and then subsequently moved around the surface facility in that geometry with these trunnion collars attached and then cranes and other mechanisms can hold onto the waste package by those trunnion collars or the trunnions on the trunnion collars. And, finally, when the waste package has been completely sealed, it is made to be horizontal on the emplacement pallet and the trunnion collar rings are removed and they're, in fact, recycled back for another waste package. Subsequent to that, the waste package is handled on the pallet not only to be placed in the transporter, but also emplaced in the drift.

The drip shield changes were made to address the concern--and, I think, maybe the Board has stated it--about separation during vibrations which might occur or operational evolutions in the subsurface in the drifts or perhaps as a result of a rockfall. It provides overlap at the drip shield junctions. It also provides alternate flow paths for water which may find its way under the top of the drip shield. One of the benefits of reorienting the drifts was that the design basis rock was decreased in size from about 20 metric tons to 13 metric tons. It wasn't necessarily a goal, but that was a serendipitous result. So, because of these things, we're able to reduce titanium usage not only by reducing the thickness of the titanium due to this change in the design basis rockfall, but also the removal of the corrugations reduced the total amount of titanium that was required for drip shield fabrication.

The drip shield, as we have it now, has a smooth surface with reinforcing ribs on the side and also reinforcing numbers on the top. These structures here are meant to facilitate handling and that is how its grasped by the emplacement gantry and carried to its emplacement site. So, you see this part of the end is an overlap which provides a region for positive coupling of the drip shield together and also provides a coverage of the joint between drip shields to prevent water from finding its way underneath the drip shield.

The next slide shows a detail of the connection which is a bit busy. Fortunately, it's in two colors so you can see what's going on. Here is one drip shield and the gold is the second. There's an alignment in seismic stabilization pin which fits through this hole right here. And so, when they are put together, there is some lateral support provided by that pin and also the fact that the waste packages or the drip shields are overlapped with one another. You can see here there are flow paths that are provided so that when water finds its say near the joint, it runs into these barriers and runs down the side of the drip shield to the invert.

COHON: Michael, what's the length of that overlap?

ANDERSON: I think, it's about 10 inches, many tenth centimeters.

Another change is the introduction of the emplacement pallet. The emplacement pallet consists of two alloy 22 piers connected by stainless steel-316 tubes to hold them together. Really, after emplacement, those structural members are unnecessary, but they are required for handling on the surface facility on the transporter and during the emplacement process. I should point out that the alloy 22 is not solid; it's both plates that are welded together and subsequently heat treated.

Finally, we put all the parts together and we've got a string of waste packages that are in the drift with the drip shield in place and you can see the balance of the drift with the steel set supports. I should point out down here the invert itself is composed of steel structural members and also a granular ballast that's put in that's not shown in this particular picture in order that you can see the major features of the structure. You can see that the largest diameter waste package is the defense high-level waste package, and it has a clearance of about eight centimeters between the outer surface of the waste package and the structural members on the inside surface of the drip shield.

Now, a number of these things have served to drive up the cost of the waste packages. As you can see, the addition of extra closure weld, the annealing process, and all of these things, that includes the net cost of the total compliment of waste packages by about a little over a billion dollars. However, we do accrue almost two million dollars in savings due to the changes in the drip shield, not only the thickness, but removal of the corrugations. This caused a benefit. The policy changed a little bit, but the net benefit is a reduction of almost a billion dollars in total system life cycle costs.

So, in summary, we have made a number of changes to the subsurface facility. We've reoriented the drifts and the placement of shafts. We've reduced the cost and complexity of construction by doing this. One of the benefits of the drift orientation is to reduce to the size of design basis rock. We removed backfill in order to create margin to our cladding temperature limit. It also simplifies closure operations. We've shown you about how we've developed a conceptual design for a drip shield emplacement gantry.

Waste package changes, the most dramatic of these has been the introduction of closure lid post-weld heat treatment and peening. Certainly, the introduction of the second alloy 22 closure lid, this extends the life of the waste package greatly and provides margin against stress corrosion and cracking. We've had to introduce the use of a trunnion ring which all together and when you consider removal of the trunnion holes, the shortening of the skirts, the use of the pallets, and finally the use of the trunnion rings, all of these things help to facilitate the close emplacement in the drifts, and of course, permits post-weld heat treatment. Smooth surface drip shield has been designed to enhance resistance to shield-to-shield separation and, finally, emplacement pallet facilitates close emplacement in the drifts themselves.

BULLEN: --questions from the Board? Alberto, Priscilla, Debra?

SAGÜÉS: Thank you. Looking at the last transparency with the pictures that you have, #19.

ANDERSON: Yes?

SAGÜÉS: Yeah, the first impression that one gets about this arrangement from an engineering standpoint, is that it's a bit complicated. And, I guess, the immediate question is suppose that something goes wrong and you do have to retrieve a package from somewhere in the middle of a drift. You go to the gantry and start taking out the drip shields one-by-one and then something happens. Those things are bound to occur. Something happens and the welding gets crosswise, for example, and then others follow down as a result of that also. How do you get out of that? Is the gantry system seriously expected to take care of those things or do you--or is there still a possibility that you may end up with the whole arrangement so jumbled up that you really couldn't get anything out?

ANDERSON: I'll defer to Dan McKenzie, the manager of subsurface design, to answer that.

MCKENZIE: I'm Dan McKenzie with the M&O. The first thing to note is the drip shields don't go in until we're done. That's a decommissioning function so that the condition that we're expected to be able to retrieve from is the condition of everything you see there except for the drip shields. They're not there yet. Obviously, there's still a possibility that things can get hosed up in a variety of ways. As you say, they always will.

We talk about retrieval in two different modes, normal retrieval and abnormal or off normal retrieval. Normal retrieval is the reverse of putting it in. We use the gantry that we talked about. It goes in, picks up the packages, and brings them out one at a time. Now, this concept does not afford the ability to pick up one package and carry it over another one. If I need to get the 30th package out of there, I've got to take the other 29 out that are in front of it. I have other drifts that are equipped and ready to take those packages and place them in so that we don't have to worry about taking them outside or anything.

The one that everybody always wants to know about is the one where everything is broken. And, we have a fleet of equipment that we envision to have on hand for that sort of thing and it's--we've only really looked at the worst case. There are a lot of contingencies that would be somewhere off normal from the normal gantry which you could probably still use the gantry, but we've looked at the worst case. There's no power, the drifts fall in, you can't do anything in a normal manner. So, you have a set of equipment that is crawled around. It doesn't use the rails. You can run it on the invert. Now, you have the steel framework--you can't see it there because it's not on the picture. That steel framework is ballasted with crushed tuff. So, it's sort of a flat running surface. If you run in there with crawl-around equipment, you can engage waste packages. We used to be able to do it by engaging the holes in the skirts, but they're gone now. So, we have to use a different concept for that. But, to kind of maneuver them around and get a hold of them by the ends, we pull them up onto a thing that looks like a--it's the world's biggest dustpan and you just drag it up on it. It's called an incline plane hauler. So, we have thought about a lot of ways and a lot of things that can go wrong. As far as the work we spent a whole lot of money on it, but we do have an equipment concept for it. I guess, that's where I leave it. But, we have thought about just about everything we can think of to go wrong.

SAGÜÉS: One quick last comment. Also, from a complexity standpoint, these temporary trunnion rings, that looks--again, there is an impression of increasing mechanical complexity. Couldn't those be made part of the gantry system, as opposed to something that you just go in and then you have to screw out and do it 10,000 times or--

MCKENZIE: Yeah. We could probably go back to Michael on this one. The trunnion rings are really only used in the surface facility. By the time I get the package, it doesn't have any of those on there. They're taken off and it's placed horizontally on that pallet and the underground equipment only engages the pallet. It doesn't touch the package, at all. We pick it up by the pallet, carry it by the pallet, set it down by the pallet.

ANDERSON: One additional statement or observation I can make that is on each one there's waste packages. The receiver for the trunnion ring is still there. It's part of the waste package and so that provides something to grasp onto in a retrieval situation; off normal retrieval situation.

BULLEN: Before you leave, how do you recover from an upset situation where the package is not on the pallet?

MCKENZIE: Well, okay. That's clearly under the category of off normal and we're not sure how it got off the pallet, but we won't go there. I'm going to assume that the drift is open. What Mike just brought up will be our primary way of engaging the package will be to get something around it and engage the irregularities where that trunnion ring was. Remember, I used to have holes that I could hook onto. I can't do that anymore. So, I've got to get the package propped so I can get something around it and pull it and again I'll try to pull it up onto that incline plane I was talking about.

BULLEN: Sure would be nice just to have the trunnion rings.

MCKENZIE: Well, except for the--well, if it had a handle on it, I wouldn't argue with it, but the handles make it wider and that makes everything bigger. It makes--bigger, it makes the drip shields have to be bigger.

NELSON: Just a couple of clarifying points. First, you said that the changes in the drift orientation were chosen. To reduce costs and complexity and also to capitalize on a smaller block, being the design block that can move out, can you tell me how this reduced the complexity of construction, the change in mid-drift orientation or maybe that's the placement of shafts that reduce the complexity of construction?

MCKENZIE: Right.

NELSON: Okay.

MCKENZIE: There are multiple thoughts in the bullets there because this was a whole lot of information to stuff into 10 minutes. So, in several places, you see multiple thoughts. The change in orientation is probably worth talking about for a minute. We knew from years ago, Russ McFarland of the Board staff was a big proponent of looking at the drift orientation and we always said, yeah, Russ, we're going to do it when we get enough information to where we can think we can make a good decision. When the ECRB was driven finally and we had fracture information on the lower sub-units, that gave us the information that we felt we had to have in order to make an informed decision on drift orientation. We have a criteria that says we should orient the drifts at least 30 degrees off of any of the primary joint sets and that's just to promote inherent stability in the emplacement drifts. The mains are not so important because we can always maintain them. There's no waste in them. They're easy to access. The emplacement drifts have limited accessibility after the waste is in them and so we want them to be out in the most inherently stable orientation.

So, once we had the information in hand, starting last summer, we started looking at orientations and South 72 West orientation was one that appeared favorable and that's why we picked it.

NELSON: Okay. So, you were using the ECRB joint information in that case because that was your first look at the lithophysal zones?

MCKENZIE: Yes.

NELSON: Are the steel sets everywhere now?

MCKENZIE: The ground support system that we're looking at now has steel sets throughout and we're looking at possibly using grouted bolts as supplementary support, as well, in the non-lithophysal units.

NELSON: Okay. Let me just ask one final question related to this. How do you envision the tunnel deteriorating with time? You've talked here about seismic design considerations. Are there other mechanisms for the deterioration that you're considering?

MCKENZIE: Nothing real progressive or extreme. We've looked at--first, looking in the heated drift even when you've got pretty extreme conditions, you've got little bitty raveling and little bitty pieces falling off, not too many of them. In the main tunnel, you see a little bit of raveling from continued vibration of machinery moving up and down the tracks and stuff. There doesn't seem to be a real progressive deterioration though. As far as the AMR/PMR process which you're familiar with, we did an analysis on drift degradation where we looked at key block formation and successive key block failures and it would be fairly small percentages of the total amount of drift that appeared like they might be affected by degradation and progression of the key block development. So, we don't see a lot of--that's going to get damp and swell or something and fail that way. We don't see that kind of mechanism.

NELSON: So, the deterioration is solely thermal cycling related that you're looking at?

MCKENZIE: Right.

KNOPMAN: A few clarifying questions. First, the granular ballast that is not shown there, but you've alluded to, could you just explain briefly what the purpose is? Are you hoping for it to facilitate drainage or not?

MCKENZIE: I don't--no, it's there as ballast, frankly; the same sort of ballast you use to ballast railroad tracks. It's just here to make the invert nice and solid so we don't have a lot of differential movement. We don't assign any sort of diffusive--any waste isolation properties to it. If we could find something that would perform that function, we could certainly put in there.

KNOPMAN: I was just thinking about the humidity control underneath the drip shield. If you inhibit drainage through the ballast, do you then create a little hothouse in through there?

MCKENZIE: You're kind of getting out of my area now, but it's just very coarse material. It's not--it certainly shouldn't--it shouldn't inhibit much drainage. Water should move fairly freely through it.

KNOPMAN: All right. Can I ask two quick other questions here on different subjects? Do you have a facility where you have a prototype can that you're working on and testing these various weld techniques on or is this being done at kind of laboratory scale at this point? You're talking about numerous multi-stage welding process. Our Swedish colleagues have a fairly sophisticated new facility that's specifically designed to try out these various welding techniques. They're running a lot, I believe, from actually doing it on the scale of the can envisioned there. What are you basing your various design changes related to welding on?

MCKENZIE: Jerry? This is Jerry Cogar (phonetic), our welding expert. He can address those questions.

COGAR: Yes, we've been working on a development program for the closure well, as well as the fabrication since 1995. In that time, we've already produced two mockups that are in current designs. One was a design of carbon steel with alloy 625 and then later carbon steel with alloy 22. This year, we're producing a mockup that has the same configuration that you see here with the alloy 22 on the outside and stainless steel on the inside. These mockups have been approximate diameters to represent the range of waste packages, but have been about 44 inches long, obviously, to reduce costs and to make the handling easier. We do most of our welding at a lab in Lynchburg, Virginia and we do the fabrication of the mockups at various fabricators around the country, one at Raynor (phonetic) in Massachusetts, one in Cleveland, Ohio, and St. Louis. So, we get a number of fabricators involved and we get a number of ideas on fabrication, as well as wealth. We had made the alloy 22 thickness welds before, but not this precise configuration which we will do this year in about August.

KNOPMAN: Okay.

COHON: Could I just ask a question while we have him at the microphone? Do you have an estimate of how long it would take to prepare a package for emplacement from the time you put the fuel in?

COGAR: Yes, we gave an estimate to the surface facility and, obviously, that's based on a number of things. Because we've done the alloy 22 weld, we have a very exact arc time on that and we'll have another one this year. That's approximately five hours to complete that weld. Now, you have a setup time in there, obviously. You emplace the package to emplace a lid. To make the inner weld, we have not made that weld, but we made a similar carbon steel well. So, we have very accurate arc times and we have--and, I believe, the number, off the top of my head, was like 24 hours total. But, if you look at the arc time itself at about a 70-inch package which is approximately 210 inches, give or take, in circumference, and about seven inches of travel speed a minute, you get approximately 30 minutes to make one pass. Our weld design is a narrow drift closure weld with auto tig. And, you get a deposition rate of about 1/16. So, you're about 16 layers or about eight hours. Our actual time make that weld because of the deposition rate changes with hot wire tape last year was just a little less than five hours. So, we can pretty well set how long everything takes with the exception of the induction annealing and the laser peening and we've given that the best estimates from labs around the country that have told us that. We'll find that out more when we do the induction annealing at the end of this year.

COHON: Thanks.

BULLEN: Can I follow up on that? You mentioned the weld time and you mentioned fabrication time including induction heating and laser peening. What about rework time and nonrestricted evaluation? Are you going to do NDE of all the welds, and if you are, does that include the rework time necessary to grind out the weld and redo it if you find a flaw?

COGAR: I think your question is on the closure weld. Is that right?

BULLEN: Well, actually, on all the welds. Are you going to do NDE on the thick 316 weld or are you just going to leave it?

COGAR: Those are welds done in the waste handling building, not the fabrication itself.

BULLEN: Right, exactly.

COGAR: We'd going to do an NDE on the stainless steel weld. We'll do an ultrasonic inspection, as well as a visual. We'll do an ultrasonic inspection of the inner alloy 22 lid weld and we'll also do an ultrasonic inspection of the outer well. Now, we've done all the ultrasonic on all of those already except for the middle end which we didn't have before this year. We're looking at a number of ultrasonic initiatives, such as they have real time ultrasonics with the rolling wheels that INEL is working on. They have non-contact ultrasonics which some of them are laser based. They have the EMAT system. So, all of those, we're looking at. But, in the meantime, we're able to go in there with just an automatic crystal and do those ultrasonic constructions and we have done those even remotely.

BULLEN: I guess the question also deals with rework then. If you find, for example, you don't get wetting on the walls or the deep penetration 316 weld and you have to go back and rework that, is that time to grind it out and fix the weld and then incorporate it into the associated timing for the packages or do you expect not to happen?

COGAR: We have not given them a rework time within that scope or time and said how long does it take to prepare this package and put it underground. We have not done that because there's still discussion going on about how is that rework going to be done? Will this be taken off line and go to a rework cell or what? That has been our, I guess, opinion of how it should be done. You take it out of the line, you take it for rework, and you rework it if you need to. You don't use that to clog up the line.

BULLEN: One final question about rework then is that if you do take it out, then would you be at a facility where you'd have actual manned access to the surface to do the rework? Doing remote grinding and seeing what you're doing is going to be a real challenge, isn't it?

COGAR: It is a challenge. It's not impossible. It is done in some instances. We would not anticipate manned access there, although that has been recommended and has not been ruled out simply because of all the shielding you need to do that and the radiation levels on the package itself. However, what we want to design is a very good system that gives us a high rate of acceptability.

BULLEN: I understand that and that's a very good point and I'm not going to mention self-shielded containers. But, what I am going to mention is if we put a shield plug at the top of the thing like a dry cast storage shield plug and you had to get back in there and do the rework, you could remove it to a cell where you can actually have access to the weld and it might save you a great deal of time and effort, particularly in light of the fact that key variabilities in 316 may not give you the welding up the sidewalls of the deep groove weld that you expect to get. Those kinds of surprises are easy to mitigate if you can get in there and grind it yourself.

COGAR: I wouldn't object to that as a manufacturing person. However, it's the design--

BULLEN: Right. I understand it's a policy issue with respect to it, but not fully shielded packages, just a plug on the top. Just a couple of more questions and we have to break. Next in line was Jerry, I guess.

COHON: Can we go to Slide 3? I'm interested in the bottom, the preclosure ventilation weight and the assumption of the 50-year preclosure period. I know with Ric Craun's presentation later on, we'll be getting into this in more detail. I just want to be clear on my understanding of the assumptions made here. First of all, why did you increase the ventilation rate from 10 to 15 m^3/s?

MCKENZIE: At the end of the LADS, we developed a set of criteria to carry forward to impose the design. One of those criteria came out to be we needed to remove 70 percent of the heat produced over a 50-year period. That was in order to be sure that the boiling fronts didn't coalesce between drifts.

COHON: Let me just get this. So, the key driver, though, was to avoid coalescence of the boiling fronts? That's where we--

MCKENZIE: Right, yes.

COHON: All right.

MCKENZIE: And, when you do the calculation, you end up 10 percent--10cm/s doesn't quite do it for you, 15 does. So, that's a pretty simple answer.

COHON: Okay. And, what did you assume in terms of average age of the fuel and also the distance between packages end-to-end?

MCKENZIE: Okay. The average age of commercial fuel is about 26 years. That hasn't changed too awful much in quite a while. This spacing is 10cm.

COHON: 10cm, okay.

BULLEN: Thank you. Norm?

CHRISTENSEN: Maybe go back to 19, if you would, and this is, I think, a followup on a question that Priscilla had. If you could just comment for me on the basin pattern of deterioration of the invert, how it relates to the ballast? I'm just trying to picture what's going to happen in hundreds/thousands of years as the invert deteriorates. Does that affect the disposition of packages; can it?

NELSON: Just maybe from the amendment? No, what I'd like to wonder is that ballast, when is it placed? Is it placed during construction to hold the emplacement canisters or is it after construction you have engineered ballast in there and place the steel invert? When is it placed?

MCKENZIE: It's placed--it's not placed during construction of the tunnel, but it's placed--we have a function in our cost estimate that we call finishing which is once you drive the tunnel with the TBM, you pull the TBM out and take all the construction, strictly construction, equipment out, the ventilation tubing, that sort of stuff, you next come in and install this invert in segments, the steel invert, and then ballast with then. It's there to ballast the floor of the tunnel so that it provides a good, solid running surface. You have a reasonably heavy gantry with a 50-ton package. So, you need a really good foundation. So, it's placed before the packages are emplaced during what we call the finishing period.

In terms of degradation, the fact the ballast is there and that the rest of it was not welded tuff and is carbon steel which will corrode actually over time and kind of swell, there's not going to be a whole lot of sinking, you wouldn't think. We expect the invert to stay, certainly, in the preclosure period in reasonably good shape because of the ventilation of very dry air, corrosion should be very, very slow.

BULLEN: Norm, do you have any more questions?

CHRISTENSEN: I'm fine.

BULLEN: Paul has a quick followup on that.

CRAIG: There's an awful lot of steel shown in there, and in the past, there's been discussion about potential problems with the steel contacting the titanium or the C-22 and doing electrochemical things. Why is there so much steel in there now?

MCKENZIE: Well, there's a lot of steel in there because there used to be a lot of concrete in there and the concrete went away because of the perception of pH problems and other long-term performance negative possibilities. As an underground designer, in a good application like this with a particularly very long life and low accessibility, I'm looking for something robust. I've really have two choices; one of them is concrete and one of them is steel. The concrete went away. So, I only got one left. So, that's why there's a lot of steel. So, if steel becomes a big problem, we've got a couple of choices. We can decide whether steel or concrete is a bigger problem and use the one that's a smaller problem or we could go to bolts and meshes on it, but I think that wold be a long-term maintenance problem for the repository. You could minimize it if you really had to. If somebody demonstrates this problem, we'll figure it out later on.

ANDERSON: One quick followup. On the bottom of the drip shield, there's an alloy 22 foot and separates the titanium and drip shield from the steel invert.

BULLEN: This is a chairman's prerogative and all my fellow Board members did a great job of asking almost all the questions I wanted to and Professor Cohon is looking at his watch. But, I have a couple of quick questions on

Chart 6. If you go back, this is going to be a recurring question and I'll apologize for it, but I still have to make it. The question is why is the exhaust main below the repository horizon?

MCKENZIE: It seems more important for it to be below than it was before, but it was below because we had a choice of putting it in the frame above or below. We didn't put it in the frame because it takes up space; so, that left above or below. Above, it potentially can accumulate water which because that drift has to be connected to the emplacement drifts, that water gets retaken right down to the emplacement drifts which is where the packages are. So, we put it below just out of the least offensive of the three possibilities. Now, it's more important for it to be below because we have these off-shafts that actually tie in straight from the surface down to it and it makes a good argument for prevention of pathways to have the main exhaust below because water that runs down that shaft ultimately has got to run uphill to get back to the waste package.

BULLEN: But, could you see any benefit, at all, about putting it above? I mean, the water that goes down the shaft, you could actually put a sump or make it go lower and you can take the feed off on some other point.

MCKENZIE: If you wanted to put it above, you could.

I'm not sure, it's probably a secondary or tertiary performance impact. It's probably not going to be a big driver one way or the other. I didn't see a compelling reason to move it and so I haven't moved it.

BULLEN: I'll keep asking. Thanks. Could you go to Slide 13, please? The final question--this is a quick one--that final closure weld is an induction annealed. Is it a complete solution anneal or is it just a stress relief?

MCKENZIE: Dr. Gerald Gordon will come to address that question.

SPEAKER: Which one was that?

BULLEN: The top weld. The outer extended closure lid and closure weld, I questioned is it a solution anneal or is it just a stress relief.

GORDON: Currently, it's going to be heated up to 1120 Centigrade which is a solution anneal temperature, but for a very short time.

BULLEN: And then, how do--

GORDON: --relief of stress.

BULLEN: How quick is the cool down expected to be?

GORDON: Less than 10 minutes below 500 Centigrade to keep from thermal aging.

BULLEN: So, you miss the nodes of that TTT code?

GORDON: It misses it, yes.

BULLEN: Thank you. Last question and this is to Michael. As you put the drip shield over the final emplaced packages and the packages are at 10cm apart, four inches apart, if you modify the design so the waste packages are farther apart, do you still put drip shields along the entire drift length?

ANDERSON: I think it would depend on how far apart they are because they reach a certain distance and then you put ends on them because there would be a net savings in titanium.

BULLEN: Good answer because it's expensive to do that.

Any other questions from Board members? Debra Knopman, last question.

KNOPMAN: With all these design changes, do you anticipate going back into the EIS and making adjustments to conform with these kinds of changes or is that not going to happen?

ANDERSON: That's a little out of my area, but I think that most of these would be transparent to the EIS.

KNOPMAN: Excuse me?

ANDERSON: I think that most of the waste package design changes, per se, may be transparent to EIS, but again I'm not all that conversant with EIS.

BULLEN: Thank you very much. In the interests of time, we're going to take a break now. I know everybody's bladder is probably in favor of that. We will adjourn for 10 minutes. Back in exactly 10 minutes, please.

(Whereupon, a brief recess was taken.)

BULLEN: Let's reconvene. But, before we do so, I want to make a couple of announcements. First, we are using this facility under the good graces of the City of Pahrump and we would like to ask you to help us in picking up your coffee cups, your juice containers, your napkins, and placing them in the proper disposal containers which can be found in the back of the room and help us keep this place tidy because we're responsible for returning it in the condition in which we found it.

Now, we're going to move onto the next presentation of this morning's sessions. If you would like to continue your conversations, please, do so outside. Professor Cohon pointed out this morning that we can hear everything very well up front.

Our next presentation is by Jean Younker who will speak to us about repository temperatures and their impact on the confidence and uncertainty in performance assessment predictions. Jean, thank you?

YOUNKER: Well, I'm pleased to be here to talk with you today. The purpose of the talk is to summarize the categories of uncertainties that we are aware of and are addressing in one manner or another and thermally-driven processes; to highlight the testing, analysis, and modeling efforts to address those uncertainties; to get your feedback to assure that the uncertainties that we're looking at are the uncertainties that you think, you know, are really of concern relative to thermally-driven processes; and, then, finally, I think there's already been discussions and we'll end with the proposed path forward for some more detailed future interactions where we can really talk in more depth than what I can in the next 20 minutes or so.

Thermally-driven processes certainly increase uncertainty on repository performance for a number of reasons that I have on this slide. Physical-chemical changes clearly are a function of time and temperature. The magnitude, volume, and duration of coupled thermal-hydrologic-mechanical-chemical effects increase with increasing temperatures. Repository time frame is much longer than the testing period. This was much of what I said before in some preliminary comments. So, both for that reason and because the thermal disturbance is over a larger distance than we can probe by our tests, it's clearly important for us to recognize this, to look at maybe analogs that would give us a potential for getting information along the time periods, and over larger distances, such that we can get some information to help us with one aspect of uncertainty which, of course, is scaling of our test results to repository scale performance. And, performance predictions for site recommendation/license application clearly include the uncertainties in the various thermally-driven processes in order to be credible. I think you've made that clear to us about your concerns in previous communications that have been summarized earlier. So, we are concerned. We're here to kind of hopefully open further dialogue, get your feedback to make sure that the types of uncertainties at a high-level that I'm going to talk about include the ones that you think we should be looking at and then propose some further interactions.

The near-field environment processes that we are looking at--and much of this is going to be review because we have had fairly detailed interactions in the past about various aspects of this discussion. So, design features for the discussion that we're going to talk about are for the type of processes that we're going to talk about and have already been discussed in a couple of other talks, but we're looking at the effect of the 50-year preclosure period, that time frame with the thermal loading of 60 metric tons per acre which is line loading of approximately 1.5kW/m, the waste package spacing of a tenth of a meter and the drift spacing of 81 meters which you don't get that sense of scale in this cycle. You will in a cycle I'm going to talk about in just a minute.

Now, to give us some kind of a sense of the thermal impacts, what we tried to do here was to not only highlight some of the types of processes that we need to consider in our modeling, but to also tell you what the results look like given those design features above and the predictions that we make with our thermal modeling. So, let me say that, you know, from the standpoint of the things that we do are important, we know what we can consider, you know, clearly it's minimal transport redistribution by mobilized water, where the water goes, what it does in terms of changing permeability, fracture permeability and matrix permeability, in terms of cladding fractures, coding fractures, and if you read the detailed words here, you'll see that there are various types of processes highlighted that are aimed at understanding the mobilization of water, where it goes, and what it does to the permeability. And, you will understand them from previous discussions. Clearly, that's, at least, one focus of your concerns about thermally-driven uncertainties.

To give us a sense on the scale, the maximum boiling extent occurs over--at some time between 200 and

500 years given the design parameters that we've outlined for you here. So, you're talking about this type of a boiling extent with the boiling number going out and then coming back in over that time frame of something like 1200 to 2000 years. I've giving you the ranges, as you are well aware, depending on which of our modeling approaches you use. In this particular case, depending on the assumptions that you make for infiltration, you get a range of values for the time at which the drift wall would drop below boiling. So, for a period of 200 to 500 years, the boiling front is moving out to this dotted line. It comes back to below boiling at the drift wall in a period of somewhere less than--or somewhere in the range of 1200 to 2,000 years of our 10,000 period of regulatory performance. And then, to give you another point in time and space, the drift wall is approximately 50 degrees C at 10,000 years and that is about the same number depending on which of the modeling approaches and the assumptions that we make. So, that one is a pretty consistent number.

I might say--back up for one second, John. I might say one other point. The extent of boiling that's shown here is not exactly to scale, but it's about 1/4 of a pillar in terms of scaling and that, as you know, is our design requirement to not have the boiling--exceed 1/4 of a pillar. This is approximately, trying to give you a scale, given your--diameter drift, this is approximately the maximum extent that will be allowed given the designing time placed on the extent of the boiling front.

To summarize some of the categories of uncertainties that we are addressing in one manner or another that we recognize we need to address, we have the categories here; hydrologic, mechanical, chemical, and then the thermally-driven uncertainties relevant to corrosion predictions and waste form degradation. We thought we would summarize them for you. This is just to make sure that you have an understanding of the types of thermally-driven uncertainties that we believe we have to address once again to lay the groundwork for getting your feedback. If there are other ones that you think we should be considering, we're very open to that discussion.

The hydrologic uncertainly, clearly, we believe you've made clear to us; the concern is the volume and fate of mobilized water. How much water moves around and what effect does that water have in terms of potentially bringing more water back into a drift environment at the time that we down the temperature gradient.

The thermally-driven potential of mechanical effects, movement of rock above the drift and I'll highlight this in one slide later. Another question or another area that came up already, I think, in Priscilla Nelson's question, drift stability and rockfall; the question of whether the extent to which you raise the temperature in the rock mass increases the uncertainty about drift stability and rockfall. That's a question that we clearly need to address.

In the chemical category of uncertainties, the question of mineral precipitation in fractures, dissolution and precipitation, redistribution of minerals, the question of altered water chemistry concentrations, pHs, Ehs, the things that make a difference when that altered water chemistry gets in and contacts the engineered barrier system, and the potential for mineral transformations. This is more of an issue if you're talking about zeolites going through transformations at temperatures where they may dehydrate or where they may transform.

In the corrosion realm, we need to be aware of and I believe we are of the impact of thermally-driven processes on the mechanisms of corrosions that are of concern, the rates of corrosion, as well as the environment of corrosion, once again, coupled back to the types of altered water chemistries that may come into the drift.

Waste form degradation--I think this one, Michael

Anderson already talked about to some extent--clearly, the 350 degree C requirement that we place on the center line of the waste package to protect the cladding is a recognition of a very strong thermally-driven process that we need to be concerned about. The solubility of the waste form and the rates of degradation are also thermally-driven to some extent and I'll come back to that in a later slide. I'll talk about where we think we are in current understanding, although my intent is not really to try to communicate to you that we have all the answers, but more to lay out what we believe the uncertainties are that need to be addressed.

Okay. This slide was put together to give you and to give ourselves a picture of perspective. When I say approximately to scale, I don't really mean to imply that I believe we've got it right in terms of the shape of the dryout zone or how big of a condensate zone we get or even if we get a really large condensate zone in every location above an emplacement drift. What you are looking at here--let me be clear--is two emplacement drifts approximately 81 meters apart, scaled. They will be 81 meters apart. My scale is probably not perfect since this isn't really an engineering drawing. However, given the 5.5 meter diameter, we tried to draw this so that this is about the right scale in the horizontal dimension. So, that's the part of this that is approximately to scale.

The average extent of the dryout zone is shown here, and to try to give you a sense for that, to some extent it was to give you a sense for how much of the pillar in the average part of the repository would remain below boiling. The drift that we used here is loaded in the middle of the emplacement schedule. So, it's kind of the average drift in terms of the ventilation period that it has experienced. This boiling front for that particular drift and the calculations that we were using as a basis for the front had about a 9 meter boiling zone around it. So, hopefully, it gives you a sense of the kind of volume of rock that we are taking to above boiling conditions. We believe that the shape, in general, of the dryout zone and the boiling zone will be somewhat elliptical in that there's some buoyancy effects that causes to have the condensate zone above. This is very schematic. Whether you get some condensate zones down in the sides, clearly, there will be some evaporation and condensation in all directions around the boiling front. It's just a schematic to give us some chance to really visualize what it might look like.

Okay. Moving to the hydrologic and chemical processes uncertainties, this slide is intended to convey to you, on this side, the thermal hydrologic processes that are of concern and must be addressed and incorporated into our understanding and our modeling, and on the right hand side, the diagram shows the thermal hydrologic chemical processes. We'll know that we'll get some evolution of CO_2 during the boiling phase. We know that we've got some changes in relative solubilities that need to be incorporated in our models to make sure that we understand what kind of redistribution of mineral phases may occur during the thermal pulse. For example, you're aware, I'm sure, from previous talks that calcite solubility, which is kind of shown down here, will decrease with increasing temperature while silica solubility will increase. So, we know that we're going to have some relative dissolution precipitation reactions going on in the fractures, as well as in the matrix. Some mobilization of silica as the temperatures go up that has the potential to change the permeability along fractures in a way that raises uncertainties clearly. Does it fundamentally change hydrologic properties, such that we could have some increased amount of flow focused back into the drifts, is the question, I think, that's on the table that has been raised both in some of your communications and by others.

From the thermal mechanical impact category of uncertainties, this is just to give us something to think about in terms of a calculated model result of an enhancement in fracture permeability due to thermally-induced shear. Now, we have results for normal displacement, as well as shear. The normal displacement increase in permeability was much less, but what you'll see if you focus right here on the screen is that above the emplacement drift which is the white circle here for the conditions that we've been looking at throughout this presentation, you'll see that on my multiplier value for fracture permeability, I'm showing a 10-fold increase in shear permeability. Show thermally-induced shear movement such that fracture permeability is increased by a factor of around 10. So, that significant number, does it mean anything to us in terms of the kinds of changes that we're going to get in transport of water into the drift when water can come back after the boiling front has collapsed. That's one of the uncertainties again that we're going to have to look at. And, at the normal displacement, I might mention the factor, the increase in fracture permeability was just something like a factor of 2. So, the thermally-induced shear seemed to be driving a larger change in fracture permeability.

Now, for corrosion which certainly had a lot of discussions with you about the effects of temperature on corrosion, we've already talked a little bit about the near-field host rock, the potential for accumulation of-- or redistribution of mineral phases and potential for movement of water that has higher dissolved content because of the temperature increase coming back into the drift. Contacting the drip shield in the waste package causing potential for concentrated solutions on the surface of the drip shield, that's something that is an uncertainty that has to be incorporated into our modeling in order for us to have a credible basis for predicting the corrosion performance of the drip shield material. I think, we already mentioned about the invert. I think, Debra Knopman mentioned is there a possibility of some kind of deposition occurring in the ballast material, such that you could plug or cause areas of higher moisture content, potentially increasing the humidity? Even before liquid water is back, you could still have some increased humidity here that would not occur if this is free-draining. So, I think that's a very good point that we're aware of and we have to consider in our modeling.

From the standpoint of corrosion performance, the general and localized corrosion has a relatively low dependence on temperature. The pitting and crevice corrosion not strongly driven at expected conditions, but we are continuing to test that, as I think you're aware. Stress corrosion cracking is temperature dependent at around 100 degrees, but less so otherwise and another one that's certainly being tested. And, phase segregation is low temperature dependence for temperatures below 260 and this again is being looked at through testing.

For waste form, to finish the categories of uncertainties that I have in the opening slide, we've already mentioned the degree of cladding degradation is temperature dependent and that rate of cladding degradation increases rapidly above 350 degrees C or in that range. It concerns both about creep rupture of the cladding, as well as unzipping. Solubility is mildly temperature dependent depending on the chemistry and colloid stability gives you a little bit of temperature dependence for the solubility of the waste forms. And then, the degradation rate, dissolution rate varies for uranium oxide by an order of magnitude between 25 and 96. So, there again is another temperature dependency that has to be incorporated into our performance modeling in order for us to be capturing those uncertainties correctly.

Now, I'm not going to spend time to go through this, but just to simply review for you that either complete or ongoing, we have a number of tests, the drift scale test, the single heater test, large block test, which you've had visits to and many discussions of, the cross-drift test which we're planning and setting up to conduct some of the analogs that you heard about from Ardyth Simmons, I think, in the previous Board meeting where we may get some insights into certainly scales that are difficult for us to get from our tests, as well as time frames that are difficult for us to gain information about without going to some of the analog type approaches for information. The international group that's looking at coupled processes certainly is a potential source of help to us in getting a better handle on how to address these uncertainties related to thermal effects. For all of these categories of uncertainties, we get some insights from our testing and then down in the closure waste form, it's the laboratory testing, of course, that's very important to us. And, I think Mark Peters is going to talk a little bit more about the natural barrier side of the testing program. We do have people here who can answer specific questions if we need to later on the corrosion waste package testing area.

Now, to pick up just one of our field tests that's really important to us in the specific area that we are talking about which is volume and fate of mobilized water, I wanted to show you a cross-section through the drift scale test and some of you may have already seen this in an earlier discussion. Mark Peters probably will refer to it in his presentation, as well. But, the observations that we're making in that test, we believe, are really important to giving us some confirmation, some validation of our understanding of both can we, in fact, predict the temperatures in the rock as we put the boiling front out into the rock and also where the water goes. Now, prior to the start of the test, some of our predictions did indicate that water would pond above the drift due to thermal response and I think we've had those discussions with you. To date, the observations indicate at this point in the test, which is not quite half done, that the water does not seem to be ponding above the drift. It appears to move to the sides and below.

If you go to the next slide, we have a color slide. Now, this is a transverse section through the heated drift and the saturation ratio is the ratio of the current ERT saturation to the saturation at time zero at the start of the test. These are electrical resistance tomography results that allow you to see and compare what the saturation change has been. And, as you'll notice, the high saturation ratios are down here below the drift, number of transfer sections through the drift, again with the bulkhead here. So, you can see that we are getting some high saturations below, but relatively not high saturation, certainly not in this area here, but down in the 60 percent. If you assume that it started out around 95 percent plus saturation, then you're seeing that this is, in fact, reaching 100 percent saturation in this area right through here.

Oh, let me go back one second. I wanted to mention it's 511 days of a 1400 day plus test. So, you know, this is a snapshot in time. It's not saying that we aren't going to see some additional behaviors here, but I think it's interesting to note at this point, you know, about a third of the way through the test that we definitely are seeing some increased saturations below the drift.

Now, from the standpoint of how do these uncertainties get translated into uncertainties in predicting performance, this slide was put together by Bob Andrews, our performance assessment technical manager, and for each of the uncertainties, what he gave me was the parameters that in the performance assessment models are the most reflective or that are the most useful in capturing the uncertainty relative to that category of uncertainty that we've been talking about. So, as I mentioned in the opening discussion, it's so critical that, number one, we recognize the uncertainties, that we address them in some manner, and translate them into performance in a way that's credible that we can explain to you and to other reviewers of the total system performance assessment and gain your confidence that we've adequately treated those uncertainties, reflected them in a way that the predictions that we get from the performance assessment modeling are credible.

So, for hydrologic uncertainties, the parameters that are used are a flow focusing factor and some parameter relative to condensation. Then, what Bob has given us is how does that affect performance and what impact does that have from the standpoint of actually seeing a difference in performance? In this case, it's clearly the seepage fraction and amount. Again, that amount and fate of mobilized water category of uncertainty related to thermally-driven processes and the water flux that can actually reach the waste package.

For mechanical, the fracture flow characteristics, rockfall size, and frequency again get at that--are sensitive to the seepage fracture and amount. As we mentioned earlier, the drip shield stresses and the stress induced cracks on the drip shield, this would then be bringing us into the predictions of drip shield performance and the rate of degradation of the drip shield.

For chemical, fracture flow characteristics; again, getting tied to the seepage fraction and amount.

For near-field geochemistry, it's how that translates into in-drift geochemistry. For fracture and matrix transport characteristics, we're now getting into the question of how does transport actually occur through the unsaturated zone.

For corrosion, we've already talked about these on the previous slide. So, I won't spend the time to go through these. I think I mentioned the corrosion rates and the types of mechanisms of corrosion. And, for waste form degradation, again performance of the cladding and the solubilities.

Okay. So, the path forward, we believe is to investigate these uncertainties through the testing that we have ongoing and through testing that is planned. As you know, the next talk by Ric Craun will talk about the operational flexibility that we've developed in our design for SR such that we can accommodate those uncertainties. And, if future understanding of uncertainties is such that it is deemed necessary to avoid boiling at the drift wall, we believe we have some design parameters that can be exercised that will allow us to reach that design solution. So, we feel comfortable that we have both the testing ongoing and some flexibility and they'll tend to our design as we proceed towards site recommendation. We propose to you--and, I think, DOE has already had this discussion with you and so I'm not offering that out of line as a contractor, but to say that we are very interested in talking with you in detail. I believe there may be an August meeting being planned to at that point go through an in-depth discussion of our current understanding, bring in our best technical folks, and lay out what we understand about the uncertainties, what we're doing to address them, and further then how we've actually rolled them in and treated them in our performance assessment for site recommendation. So, we believe that would be extremely valuable and we hope we're able to do that.

Thank you.

BULLEN: Thank you, Jean. Questions from the Board? Paul Craig?

CRAIG: Jean, you've certainly made some progress on identifying key parameters to look at and that is good. You've shown us how you've got uncertainty in certain areas, and as more information comes in, and in some cases, your uncertainty will go down; in other cases, your uncertainty will almost certainly go up. What I'm interested in is how to take that kind of thinking and incorporate it into an understanding of uncertainty with respect to the actual repository. So, I have to go beyond the specifics of your talk to talk about the general area.

For the various kinds of quantities that you talked about, are you going to give us statistical uncertainties on a particular number like a corrosion rate and give us a signal plus or minus and tell us that for some reason which you will explain to us you think that the distribution is below normal or normal or something else? That's one approach. But, again, if you have a distribution, you need to tell us why you choose a distribution.

Then, there were model uncertainties. Model uncertainties are very tricky. When you talk about stress corrosion cracking and you extrapolate some experimental data out into the future, there has to be an underlying theoretical construct of some sort. Maybe not well articulated, it needs to be articulated so we can talk about the uncertainty in that.

And then, there is the issue of components and the Board's interest in breaking down the system so that we can provide--we can do some defense-in-depth analysis or at least defense-in-depth thinking as an alternative approach and that also is related to uncertainty.

So, what occurs to me about the presentation that you gave is you've got a list which looks like it's a reasonable list, but I don't understand, at all, how you propose to go from that list into specific statements about the treatment of uncertainty. That seems to be lacking at this point. To my way of thinking, it's absolutely essential.

YOUNKER: Let me think about this now. There were probably three parts to your question and I think that certainly in some cases if it's a kind of uncertainty that really is reflected in a parametric, you know, in a PDF, then in that case you can characterize it statistically. Although, I think in some cases we are probably in a situation where we have a combination of different types of uncertainty really reflected in the PDF that we're feeding into performance assessment. So that we're going to make some attempts, I believe, to try to identify the different types of uncertainty, but I won't commit to you that that's a huge part of our focus at this point in time. I may in a minute ask Bill Boyle if he wants to comment because we are going to put some attention on that.

The modeling uncertainties, you know, if you step way up at the level of alternate models, you know, are there alternate models that are consistent with our understanding? In that case, you really do have to consider in performance assessment, at least, and completely alter the approach if that's still on the table and consistent with the information. So, I know in past performance assessments, we have, in fact, had two different ways of characterizing a certain process and you look at the effect of representing in those two end members and look at the results, look at the sensitivity of performance to those. So, you know, from a modeling uncertainty standpoint, I think there's a way to do it and I think if we sit down and look at every one of the discrete process models that's rolled up into total system performance, we should be able to go through and explain the ones where we treat it that way versus where it's just imbedded in parameter uncertainty. So, I think, we can get at that. You know, I'm not sure it will be to your satisfaction at this point, but I believe we can get at that.

What was the third part? There was ma third part, I think.

CRAIG: There's more, but you said there's going to be a meeting at some point in the office and then perhaps it can get pursued at that stage.

YOUNKER: Uh-huh.

BULLEN: Norm?

CHRISTENSEN: Jean, most of the discussion here has focused really on sort of two dimensions. I'm just curious about whether there is anything to worry about in the third dimension; that is the long drift variability.

Clearly, 1.5kw is an average value. You're taking fuel and canisters that will have a radiated different amounts. Is that a factor we should be looking at or be concerned with? Is that simply going to sort of all out in this average? And, similarly if we're dealing with issues of using spacing as a way of modifying overall temperature, does that again introduce issues that have to do with the long drift variability in the model that you've put up here?

YOUNKER: So, let me see, I think you're asking me if we were to exercise the design flexibility and move the waste packages further apart, for example, or--

CRAIG: Yeah. Or are you looking at waste that's been aged at different times or different kinds of waste, the defense waste versus, you know, other forms, the temperature profile as you move along the drift is going to vary by an order of 10, maybe.

YOUNKER: Well, the intent--let's see now. In terms of the actual thermal loading, you know, the line loading of the drift, I think in what Ric Craun will talk about, you'll see that we do have a range of thermal loadings, line loadings that we can look at and accommodate and I think in our sensitivities in PA, I'm not sure that we'll do the complete range, but we're expecting to look at some sensitivity to those in the performance assessment for SR.

You're asking like can we accommodate those into our modeling? The changes that that will cause into our representation of the processes? And, yeah, if we got the processes right, then we should be able to if we've--

CRAIG: Yeah. I guess I can understand how if you had--looking at what you have there were a Y to Z axis and how, if the temperature varies, how you could model in the Y and Z axis the boiling front and so forth, but there's also going to be this X axis.

YOUNKER: Along the drift, right.

CRAIG: And, there's going to be variation then in the performance along that axis. I just--

YOUNKER: Let me ask Jim Blink to step to the microphone to see if he can help with the answer.

BLINK: Jim Blink from the M&O. The thermal hydrological analyses that are used in the TSPA do include that third dimension in the calculation along the drift. So, we do see the variation of temperature and humidity in the drift, along the axis of the drift, and also the variation of saturation in the rock along that same axial direction. The further coupling to chemistry and mechanical properties has not yet been done in 3-D, but has been limited to 2-D, so far.

YOUNKER: Thank you, Jim.

BULLEN: Norm, any more questions?

CHRISTENSEN: That's fine.

BULLEN: Debra?

KNOPMAN: Jean, let me lay a question on the table which perhaps Ric or you might want to answer after his presentation. But, it has to do with where the default assumption or position lies on whether you--what temperature the repository should operate at. Given the uncertainties that you walked us through and I very much appreciate what you've done here this morning, what's the thinking behind kind of hanging onto an operational load that would be above boiling, as opposed to starting with a below boiling design knowing you can go above, just as you know for your current design, you could go from above boiling to below boiling? I think we're clear that there is that operational flexibility. So, that's no longer the issue. The question is where do you want to sort of set yourself going into a site recommendation? Help us think through why your default position is the above boiling design given this fairly extensive list of uncertainties that the above boiling side leads to?

YOUNKER: Well, I think, you know, our basic work over the past few years has been directed toward trying to establish what the thermally-driven uncertainties are and I think at the technical staff level within the laboratories and the M&O staff, I think we have a reasonably good confidence that we've captured those uncertainties adequately in our both process level models and represented them in performance assessment. I guess if you go back to the peer review on the total system performance assessment for VA, there were certainly questions about that, questions from your Board, as well. I think, we've recognized those and made some substantial improvements in the way we've represented the uncertainties. We do have some additional field data. So, I guess, you know, our general sense of confidence that we have accommodated those uncertainties in a way that is technically credible, it is good enough for us to give DOE, you know, the confidence to at this point in time with the flexibility that you've noted present a design that has a boiling zone no more than 1/4 pillar as a basis, at least, for the site recommendation consideration drift. But, you know, whether that's the one, I'm certainly not the one that will make the decision whether that's the one that will go forward as "reference design" for site recommendation. I think all of our work to date has been focused on making sure we have a credible documentation of the basis for that and the processes related to that design.

KNOPMAN: Okay. If I could just follow up, I mean, I guess I don't feel like you quite answered the question.

There's got to be something you're getting from the above boiling design that outweighs the reduction of uncertainty, at least, at this time that one could get by having a below boiling design. And, I assume it's because of the dryout properties that you want there. But, I mean, it's really just in the last couple of months that you've actually had field data to be able to stand by that.

YOUNKER: Yeah, I think the quantitative definition of how much benefit you get from the dryout period time when there isn't liquid water in the drift--the potential for it to come into the drift versus the impacts of the uncertainties relative to thermally-driven processes is really the bottom line. If we can adequately define that or characterize that, I think that would be the answer to your question. And, I don't know where--if Bill Boyle wants to comment, we hope to be able to do that. Bill, are you here?

BULLEN: I'm going to actually wait until after Ric's presentation to try to follow up on this because Debra laid the question on the table so we can follow up from that.

YOUNKER: Okay.

BULLEN: We have two more quick questions and then we've got to move on. Priscilla and then Jerry and then we're going to move on.

NELSON: All right. I'll make it quick. I have asked several people about the ability of PA in the models at the level of PA to discern a coherent impact on performance of temperature. Some people will say that PA cannot distinguish between low temperature and a high temperature response as it is now. And so, I wonder where the tool is that would allow the project to actually consider well what goes on with low temperature versus high temperature repository. In an integrative fashion, you've got a thermal hydrologic process here on Page 6 that is a sketch which may be rational, may be understandable, but in terms of both 2-D and three dimensional variability from the initial condition to what happens as you heat something up to run out and trying to cool it back down, there's a lot of stuff going on. That's not modeled to my knowledge in any model that the project has. I'm not saying it's easy; it's not there.

And, in #8, you've got thermally-induced shear. Well, when you heat up the rock and the rock is fairly coherent, you are going to have strains that are existing. And, here, you've got some way; you've evaluated fracture permeability increase. There is a document--I think, it's quoting Bo at some point--about how this kind of situation can produce additional fallout which will increase permeability and flow into the opening. But, yet, I see this as a stand-alone sort of analysis, sort of look and see what happens. And, how does that fit back into what's happening with performance assessment for a low versus a high temperature design?

YOUNKER: Right. Yeah, it's a valid point and I think one of the things that Bill would say if he had answered the point was that we are going to try to look at the processes where there are large thermally-driven uncertainties and look at them to some extent, not stand-alone, but to see what kinds of uncertainties we can, in fact, characterize for that given process, as well as how it is represented in performance assessment because you're probably right. When we get our results and we try to do any kind of sensitivities to either peak dose or to 10,000 year performance for a boiling versus non-boiling concept, you know, it's unlikely we're going to see significant differences--

NELSON: You're not going to do an integrative model?

YOUNKER: No.

NELSON: That is on the whole testable and understandable from its interactions. Then, it's really going to have to be really clear how you're going from all these bits and pieces into some--

YOUNKER: Very true. Very true.

NELSON: And, for me, we've already got to do it.

YOUNKER: Yeah, I think the emphasis on how the uncertainties are represented in performance assessment is going to be absolutely key. I can't agree more.

BULLEN: Jerry, last question?

COHON: This is just, in effect, a followup to what Paul Craig and Debra Knopman asked about and talked about and in some sense Priscilla's. The table in Slide 14 is very valuable and it's good to see. But, it's overdue--you're overdue--and maybe you've done this and we just don't know it--in codifying the uncertainties associated with each of these suggesting some sense of priority among them where you're just a few months perhaps from recommending the site and this is a major area that must be dealt with. Unfortunately, just to put a sharper point on Priscilla's point, how can you credibly quantify these uncertainties with a model that does not have coupled processes? I think, you've got a real issue with technical credibility.

YOUNKER: Well, there are some coupled processes, but not a fully couple THMC, if that's what you mean.

COHON: That's true.

YOUNKER: I mean, certainly, the--

COHON: No, no, no, that's right.

YOUNKER: But, I--agreed.

BULLEN: It was pointed out that I can't see through the projector. Did Jeff Wong have his hand up?

SPEAKER: He did. I saw him.

BULLEN: Jeff Wong can have the last question if he wants it. I just can't see through the projector. My x-ray vision doesn't work today. Jeff, it's all yours.

WONG: Okay. I don't ask questions very often, but of all of that menu or list of uncertainties, which one do you think is the biggest contributor to uncertainty or a contributor to your lack of understanding the system. And, Dr. Beacon (phonetic) talked about budget cuts and your budget cuts influence the breadth of your studies. Which one of those studies would suffer? And then, if your studies do suffer, what's it going to take that's going to prevent you--or what would be the consequence--or how would the consequences occur where you would start to say I can't support a site recommendation? You're faced with a budget cut, you have to make a choice amongst all of those. So, this is initial prioritization.

YOUNKER: Right.

WONG: What's going to be the critical--you're not going to give me more money to deal with the mechanical, I can't make a site recommendation or I can't support your decision or we're going to be guessing?

YOUNKER: From the standpoint of performance, I mean, I think we've said for a very long time that it's the amount of water that could eventually contact the waste that really matters. So, anything having to do with the fate of the water, whether mobilized by boiling or whether coming into the system through changes in infiltration due to natural causes will certainly always be a key parameter. So, you know, I would never want to put that at a lower priority.

But, from the standpoint--to answer the rest of your question, I would say that the answer is depending on what performance period you're most concerned about, if it's the period of 10,000 year performance in the regulatory period, then clearly the potential impact on corrosion of the drip shield and waste package is very important to us. So, I would want to make sure that I kept my focus on looking at any kind of chemical effects, anything that could potentially change our understanding of the behavior of our drip shield and waste package. But, the fundamental question of whether there will ever be transport from the system, transport of radionuclides, clearly goes up to the hydrologic uncertainty.

WONG: So, that would be your highest priority?

YOUNKER: Uh-huh.]

WONG: What would be your lowest priority?

YOUNKER: Well, I suspect I would probably put mechanical uncertainty at the lower end just because I think I can probably deal with that in a bounding approach. I think, the overall fracture permeability, I can probably put some bounds on and treat that in a way that Dr. Nelson would find was acceptable without doing an awful lot more work in that area.

WONG: Thank you.

BULLEN: Thank you, Jean. We're going to call a close to this part of it and bring on Ric Craun who has the unenviable task of being the last speaker before lunch. We do have a public comment period and I know that, Mr. Chairman. I'm going to turn the microphone over to him as soon as this session is closed. Ric is going to talk to us about the variation in operations to affect repository temperatures which is a very obvious follow-on to the previous presentation. Ric?

CRAUN: I'm Richard Craun. I'm pleased to be here and have the opportunity to discuss with you the operational flexibility of the repository design. My title is senior policy advisor. We shortened it just to fit on the slide here. So, with that, I'll go ahead and go to the next slide.

I'd like to discuss with you today the reasons for examining operational flexibility, do a quick touch on the SRCR design; discuss the considerations that we went through to come up with the parameters that we would say would be flexible from an operational perspective; look at controlling the drift temperature response with these operational parameters; go through the process of how we selected the operational parameters of which we've selected staging, waste package spacing, and ventilation duration; and then, look at some repository operating curves that take all of these parameters together and look at them all at once and some of the tradeoffs associated with that.

The program objective is to have a resilient SRCR/SR design and one might even say an LA design. And, we need that resilience to accommodate policy decisions, alternate technical objectives, and new information that may emerge between now and SR or SRCR and LA--you might want to turn back one slide--and other considerations. Now, you can go forward.

In order to start this discussion, I thought I'd take just a moment and go through this slide and the next slide which will summarize the SRCR/SR design. We have several design requirements of which I've stated two here. One is that the cladding temperatures remain below 350 degrees Centigrade and that the water is to drain between the emplacement drifts. Now, I believe, Jean talked about 50 percent of the drifts or pillar in a non-boiling condition. That's the lower level requirement to what the DOE has; basically, is that the water is to drain between the drifts.

Now, on this slide and the adjacent slide, I've started to break apart the design features from the operational features. The design features of the current design are 81m drift spacing. That would be center line to center line. We have an average waste package power output of 7.6kW. Now, this is an important parameter because there's a wide range of power outputs. If one looks at the PWR waste package, the average PWR waste package is about 11.3 plus or minus .5. So, it can be as hot or as much power as 11.8. So, there's quite a variation in the lower power waste packages to the upper power waste packages which translates into how one has to look at the emplacement drift to insure that the bulk of that drift does not go into a boiling regime, if that's so desired.

Now, we also in a lot of the analyses we did, we looked at--since this is the first cut of this analysis and a preliminary analysis, we looked at the kilowatt per meter which is simply the average waste package power divided by the approximately length of the waste package which is, one could say, 1.5kW/m or a more accurate number would be 1.42, but that's just a simple derivation of that number. We considered as a design feature the 15m^3/s ventilation rate and this really could be considered as an operational parameter, but for the purposes of this study--and I'll get into that a little bit later--we considered it as a parameter that we would not be varying. We have a drip shield in this design and we have an average 26 year old at receipt fuel. Now, that number if also very important because we use that number, age of fuel, we vary that to simulate staging. So, that's how we simulated staging in our calculations.

Now, the operational parameters that I chose to identify which are adjustable under this same design would be the .1m spacing end to end, skirt to skirt, of the waste package. The 50-year preclosure period and the 50-year preclosure period was really a goal that we had in the LAD study. It may have been a requirement. I don't know that I recall, quite sure on that, but that was a goal that we had in LADs. And then, a 0 year staging. By this, we had a receipt rate and an emplacement rate that were about the same. Now, I'll come back to that staging to describe that a little bit more fully and a little bit later on.

In a summary, kind of the results of this design and operational selection is that here we will have a peak drift wall temperature of about 200 degrees Centigrade and the evaporation fronts go in about 12 meters. Now, I think Jean had in one of her versions of her presentation 9m. She was looking at some of the emplacement drifts at the mid-point of the repository. This study is looking at the very last emplacement drift. The reason we chose that drift is because it will be the most difficult drift because it has the shortest period of time of ventilation. It will be the most difficult drift to keep below boiling.

We started out with a brainstorming session. We said now how do we accomplish this? We wanted to try to sit down and think of the different ways you could control operationally or design the parameters that would affect the temperature, the thermal response of the repository. So, in that brainstorming session, we had some very bright people and they invited me, too, to participate and identified what parameters we could change. We identified enrichment, exposure, age from discharge, thermal output of the individual assemblies, etcetera. Now, I will touch on each of these separately. So, let's go to the next slide.

If you'd like to for reference keep thumbing back to that slide because each one of these parameters now are from that slide. As we went through the parameters, we then decided we need to define or make a decision as to whether or not the parameters are available for change. Can we change them? Yucca Mountain, do we have access or control over those parameters? Are they significant parameters? Will it make any difference if I change them or not? And, are they equivalent to another parameter? If I have two parameters that are interchangeable/equivalent, then I may choose to change and not the other just really for the purposes of simplifying this first analysis that we're performing. Then, with a checkmark, we've identified those parameters that we chose to identify as operational parameters that we would try changing or varying.

So, as one can see, enrichment, we decided the program cannot change that parameter readily. Exposure, we cannot change readily. The age from discharge, the concept here--and I kind of alluded to it a little bit earlier--the concept was we wanted to separate the receipt rate from emplacement rate. The emplacement rate is to start emplacement in the repository at 2010, but we wanted to separate receipt from emplacement so that we could receive at a rate higher than emplacement so that we could take then maybe the hottest fuel, the highest power fuel, and set that aside and so that we would be building this staged fuel up, and then as we finished our receipt, we would then go ahead and empty this staged area. So, the concept was to separate receipt from emplacement where on all of the other designs that have been discussed those two parameters are locked together.

COHON: Ric, what is exposure?

CRAUN: I beg your pardon?

COHON: What is exposure?

CRAUN: Exposure is the duration that the fuel is in core, burnup.

The next three parameters that we looked at here is the number of assemblies per waste package. Now, that is a parameter that we could vary, but the waste packages, as most of you know, are fairly expensive. So, we chose not to vary that parameter. What we chose to do and we said it was an equivalent parameter is we could just space them further apart. It will drop our average power per meter down, but we recognize that there will be hot spots and so I'll come back to that. If you were to reduce the number of assemblies in a waste package, reduce its overall power, then it would have less of a tendency to have a hot spot. But, for the purposes of this study to do it on a first-order analysis, we chose to leave the number of assemblies in a waste package constant. We do not vary that and we just vary the distance between them.

Blending, we did already in the current design, base case and operations, we do take credit for blending of like assemblies. For the purposes of this study, we did not blend dissimilar assemblies, PWR to BWR. I'm not saying that that's not possible. It's just for the purposes of this study, we did not consider that. And then, we did identify distance between waste package and we identified that as a parameter that we would vary.

In going through these, in this slide, I wanted to start out and say that this is a parametric study, it's a first-order study. We've done some simplifying assumptions in our calculations. I will later on talk to you about those parameters that we know will change as we get to a more thorough analysis. So, I would classify it or categorize it as a first order parametric study and wanted to see how those parameters could be varied and affect the boiling and non-boiling of the repository. We have identified staging, increased waste package spacing, and increased ventilation duration as those parameters that we were going to adjust in this parametric study to look at the way in which we could operate the repository. We do recognize that there are hot spots. They will exist where the drift components contact the drift invert and those areas opposite the higher powered waste packages. The 11.8kW, PWR waste packages are much hotter than the 7.6kW average waste package. So, we do know there's issues there that we have not yet addressed. I'll get back to that a little bit later.

Now, I'm not sure what's in your handout. You may have the assembled final version of this chart, but what I wanted to do for the purposes of helping you read this chart is go through how we assembled it and so it will make it a little bit easier for you to look at the completed version. Distance between the waste packages is here. This is in meters, 1 through 5 meters, and the preclosure ventilation duration. Again, it's on the last emplacement drift. So, if I was talking about preclosure ventilation of 30 years, that would be after I've loaded the repository and loading the repository is about 25 years. So, this ventilation duration is post-loading of the repository. So, that would say that the initial drift was ventilated for about 55 years, approximately. That kind of helps you understand the scale.

Now, just for reference purposes only just to let you know where the current SR design, the base case design, and the base case operation, what is it, it was .1m and it was approximately 26 or 27 years of ventilation on top of the 24 or 25 years to load the repository. That meant that 50 year goal of repository closure in 50 years. So, just so that you know where this point lies. It doesn't really have anything to do with this parametric study, but it just gives you a reference point.

Let me walk over to this side for a second. Again, we had the 26 year old age of fuel, went through the entire study, and we started then putting our first line on it. What we did is we said, all right, let's not--let's zero out staging. Let's not have any staging for this first line. And, we said, now, what sort of drift spacing, ventilation duration, would be required in order to get at the 96 degrees Centigrade line? For example, at 4m spacing, it's about 50 years post-loading the repository would produce a non-boiling design. If you ventilated a little bit longer, it's further into the non-boiling design. If you ventilate a little shorter, it goes into a boiling design. So, that's what it gives you. Now, for each of the successive lines that we show for staging, to the right of that line is non-boiling. To the left of that line is above boiling.

So, we then added a series of--and these were picked kind of randomly, just made the numbers easy. We picked a series of staging lines, 5, 10, etcetera, on up to 75 down there. You'll see then, for example, if we were looking at the 3m spacing, 10 years of staging, and we'd come down to about 42 years of ventilation post-repository closure, we'd be required to make that a non-boiling repository operation.

Now, I wanted to add a couple of other lines of information. I wanted to add a 100 year preclosure period. I wanted to know at what point does my operation of the repository plus my staging, plus my ventilation post-loading, when does that reach 100 years? So, that's what this line indicates. So, for example, if I were at about 2.3 meter spacing and about 75 years postclosure ventilation, it turns out to be 100 years.

Now, I'll come back over to this side. I also wanted to add some information that was to indicate at what point do we not have enough repository footprint so that we know that at 97 metric tons that if we go with a drift spacing in excess of 4m, we will exceed the current footprint of the repository. Now, we put a footnote on there and that's with the current 200 meter overburden. If that requirement is softened, then, in fact, we would have more area and we could then raise this up so that these spacings would also be available for us.

NELSON: Can I just ask one thing?

CRAUN: Sure.

NELSON: I thought I heard you talk about postclosure. Is everything on there preclosure?

CRAUN: Everything is preclosure. I should not have stated this--the only thing that's postclosure is the point in time in which we do close. So, the 100 years would be the point where we--

NELSON: --postclosure ventilation?

CRAUN: That's right. No, no postclosure ventilation. No.

Okay. The next thing we wanted to do is we wanted to add some costs. We wanted to look at what the costs were associated with some of these and we just picked some points at random--well, not at random; we picked some points that we had some information on to look at the delta in costs between the current design and one of the latest TSLCCs. Then, I also in brackets/parenthesis, we looked at the net present value of those dollars because, as you're inducing or delaying the emplacement of some of that waste, you're going to be spreading out some of your costs. So, we wanted to look at both the delta an the total cost and then also the net present value of that delta.

Now, there's some interesting tradeoffs. One can see on here the impact of emplacing the waste and ventilating it for a long period of time versus staging it for a long period of time. Let me draw your attention to two points. It would be this point right here which is the 75 year staging at zero postclosure ventilation. So, I would say as soon as I load the last drift, I close it. So, that effectively means that all of the fuel was staged upon the surface, as compared to 75 years of ventilation at a zero year staging. Zero year staging means that all the fuel at receipt comes right to the repository and goes underground. What you'll see is the delta in drift spacing which is about 2.2 to about .4, is associated with a 70 percent efficiency in the ventilation system. This actually will put about 30 percent of the heat load into the mountain. That 30 percent of the heat load going into the mountain requires your waste package spacing to be a little bit larger. If that heat end staging is not going into the mountain, then your waste packages can be a little closer together when you emplace them underground. So, the chart, if you study it a little bit, you can get quite a bit of insight from the chart in just looking at it. But, I think that's the development of that chart.

I'm going to summarize and I'm a little over schedule, but this was an initial assessment which we feel indicates that the SRCR design and the SR design are sufficiently flexible and resilient enough to operate such that the emplacement drifts can stay below boiling. Now, we do have some refinements that we do need to make. Earlier, there was a discussion of along the axis of the drift. Right now, we took a two dimensional cross-section that cut through the emplacement drift. If this is the emplacement drift, we cut through it. We looked horizontally and vertically. We did not look down the drift. As you look in the three dimensional term down the drift along the axis of the drift, you will start then looking at the variation in waste package power from the average up to the peak to the low. And, it's very important that we look at that and see how that affects these curves. It will shift them. It's not clear to me that they'll shift a lot, but they will shift. There some other pieces that will probably pull that shift back unless the heat transfer--we obviously ignored the heat transfer down the emplacement drifts. So, doing that two dimensional analysis in the first cut simplified analysis, there's some things that will push it to the right and there are also some parameters that will push it to the left.

We simulated, that last bullet there, the staging by just looking at the average waste package power for 26 years and we then aged it. For example, if we had a 10 year staging, we had it all at a 36 year old fuel. So, that's how we did that. It was a fairly accurate, fairly simplified process, but in reality, we need to recognize that we're going to have some younger fuel and some older fuel and we have to work that in. It won't change it that much in my mind, but it is a parameter that needs to be addressed.

I'll open it up for questions?

BULLEN: Thank you, Ric. I'm going to hold the line on 15 minutes worth of questioning so that we have 25 minutes for public comment and we'll be done at 1:00 o'clock. Is that okay with our chairman?

COHON: Yeah, it's all right.

BULLEN: Okay. Actually, let me see the hands of the questions again? We'll start with Alberto, Jerry, Paul.

SAGÜÉS: Yeah, going back to 11, when you just showed the very first graph, can you do that? The very first line, the line of zero. Okay, great. So, based on the uncertainties that you have right now about this step of analysis on the viability, how much would you expect the line to move, say, left to right? Would you expect for it to go, say, where the little zero is for that particular case--could a thing go all the way up to, for example, say 100 years or 150 years or is the uncertainty of that quite small, maybe 10 years to the left, 10 years to the right?

CRAUN: Well, let me answer by saying my first concern was associated with the fact we were using an average waste package power of 7.6, recognizing that we've got an 11.8kW waste package coming in which is a substantial percentage change. In what we've been looking at, so far, I don't expect this to move that much, maybe

20 percent, maybe a little bit less, maybe a little bit more. We have not done the calculations. We have not done them. So, we have to go through that three dimensional analysis. We did not consider the heat transfer down the axis of the drift. So, that will help pull that back to the left. We do have other things we can look more seriously at different blending scenarios to also help us pull that curve to the left or to the right. But, I would expect it to move, I would expect it to change, but I'm a little soft on how much. We just simply haven't done the numbers, the analysis.

SAGÜÉS: But, not twice as much to the right?

CRAUN: I wouldn't expect it to double, no. No.

SAGÜÉS: All right. Thank you.

CRAUN: No.

BULLEN: Jerry Cohon?

COHON: I'd like to go to the figure with everything on it. Let me say, first of all, Ric, I found this very useful. I think it's a really great exercise. No doubt it could be extended to other combinations of design parameters. You may have said this and I was distracted for a minute and I might have missed it. If I did, I apologize. But, with regard to the cost increases, I infer from the information shown that 10 years of staging would add about $1 billion in current costs that is not net value?

CRAUN: That's about right.

COHON: About a billion. And, is it very sensitive to the number of years of staging, the additional cost increase?

CRAUN: Well, the net present value for 75 years would be much lower, right.

COHON: But, let's just talk about current costs. That is not discounted costs?

CRAUN: Current costs?

COHON: Would that go up much with years of staging?

CRAUN: I would think it would reach a threshold somewhere in here where we would have then difficulty--

COHON: Because of the amount, yeah.

CRAUN: Yeah, where it actually may start dropping down. Well, let's see, that would be discounted. Things are going to start getting--in the 25 to 30 year period, they're going to get a little gray for me because the analysis is based on staging and based on age of fuel. There's a point where if I have too much staging, I can't get--I'm going to have trouble getting down to that decay curve. So, there's some issues there that are associated with that where in this area it would--I guess, I get awfully soft on how those numbers might change. They might start actually going up.

COHON: Well, just, say, give me an idea? Would it be something like 2 billion instead of 1 billion or 10 billion, say?

CRAUN: On the net present value, it looks like most of the numbers are between a half a billion and maybe 2 billion net present value.

COHON: Thank you.

BULLEN: Paul Craig?

CRAIG: I'm going to follow this same line of reasoning because I think this is one of the most interesting graphs we've seen and I think it's real important to carry it the rest of the way or, at least, somewhat further. You said that staging means you can receive waste at a rate higher than you can emplace it. If I'm going to delay for 75 years to take that point at the bottom corner before emplacement, I don't have to drill any drifts, I don't have to manufacture canisters, I don't have to manufacture drip shields. I've done a huge amount of saving. At some point, your numbers--your net present value numbers have to turn around. There has to be a negative number.

CRAUN: I would agree with you.

CRAIG: All right. And, you don't have any negative numbers on your chart. So, I say, gosh, a major feature of your analysis or a major result that should be drawn from your analysis simply hasn't been analyzed and it needs to be. So, there are a bunch of savings which have apparently not been included of things that you don't have to do now because you've got all the staging. What does that mean? I think it would be really good if you'd carry out the rest of the analysis.

CRAUN: Well, I think at this point, this curve really represents a different approach to geologic disposal.

CRAIG: Well, that may very well be. You can say 50 years of staging amounts to surface storage if you want to. There's no question that you can change the language. But, you started a line of reasoning here and it's an important line of reasoning with respect to the management of the repository. And, I'm going to argue that even though the DOE management may not think that's an important line to explore, there's a bunch of public out there that think that's a really important line to explore.

CRAUN: I'm not one to say it is or isn't important. We can do the calculations fairly easily.

CRAIG: Yeah. Yeah, I hope you will.

BULLEN: Ric, just a couple of quick questions here. If we could go to Figure 5, please? That last drift loaded appears to be a real challenge with postclosure wall temperatures going up to about 200 degrees C and the evaporation front advancing for 12 meters. Is there a reason that the last cans have to go in one drift? Why don't you put--I did a little math and said if it's 1,000 meters long and they're 5m cans, there's 200 cans, I've got 100 drifts, why don't I just put one at the end of each of the drifts all the way around and then I don't have that last drift? Of course, conversely then, you could load the entire repository in a spiral or however you want to do it, but have you looked at other than linear thinking associated with the loading options?

CRAUN: Well, let me answer yes and no. For the purpose of this first study, no. No. In reality though, let me try to take your concept and take it a little different direction. For example, we assumed 81m spacing between the emplacement drifts across the entire repository. One might want to vary that so that the initial drifts loaded might be actually a little bit closer and the final drifts loaded might be a little bit further apart. I think those sorts of operational parameters--those are parameters--need to be explored. But, for the purpose of this first cut parametric study to see what ball park we're in, what those series of curves could look like or might look like, in this case, no, we did not vary that.

BULLEN: Then, if you go to Slide 8, it's another follwup where you're essentially fixing a number of assemblies per waste package. For the purpose of this study, if you really had problems with how a waste package is at the end, you could always derate them or underload them?

CRAUN: That's right.

BULLEN: Okay.

CRAUN: For the purpose of this study, we felt that this really from a kW/m perspective, that parameter allowed us to look at what we were wanting to look at, but yet you could do it, either waste package spacing or the number of assemblies per waste package.

BULLEN: Okay. And then my final question is on Slide 11, full blown with all the numbers on it, if we can get to that one. When you put all these numbers in, you have a plus $6 billion in today's dollars, 1999 or 2000. How does that compare to the total projected cost of the repository? What's the total cost?

CRAUN: The total is about 48.

BULLEN: So, it's about 15 percent or so increment one way or the other?

CRAUN: 10 to 15.

BULLEN: 10 to 15, okay.

And then, Debra wants her to place her question back on the table. So, I'll defer to Debra for the last question.

KNOPMAN: Actually, I'd like to just very quickly as a clarification. For calculating these curves, you make assumptions about thermal conductivity of the rock and were you using numbers associated with the lower lithophysal zone or--

CRAUN: Actually, all the different units were used. The calculations are done so that the number of emplacement drifts at the different units, the different structure. We use the values there. So, all of them.

KNOPMAN: All right. And then, I'll just see if you want to take a crack at the question I put to Jean. This figure which I like very much because it does begin to show in a very clear way tradeoffs that are involved in operational modes and really your policy--in some ways, policy decisions. It's quite illuminating. Given though what this shows, it shows it's not hard to get to a below boiling design. It's easy. It's just what else you may want to give up in the process. I'm not saying there aren't--you're not giving up something there. So, it's not a problem.

Could you give your thought in just two minutes of why it's still attractive to use a reference design that's above boiling?

CRAUN: This is a career opportunity.

BULLEN: For the record, Ric, you have four minutes. So, go right ahead.

CRAUN: I appreciate that.

Well, I think Dr. Itkin had a sentence in his presentation that I want to kind of read. I thought I might get this question. So, I wrote it down. He stated that the design flexibility permits us to refine the operational parameters of the repository as we gain a greater understanding of the uncertainties associated with the thermal loading. I think it's important from my perspective to do these studies, to look at what we need to do with he repository design and operational modes so that we have that flexibility. This was a first cut of the analysis that needs to go forward. It needs to mature. It needs to be taken to the next step. Might we change our approach in the future, we might. At this point in time, it seems early to me based on what we've seen here. This is really of a great deal of interest. It shows a lot of potential for us to be able to make some changes in the future. It tells us what impacts those would have on us and what that might cost for the program to make those decisions. I think from my perspective, it's important to have that flexibility.

As to how I proceed into SR or LA, I think those decisions will come with time as we work the uncertainties. I think, Jean's presentation tried to touch on the uncertainties to try to get an agreement on what are the uncertainties, how do we need to approach those uncertainties, how do we need to resolve them if they're resolvable, and that approach, we need to follow that approach and go down that. Might that lead us to a non-boiling design or we may find out that above boiling design is better. Currently, I think a lot of people on the program feel that the above boiling design pushes the water away, it's better. It's better. Might we find that that is not the case and we need to go with a below boiling? Yes, we might and this would give us the flexibility to operate the repository in that manner. I'm out of time, I hope.

KNOPMAN: I was just going to say I think what it suggests is there's another set of tradeoff curves, many sets of tradeoff curves we want to see, I hope, at a later Board meeting that really starts showing what you gain or lose in terms of uncertainty under these different operating modes.

CRAUN: I agree.

KNOPMAN: That's the big missing piece in this discussion and once there's more clarity there, then you can make the tradeoff.

CRAUN: That's right.

KNOPMAN: Then, you can justify the tradeoff. We really can't do it one way or another right now.

CRAUN: I think those uncertainties should help us make this decision.

BULLEN: Priscilla, would you like the last word?

NELSON: Well, you just tricked me with that "a lot of people no the project feel that this is"--and, you know, I guess I don't mind people feeling that way, but I would really like to understand coherently, you know, what's going on with temperature in terms of tradeoffs and uncertainty and to have that happen over the next period of time, a year or two before SR. I think it's possible to understand what's very good and what's less good for each of those. I think you can get there and be coherent.

Let me just ask you one thing about this. Did you do a weighted average of the thermal properties or-- because there's no way to otherwise include this here. Where did the 81m come from?

MCKENZIE: As far as general conductivities go, the thermal models have sort of a layer cake in them where all the different units are represented and their thermal conductivities, as we know them now, are represented. The drift, itself, is in the lower lith because that's the one that has the lowest thermal conductivity, so that makes it conservative. It's also the drift that happens to have about three-quarters of the repository in it, in the lower lith, so that's why we use that one.

81 meters, nobody is going to tell you that it couldn't be 85 or 75, but 81 meters was a number that was large enough that we were pretty sure, coupled with the ventilation, that we wouldn't get coalescence of the boiling point. So what that leads you to think is that, okay, there might be a different drift spacing that might be optimum for a below boiling repository.

BULLEN: Thank you very much, and we're going to call the morning technical session to a close. I'd like to express the Board's appreciation to all the speakers. They did a great job. And I turn the podium back over to our chairman, Jared Cohon.

COHON: Thank you, Dr. Bullen, for that excellent job of chairing. We turn now to the public comment period. I'm a full service chairman. Four people signed up to comment. I just want to confirm that those are the four. Ron Rockwell, Sally Devlin, Kalynda Tilges--we'll see if I have pronounced it right--and Grant Hedlow.

Is there anybody else that wanted to comment during this public comment period?

(No response.)

COHON: Seeing no other hands, I'll call on now Ron Rockwell, who is a scientist with Rockwell Scientific Research. Mr. Rockwell? If you want to use the podium, you can come up here.

ROCKWELL: Jerry said to keep this down to nine minutes, 18 second, because it's lunch time.

My name is Ron Rockwell, scientist and master machinist for Rockwell Scientific Research. I was sent information on this meeting just a few days ago, and I worked with the Rife Laboratories since 1964 in the Crane Laboratory. And in that laboratory, they had a lot of interesting prototypes and working prototypes. Well, I worked with some of the work that he has very well known and documented in the Smithsonian Institution Report of 1944, Report Number 3781, by Dr. R. E. Sidell, and it's call the New Microscope, but that was one of his several projects.

The working prototypes that were in that laboratory got my interests, and great interests, so after John Crane passed away in 1995, I proceeded to redevelop this work, and I took one of these prototypes that had my interest to several professors well known around the world. And he has also served as consultant in underground nuclear weapons tests with the EG&E, Physics Division, including energy measurements and interactions.

I continued to work with him, and he looked this prototype over and we continued more further work on it. He has also worked with national laboratories, Brookhaven Q clearance, Lawrence Livermore Q clearance, Los Alamos Q clearance, U. S. Berkeley Radiation Laboratory Q clearance, DOD secret and Q, EG&G secret and Q, Test Site Nevada Q clearance. He renamed this device that sat in that laboratory for 45 years as a radioactive neutron accelerator.

We have tested it several times on small low-level, and there has been a great success in it, but he said we need to take this and use U233, enrich U235, and enrich U238, and test it. My corporation is very well sound financially. There is no money needed from the government. I believe along with these professors who would attend the tests, this needs to go to Area 25 for a test. Just imagine if this really worked. If jerry can set this up for a test, we'll do it.

COHON: Mr. Rockwell told me about this in advance, and I told him the Board was fresh out of U233, but that I was sure there would be people in this room who would know where to get some if they thought this would be something that they'd like to pursue. And you see who he is and he'd be happy to talk with you.

I call now on Sally Devlin. You want to come up here, too, huh? You like this. Okay.

DEVLIN: Can everybody hear me? I'm Sally Devlin, and I live here in Pahrump, Nye County, Nevada, and I want to welcome everyone of you. We're together many times during the year, because I attend all the meetings of everything, but the most important thing was that you came back here, even if it was three years. So a hearty welcome.

And a hearty welcome especially to our Swedish friends. They enlightened me to a new acronym, because I've been known to yell at 21 acronyms, and that one was DAD, decide, announce and defend. Well, that's a very male sort of thing, a DAD, in this country, and we women are considered panty waists. I think most men think of us as wasted panties, but I really do feel that you enlightened us. And, of course, we're going to enlighten you, because of my next presentation. I have done this before, but I've done it formally now.

And I want to personally thank Dr. Bullen, who is my mentor, who introduced me to a world I never knew existed. And the core problem to me that we face from the Yucca Mountain and Nevada Test Site projects besides economic ruin is complete lack of any medical facility in Nye County and the impacted counties. We requested from the Yucca Mountain project and Bechtel, the Nevada Test

Site for $50 million each for research, medical research and a training facility. Both of you are on the same 1375 square miles. Everyone is aware how radiologically dangerous the entire test site is, and the radionuclides will continue to spread. Mr. Rockwell just go up there and take a handful.

We must compare the Yucca Mountain project interim storage perhaps and repository project with a NASA project. NASA, under Dr. Golden's direction, has the commitment to the human race, and he just received $16 billion for their project through 2005. Their goal is to accomplish peaceful economics and scientific goals. A three year contract was awarded to Mt. Sinai Hospital in New York. All people would benefit from their studies affecting astronauts.

We hope that this one subject alone will lead to medical breakthroughs that will benefit all mankind. NASA's space program has accomplished many successes, as well as major failures, but their stated goal is to perform all the research possible to benefit the entire world. We will repeat their goals; to accomplish peaceful economics as well as scientific gain? The diminishing appreciation, respect and reverence for human life, especially before human generation, as well as the 43 states, is totally ignored by DOE, Yucca Mountain and the

Test Site.

The Yucca Mountain project projected for two repositories, and I say this at every meeting, not one but two, it's in all of your reports. That's 148,000 metric tons. And these two repositories will be filled with all the highly radioactive material that the DOE deems waste, and we all know that. All four states involved will be ruined, especially Nevada. How can we who live in the shadow of Yucca Mountain and the Test Site force you to consider the possible health risks in all states from radioactive waste. We need full disclosure. The only way we can get it is to get the scientific and technological information, is if there is a medical research and training facility here.

We all know that the money you are currently spending could be used by the scientific community to make the problem of radioactive waste disappear, and that's what we're for. A research and training hospital here is absolutely needed immediately. And the one word I leave out, because I have just learned it in the last few years, is virtual, and I'm talking about I want a virtual hospital like they have, the system in Iowa. I want the same wiring that you have at Summerlin that can run the world. I want, and again it is not for the DADs, but it is for the future generations.

Thank you, Mr. Chairman.

COHON: Thank you very much, Ms. Devlin.

DEVLIN: I want to form a committee now.

COHON: I think Dr. Bullen will chair it; right?

Kalynda Tilges? Please restate your name.

TILGES: Tilges.

COHON: Tilges, okay. Do you want to do it up here? There's a microphone right here.

TILGES: Good afternoon. I'll try and make this short. My name is Kalynda Tilges. I represent Citizen Alert. We're an environmental group based here in Nevada, both in Las Vegas and in Reno. I have some comments and I have a few questions.

First of all, I have to say that Dr. Itkin's statement about Yucca Mountain being a working laboratory is disturbing at best. I don't imagine there is anyone living in any state who would enjoy themselves and their children being guinea pigs for the most fantastical experiment the world has ever known with such dire possible consequences being involved. That bothers me very much. But I also--I have to say that at least he sees that, but I hope that the Board would also take that into consideration.

I very much appreciate Mayor Carlsson's presentation. I think it was very interesting to find the way that Sweden is handling their waste, and I think that also their public opinions and the politics involved, I think we could learn a lot from that. Thank you.

Questions I have, first of all, I didn't understand the answer to how the design changes would impact the EIS. The answer is clear to me as the question to begin with. It wasn't clear at all. I don't feel the question was answered properly, and I don't know if I can just stand here and ask questions, or if I can actually get an answer to that.

COHON: You certainly can. Would someone like to respond to that? This is a question with regard to how the design changes will be reflected in the final EIS.

TILGES: I'll take anyone's answer as long as it's clear.

DYER: This is Russ Dyer, the Project Manager at Yucca Mountain. The EIS doesn't have the level of detail and design in it that some of the things that you saw here today. And the idea of the EIS, as design detail evolve over time, is to try to provide a bounding analysis of what the impacts of whatever repository design would ultimately be used, try to bound that and see if that impact on the environment is acceptable or unacceptable.

There are some things, that as we go through the evolution of design, those features need to be picked up and accommodated in the final EIS. There are other things that are so far down in the level of detail that you probably won't ever see those explicitly mentioned in the EIS. So it's going to be a mixture of both. I mean, the final EIS must capture and bound the repository performance.

COHON: So to the extent that the design changes influence what you must print in the EIS, it will be reflected in the EIS?

DYER: That's true.

COHON: Thank's, Russ. Ms. Tilges, just before I go on, just I don't give you a false impression, they're not obligated to respond to your questions, but we've found that they're always willing to do so. So you keep firing away, and we'll see if they respond.

TILGES: Thank you. On the welds and the laser peening, I believe it was, I still, maybe I don't understand technical language well enough, but I still also don't understand how you can decide that a weld will hold for 10,000 years. That's actually supposed to be a question, if anyone would like to answer that.

COHON: You may set a precedent here. They may choose not to answer that one. We'll see.

Does anybody care to talk about how you can predict--here we come, someone is coming. This is a day filled with career opportunities.

GORDON: Yes, my name is Jerry Gordon. With respect to the laser peening, that's a process to reduce the stresses in the weld. It doesn't directly affect the weld, and the process is mitigation for stress corrosion cracking.

COHON: So by doing laser peening, the intention is to increase the life of the weld; is that a fair statement?

GORDON: It's to avoid a potential corrosion mechanism, stress corrosion cracking, by eliminating the stress, which is a necessary condition.

TILGES: How do you decide that that will last for 10,000 years? I understand what it's supposed to do, but I don't understand how you can come up with the idea that it will work for that amount of time. There's no data to back that up that I could see.

GORDON: The laser peening process per se won't last for 10,000 years. It's coupled with another process on the other lid, and the combination of the two processes, based on corrosion rates, will last for 10,000 years, or more.

COHON: Let me just say you've touched on a question that the Board has dealt with at great length and at many meetings with the DOE and its contractors. That is a central question. No one can know that anything is going to last for 10,000 years. But the best they can do is make predictions, and we look very carefully at the basis for those predictions. Keep coming to these meetings. You'll learn a lot about that.

TILGES: I plan on it. I plan to be a permanent fixture.

COHON: Good.

TILGES: I'd also like to ask where I can get copies of the designs for this world's largest dust pan? And is there also a design in process for the whisk broom to go with it? Do they have an answer for that one as well?

COHON: Yeah, here he comes. Look, they're fighting for the microphone.

HARRINGTON: I'm Paul Harrington, DOE. We have in past presentations to the Board had sample pictures of concepts for those sorts of things. They exist conceptually now. If we can get with you with our Public Affairs folks, we can get that sort of information given to you. I'm trying to think of other published documents that that's in, and there isn't that I can think of offhand.

TILGES: I guess basically the last thing I wanted to ask was of the Board. How will the public comments, or what does the Board do with the public comments? Do our comments affect the Board, and how so?

COHON: Let me take that on, unless someone else--do you want to fight me for that?

I guess the first thing that needs to be said is that the role of the Nuclear Waste Technical Review Board, as I indicated in the opening remarks, is to advise the Secretary and Congress on the technical aspects of what DOE does, sort of basically a reactive and responsive agency.

The public comments of the sort you just gave us, the questions that you just asked, are valuable to us, the Board, because it, on occasion, reveals issues that we may not have thought of, or it might bring more clarity to them.

Another purpose of the public comment periods that we have here, though, are to provide exactly the kind of dialogue that's happening right now, to give the public an opportunity to question DOE, as well as the Board, about matters related to this project.

Everything that is spoken is recorded. Scott over there with the head phones on is doing that. In addition, all public comments you give us are also--I mean written comments are also included in the record of this meeting. So that's how it factors into what the Board does.

What I have to emphasize, though, is the technical nature of our Board. So, for example, questions like should there be medical facilities of the sort that Mrs. Devlin was talking about, that really is outside the Board's purview, and we will not comment or do anything with that comment, but DOE heard her.

TILGES: Thank you.

COHON: Thank you. Please come back.

TILGES: I'm done.

COHON: Did you finish? Okay.

TILGES: For now.

COHON: Okay. Grant Hedlow.

HEDLOW: I have some questions that for the last five years anyway DOE, NRC, NWTRB, and so forth, have not been able to answer. So if somebody wants to volunteer now, they've got a real chance to be a hero.

The containment in the cask, there's some metallurgy that's commonly used in the chemical industry that will contain the material at 360 degrees C, or quite a bit higher. The tests so far started in 1955, and by 1975, there was absolutely no damages, no corrosion, nothing. I haven't kept up for the last 25 years whether that's still going on or not. So that's one solution to your problem.

The Swedish engineers came up with another solution. I don't know whether you noticed or not in their presentation, their casks are only 210 degrees, and at 210 degrees, almost anything will contain it. It's no problem at all as far as the corrosion is concerned.

But one of the keys to that was that they had to have it in a swimming pool for 40 years. I think a great deal of ours will be in a swimming pool for 40 years anyway.

The other solution is one approved by the NRC, and DOE had a hand in it, they used Sandia as the M&O. What they did was they used six inches thick stainless steel, and they got caught with it splitting open after five years. This is after guaranteeing that it's going to last for 10,000 or whatever the number was. And I told them probably six, seven, eight years ago that stainless steel would not hold that material for that length of time.

The surprise to me was that it didn't split open in six months. But we don't know how long it lasted, because they got caught with it splitting open. They added some acid to it for some reason or another, which generated hydrogen, and then they hit it with the welding equipment, and it blew up. So that caught them.

That doesn't give me too much confidence that people are watching the store. Not only the NRC, the DOE, but the NWTRB, cannot find the technology that's used every day in the chemical industry to contain this kind of material.

The other thing that I wanted to mention that I think has not been mentioned at all except for Rockwell, the transmutation of this waste will generate a trillion dollars worth of electricity. Livermore took a shot at it in the 1960s. They actually discovered it. Los Alamos took a shot at it in 1980, and Los Alamos now is looking at it again.

I'd like to ask you how many businesses you think Livermore and Los Alamos and other scientists started, and occasionally somebody starts a business after they learn some business procedures. You stay as a scientist in a lab; you don't start businesses.

That's all I have. I guess it's time for lunch, huh?

COHON: Almost. Mr. Rockwell has one quick question.

ROCKWELL: This is directed to the Board, and I hope it gets to the NRC. If you go east of Flagstaff, Arizona probably about 15 miles, there's a crater out there in the old 66 one mile in diameter. If you go up in Canada, there's one that's 64 miles in diameter. Has the NRC ever thought what happens if--this is a gambling state--what happens if one hits the test site, hits that Area 25?

These welded containers are not going to hold together.

COHON: Yeah, I don't know if anybody has studied that. The good news is if something like that hit the earth, you wouldn't care about the nuclear waste anyhow. The earth would be obliterated.

Those kinds of extreme events are very much part of the studying that DOE is doing and NRC is paying a lot of attention to that. Whether they've looked specifically at astroid or meteorite hits, I don't know about that, but the question is now on the record, thanks to you, Mr. Rockwell.

My thanks again to all of our speakers, as well as our public commenters this morning. We are adjourned until 2 o'clock.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

KNOPMAN: I want to welcome everyone back.

Our focus this afternoon is on ongoing scientific studies at Yucca Mountain. We're going to have four presentations.

Abe Van Luik is going to talk about what he's characterized as open issues in performance assessment. He'll explain what he means by that.

Mark Peters is going to be giving us an overview of the ongoing studies, I believe focused primarily on the cross-drift studies.

Don Shettel from Nye County is going to talk about some geochemical studies the county is running, as well as other hydrogeologic investigations.

And Bill Boyle and Marc Caffee will be talking about the chlorine-36 validation studies.

We anticipate extensive questions and discussion throughout the afternoon, so I think we'll go directly to the program.

Just by way of quick introduction for Abe, Abe is a senior policy advisor for performance assessment, and he is with DOE.

VAN LUIK: I hate wearing a tie, but this one reminds me there are some parts of the deserts that have flowers right now. If you go from Searchlight, Nevada to Nipton, California, there is on the up slope on the west facing slope--no, that would be the east facing slope, there is a very nice display of Indian Paint Brush, and a bunch of other purple and yellow flowers, and it's one of the few places where I've found any this year.

Senior policy advisor means, you know, the abbreviation is PAPA, which is papa, senior papa means grandpa, I guess, but I'm here to decide, announce and defend.

I was asked to talk about calculational time frames and the status of TSPA-SR, and what I wanted to do is talk about a decision we had made about the time frames, and I will announce that and defend it to anyone who wants to challenge it. And that goes for undisturbed performance, disturbed performance and human intrusion. There was a decision made. We implemented it, you know, thoughtfully, and the peak dose analysis.

And then the status, PMR and AMR schedule, inputs, system performance modeling, sensitivity and uncertainty studies and summary, and this will be a relatively quick talk.

We made a decision, we meaning not me, Project Operations Review Board, the people that are empowered to make decisions, made a decision 16 February 2000, which is in our decision database. And the decision was what is going to be the content of SRCR Volume 1 and Volume 2.

Volume 1 is to include a complete summary of the

TSPA-SR. Now, that means it's to include calculations beyond 10,000 years to provide insights into the robustness of the repository system. It's also to include peak dose evaluations. That is the decision that was made.

Volume 2, however, is going to be our trial of a regulatory compliance argument. We require showing compliance with 963, which in turn calls on 63 and 197. So SR, Volume 2, the suitability part of the SRCR, is going to be a 10,000 year compliance demonstration. That's the way it breaks out.

The SR's undisturbed performance. We are looking basically at 10,000 years for the compliance case. But to give us added assurance, we will look for the undisturbed performance case to 100,000 years in all of our calculations. Now, undisturbed includes climate changes, thermal effects and design basis seismic events.

These longer term calculations provide additional assurance of robustness for the 10,000 year compliance calculation. And also we need to illustrate the role of all the processes in our models, and if the first 10,000 years, the engineered system hasn't really broken down, then we need to go beyond that time to get some of the natural system into play. So this supports the demonstration of meeting the multiple barriers requirements in 10 CRF 63.

For disturbed performance, we're going to do something just a touch different. Volcanism direct and indirect effects we will calculate to 20,000 years to put the 10,000 year results into a wider context. And human intrusion is to be addressed for the SRCR, not for the SR when we have final rules. But we will assume once that the event occurs at 100 years as the NRC wants us to do in 10 CRF 63, and then we will also do it having the event occur at 10,000 years, which is more in keeping with the 40 CRF 197 draft that we have seen.

Actually, they say if you can make the case that it's beyond 10,000 years, that it's likely that current technology would actually penetrate a waste package, if it's beyond 10,000 years, then you take that calculation into the EIS and don't treat it as part of the regulatory requirement.

It will be treated separately as a stylized analysis, which is a point of agreement between the two draft regulations. We only disagree on when it should be done. And we will do these two analyses also to 20,000 years. Because once the event has happened, basically after that, you're just bean counting.

Principles governing the peak dose calculation for the EIS. Well, this is for the EIS. It's not a licensing document addressing a requirement in a regulation. So NEPA requirements usually say best available information, best estimate calculations, and it discourages speculation. So we would like to provide a realistic, meaning non-pessimistic, system performance calculation from closure to one million years post-closure for the undisturbed system.

Volcanic events, if they happen at all, are more serious earlier in repository life than they are later. So we think that the 20,000 year analysis for volcanism will do, because that will capture the peak potential consequences.

Peak dose. What do we make of peak dose? We have this topic under discussion right now, and some people have been assigned to look at all the aspects that are part of the peak dose and what it may mean. And the idea is that these discussions will lead to a policy statement, a core position, so to speak, that will be published and part of the record.

We, DOE, we're a participant in creating an international statement of principles that includes this topic in the Environmental and Ethical Basis of Geologic Disposal, something done by the Radioactive Waste Management Committee of the Nuclear Energy Agency back in '95. And we interpret that document to say that a repository should not present public health risks unacceptable to current generations.

This translates to a small fraction of natural background in terms of potential added dose. However, resources should not be spent by a society to minimize small potential risks in a very distant future when those same resources could be used to address present more meaningful risks.

So, in other words, there is a balancing act to be played here, and this recognizes that repositories are not decisions made by any one entity, but these are societal decisions because of the implications that they have in the long term.

What is the status? Pretty good, actually. Integrated site model was accepted 2/16/00. That was a busy day. Unsaturated zone flow and transport has just recently been accepted with conditions, and the M&O is working on incorporating DOE's comments.

All of the others, except the last one, is undergoing DOE acceptance review. In fact, I just received this one this morning, so we guessed right that it would be in before this meeting. And disruptive events is coming in on schedule in a couple of weeks.

So we feel that we're in pretty good shape. These PMRs provide the basis for TSPA. And so the quality of these documents here reflects directly on the quality of the total system performance assessment.

Analysis and model reports are the next lower tier of documents that support the PMRs. Out of the 121 AMRs scheduled, 97 are completed, and these reflect the design with backfill. Of these 121, all but three have completed checking. 27 of these are currently being updated to reflect the removal of backfill. Most of these changes are not significant, but as you can understand also, the TSPA has to await the full incorporation of the no-backfill case and its supporting calculations.

Status of TSPA-SR. Model development has been delayed due to late feeds from the process models, the late design changes, and frankly, we had a little bit of problem with GoldSim. It needed a lot of debugging because of the demands that we were making on that code.

We feel that because of this cooperative development between DOE and the vendor for GoldSim, Golder, that we now have a very good tool.

The TSPA-SR model without backfill requires modified thermo-hydrology and indirect volcanic effects to be re-evaluated basically. They were done once. They have to be redone.

The TSPA-SR model itself has undergone testing and is in review by AMR suppliers. Now, the analysis and model report, PIs that do the supporting calculations that feed the PMRs and the TSPA, need to see the TSPA, how it uses that information, and what the output and the results are. We find that that is a very important part of the checking, because we never want to be in a position of having the scientists and the engineers say PA must have made that up because I don't recognize this. You know, their nose is being put into the document saying this is what you gave us, this is how we used it, this is the outcome. What do you think? So that's part of the checking process.

Rev 00A, the very first documentation is expected to be completed in May with a punchlist of remaining items, including identified sensitivity analyses.

Feeds to SRCR are being delivered in advance of result finalization. In other words, as soon as results come in from TSPA, we give them to the people doing the SRCR writing with the proviso that if checking discovers an error and the calculation is rerun, they run a little bit of risk. But the way things are working, we can't do everything in sequence.

Rev 00 documentation is expected to be completed on time, August 31st, as per the schedule. And a range of possible uncertainty, sensitivity and barrier importance analyses, methods and approaches and have been defined.

There's a big long list that we've developed, and it will be a real challenge to get all those done.

So, in summary, decisions have been made with respect to calculational time frames. I think you have the answer. We made that decision in our decision-making process and actually reported it. A potential policy regarding peak dose and what it means to DOE is being discussed.

Backfill inputs are now in place. The model is running, although continued testing, verification and documentation are under way. TSPA is catching up to its original schedule, but many activities are being conducted in parallel, which makes it require more checking. You find an error in one, you've got to go back two or three places instead of just one.

Sensitivity and barrier importance analysis are required to address 10 CRF 963 criteria, and they have been identified and we have a list of those. That long list of criteria, each one of these needs sensitivity and importance analysis, and of course the Board's comments on all of these issues are welcome.

Some of the other issues discussed this morning, I didn't think that in this talk you wanted to get into, such as the confidence that we have in the model. I like TSPA-VA myself. I thought that was a good product. And we have taken a lot of the comments that we've gotten from the Board and from the peer review and addressed them head on with either extra work, extra sensitivity analyses, and I think many of us will be very pleased with TSPA-SR, although as soon as you see it, you may like it, but I'm sure that, you know, your job is to find where the weaknesses are and help us zero in on them to move forward.

It's been a very difficult process getting all of this material to come together at the right time and the right place. We have been running late up to this point, but we're very rapidly, now that everything is working, catching up to the original schedule.

KNOPMAN: Thank you, Abe. Questions from the Board? Jerry?

COHON: Abe, I have several specific questions that I think are short answer type questions. Who are the members of the PORB, that decision-making body?

VAN LUIK: Don Horton is the chief of the PORB. I know that I think it's the deputies--it's the assistant managers to the project manager, that is the board.

COHON: You indicated that for the EIS, with regard to the period for calculation, six years would be used. Why in the EIS and not in anything else? What's the argument?

VAN LUIK: The TSPA-SR document will address the million year calculation. It is being done primarily because it's required by 40 CRF 197. But the decision that I read was that it will also be reported in SRCR Volume I, because the TSPA-SR will be the basis for both documents now that they're coming out at about the same time. And we've always shown it in the past.

COHON: Okay. Could you put up Slide 7.

VAN LUIK: Seven?

COHON: Yeah.

VAN LUIK: Okay.

COHON: This seems a small thing, but I want to pursue it anyhow. This last point, that resources should not be spent by a society when those resources could be used to address present, more meaningful risks. Some would argue from the context of public choice theory that the word should be will be used. That is, public projects have been justified in the past when there has been a hypothetical claim that one can claim benefits for this project, because if you don't build this project, then something else might happen. And that's been attacked because you can justify almost anything by creating some hypothetical other event or project if you don't do this one.

So, thus, the word, I would argue for the word will instead of could. I know you like philosophical problems, so I thought I would raise this.

VAN LUIK: Yeah, of course the problem here is, and this is a problem I have with the NEA statement, this is a collective opinion type statement, is that it is assuming that the society that decides to not reduce this risk by this much and, instead, spend societal funds somewhere else, that it actually works that way. But when you have dedicated pools of money and you have assumptions about governments very far into the future, all of these things become a little bit murky and it's hard, I mean, to say will when you're talking into the far future is--or even to say should--

COHON: Or maybe probably would.

VAN LUIK: Probably would, yeah.

COHON: Of course then we'll insist that you quantify the probability of it.

VAN LUIK: Yeah. I think the reason they said could is because society could decide to do the right thing, but often does not. And this is not a DOE statement. This is a collective opinion that 14 countries, the CEC and the IAEA all contributed to and finally agreed on. So it originally said much stronger things than it does now.

COHON: I understand.

VAN LUIK: But I think the basic principle is correct. Don't do any damage that wouldn't be acceptable today, and in keeping with that, make sure that you don't destroy society today to protect it in the future.

COHON: Last question. With regard to schedule, it's no surprise that TSPA-SR, for the purposes of SRCR at least, is set in terms of its content, more or less. But I also infer from the fact that you're already feeding stuff to SRCR that the design is probably set as well. Is that a fair assumption, or am I making a leap there?

VAN LUIK: You're making just a little leap. The portion of the design that's important to PA is the setting of the design. What we're going to be doing is looking at the design that was explained to you this morning, and then look at the lower temperature variation as the sensitivity study to see what the differences are in the outcome.

When you're talking about the addition of what we in PA would consider minor additions to the design, or subtractions, of course we immediately look at those through sensitivity studies, but we don't think that those types of things would materially change the outcome of TSPA.

COHON: Well, just to pursue this a little bit further, because I think it's so central to what we're going to be focusing on for the next several months, if in those sensitivity studies the PORB or someone else were to say Eureka, you know, we really ought to go with a cold repository, is it too late to put a cold design, a below boiling point design, into SRCR?

VAN LUIK: For SRCR, it would be my opinion only, and Russ Dyer is the boss, for SRCR, I would say we would go ahead with the current design, since it will have the discussion of the alternative, but for SR, that would be a different case. And, in fact, it would give us, you know, something to explain and make things more difficult in the public meetings that we'll have, say here's the document, of course there's been a change, and we'll address that in the SR.

But I would say that that would be the right way to do it, because to stop everything at this point and not go forward with basically the declaration that you're thinking about, you know, making a site approval, recommendation to the Secretary, I think is not justified just on the basis of that alone.

COHON: Thank you.

KNOPMAN: Okay. Dan Bullen?

BULLEN: Bullen, Board. Abe, if you could actually flip to Slid 10, please? Your first comment about the software package, GoldSim, which by the way I've been using, too, and I did notice was a little buggy, raises an issue about validation and verification of the code, and will that be necessary before SR, or are you just going to make sure that it's done before LA?

VAN LUIK: To a large extent, it will be done before SR. In fact, the debugging that I am talking about there is basically a verification. Golder has done an excellent job, basically, of verifying it. Where we are having a more difficult time with verification is in the calls it makes to FAM and those other codes. But the checking process is in full swing, and that's why, you know, even though we have the first runs last week, we have learned from the VA experience, until the checking is done, you know, you don't talk about them, because VA, what we first did and what came out after checking was quite a bit different.

BULLEN: Right. So the pedigree will be in place for SR, is what you're saying?

VAN LUIK: The pedigree will be in place for SR, and it will be even firmer for LA, unless of course we do something drastic and go with a different design, or something different.

BULLEN: I guess just as a followup to the second bullet where you talk about the modifications to the thermo-hydrology, could you tell me how the modifications are to be done, or how significant the modifications were, keeping in mind that I'm not a thermo-hydrologist?

VAN LUIK: It's my understanding that the thermo-hydrology calculations were rerun and that the impacts on the flow fields were rather minor, and that's all I know at this point. You see a slight contradiction between this viewgraph and the previous one saying we're still waiting. They are actually coming in this week.

BULLEN: Thank you.

KNOPMAN: Okay. Priscilla Nelson?

NELSON: My comment is regarding Slide 4, and this decision to include 100,000 year calculations, with the express purpose of demonstrating how the natural environment kicks in. And this sort of stumps me because to me, the natural environment has kicked in from day one.

VAN LUIK: Yes.

NELSON: It is control of the environment that exists in the subsurface, and the consistency of that environment, and the ability to design a waste package for that environment is due to the natural environment.

VAN LUIK: You're absolutely right.

NELSON: And I do not understand why there cannot be some way created to encompass that participation of the natural environment in the performance of the first 10,000 years of the repository.

VAN LUIK: It's exactly as you say. In the first

10,000 years, the natural environment controls the environment in which the waste packages and drip shields do their job. However, things like the flux that is potentially able to carry radionuclides, we don't see that happening until the first failures of waste packages.

Now, we have two choices in order to evaluate, you know, just how that works. We could artificially fail waste packages early, or we could just carry our calculations out to where all those other processes kick in, and that's what we've decided to do here. Plus, I think if you're trying to demonstrate that you comply with the 10,000 year case, it's very nice to know that at 11,000 years, you don't go straight up, you know, on the curves.

KNOPMAN: Paul Craig?

CRAIG: Craig, Board. My question relates to Number 12, your summary, and specifically the last bullet talks about sensitivity and barrier analysis. When you use the language barrier importance, that suggests that you are indeed thinking in terms of well defined barriers. And if you are thinking in terms of well defined barriers, which I would think you should be, that is getting you in the direction of defense in depth, which, as you know, the Board is much interested in.

Some of the most interesting graphs we've ever seen were the one off analysis, which is a certain form of sensitivity analysis. To what extent will that kind of analysis be included in the present activities?

VAN LUIK: That analysis will not be completely reproduced the way it was done before. What we're thinking of doing is staying within the distributions rather than going outside of them and setting things to zero, with, like, whichever direction fifth percentile or 95th percentile is pessimistic, taking all of the properties of a barrier and setting them at pessimistic values and evaluating things that way as a show of importance. These analyses have been defined, but they have not yet been carried out. And if that doesn't do the trick, then maybe we need to go back to something more drastic.

But we felt that the problem with the other analyses, they were excellent to give us insight into what's important and not, but the problem with them was that they were fictitious because they lay outside the realm of what we thought was possible. And so we would like to do the same thing within the realm of what we think is possible.

CRAIG: Well, another way to think about the same question is in terms of the bounds for what is possible. And there are big issues relating to the degree to which

C-22 stress corrosion might or might not be important. That's an absolutely key thing.

VAN LUIK: That's a key uncertainty, yes.

CRAIG: It's a key uncertainty, and if your bounds are too small, you basically say that stress corrosion, cracking can't occur for 50,000 years under any circumstances, then there's a whole set of issues which you simply don't examine which some folks think are really important.

VAN LUIK: Yeah, that is one of the ones that we're going to stress, and in fact we're looking very hard at the uncertainty assumptions that have gone into the analyses so far.

Another thing is that when it comes to the bigger issue of, you know, have you defined, or what if you're completely wrong about something, we do have the drip shield on top of the waste package, and we, in the past, through the one off analyses, have shown that for 10,000 years, one or the other will do the job. So we're looking for something a little bit more complex to give us insight for this next go around. But certainly, you know, the Board will help be the judge of whether we have achieved that objective in showing importance and at the same time staying within the realm of what we think is possible.

KNOPMAN: We have a couple questions from staff. Dan

Metlay?

METLAY: Dan Metlay, Staff. Abe, I just have a point of clarification on your Slide 3.

VAN LUIK: Okay.

METLAY: With reference to the compliance argument in Volume 2, are you going to look separately at these various time periods not only for the maximum dose, but also for the EPA groundwater protection standard?

VAN LUIK: We are going to look at what those particular regulations 963, 63 and 197 require, which is strictly a 10,000 year peak dose evaluation. We will look at addressing the groundwater requirements. But this will be difficult for SRCR because we don't know all the nuances until later this summer. But definitely we will address that requirement. There's no question about that. But nothing beyond 10,000 years, because this is an argument saying, as 963 says, because we have high expectations of being able to meet what society has laid down regulatorily, we believe that the site should be recommended. I think that's the way it's going to come out.

KNOPMAN: Leon Reiter?

REITER: Leon Reiter, Staff. Abe, just a few questions on compliance. For the first 10,000 years, you used to talk about having an order of magnitude of margin between what you calculate and what the criteria is, and the last time we see that, we're talking about safety margins. What are you thinking of in terms of how close enough do you think is good enough to be?

VAN LUIK: Well, that's a good question. You know, it really is a moot point when no waste package has failed for 10,000 years.

REITER: We know there are other things that could happen, that could occur that might give you a dose.

VAN LUIK: Yeah. I'd feel pretty good if the final numbers come out an order of magnitude lower than the regulation. I'd feel really good if they come out two orders of magnitude lower, because in the compliance process where the NRC will put us on the stand and ask us what we're sure of, you know, we will be forced to do calculations that are much more conservative, and so we need that margin for the licensing aspect.

REITER: But this is part of the repository safety strategy, one of your main elements. Are you going to declare before and say, hey, we want to achieve this kind of margin?

VAN LUIK: RSS-4 declares that we need margin, but it, again, does not specify how much. Maybe it should. We'll have a discussion on that.

REITER: Second question is with respect to peak dose. I think on Page 10, you say DOE interprets the document to suggest that peak dose just translates to a small fraction of natural background in terms of potential added dose. If I remember the calculations correctly, your peak dose was more than a small fraction of natural background. So is that going to be a criteria?

VAN LUIK: The third bullet also needs to be factored in. To set an arbitrary limit on a dose that's 300,000 or 400,000 years in the future is I think pound foolish.

REITER: That overrides the--

VAN LUIK: I think there's a tension between those two and, you know, I have a personal opinion, but the reason we put together this task force is to look at all sides of this. My very personal, non-DOE opinion, anything below 100 millirem is acceptable because that's what the regulators say. But that's my personal opinion.

REITER: But above 100 millirem is not acceptable?

VAN LUIK: Yeah. Of course then you're looking at uncertainties that just kind of spin out of control at that time frame, too. PA is not a tool to predict the future. It's a tool to give you indicators of performance, and there's a big difference between those two. So the task force that's looking into this, of which I'm only a peripheral part, has to weigh in all of those aspects of the uncertainty.

REITER: When will the results of the task force be available?

VAN LUIK: Usually these things run a month or two, I would think.

KNOPMAN: We have time for one last question. Dave Diodato?

DIODATO: Dave Diodato, Staff. With respect to Bullet Number 2 here, we're definitely interested in incorporating the thermo-hydrology into some TSPA analyses, and Dan Bullen brought up the question and you said your understanding was, well, some of these things have been put in there so far and you didn't see a big impact. So at least to date, your analyses with thermo-hydrologic effects in the TSPA didn't bump it that much one way or the other. So one of the things that we've been talking about, and we kind of wonder, is have you demonstrated that you have any sensitivity in your analysis itself to these changes?

VAN LUIK: Well, I think that's the challenge before us. If we have 100 per cent total confidence in the TSPA model and the way it addresses this, then we would just declare to you that this point, although it's interesting, has no meaning in terms of public safety or health. But we do need to look and carry out the 3-D calculations that have been proposed at the drift scale, and we do need to look closer at this before we can declare a victory on this one. So it's a work in progress. But right now, we feel that we have incorporated a lot of the thermal chemistry and a lot of the thermal hydrology results, bounded them directly into the PA. So we're beginning to feel more confident than we have been that whatever comes out of these closer studies will not lie outside the bounds of what we've done.

DIODATO: Also, you'd be interested in looking at the empirical basis for the analyses and conclusions in some cases where the actual data is somewhat scant?

VAN LUIK: Yeah. In fact, the AMRs have that burden, exactly, to not only give the calculation that goes to a PMR into TSPA, but to give the basis for that and say why this is or is not sufficient work and what still needs to be done. So we hope to be documenting exactly what you're talking about.

DIODATO: So would you be able to then express it in terms of an uncertainty thing in your TSPA analyses because you have a large uncertainty in your empirical database?

VAN LUIK: We are certainly attempting to do that. But it's such a large and convoluted problem that although we may be real pleased with the results, someone else coming from some different aspect of the science may think that there's more work to be done.

DIODATO: So, in fact, the output from an ambient simulation versus an elevated temperature or above boiling temperature simulation, they might all be within the same bounds of uncertainty, so you can't necessarily pick those out until you're quantified that.

VAN LUIK: Yeah, intuitively that makes sense, because we have a waste packages that's pretty immune to temperature and the environments. It's pretty immune to the whole range of chemistries that are expected in the environment. And if they last more than 10,000 years, then what we're talking about is a prehistoric blip basically in the environment that they have experienced.

DIODATO: Okay, that was different from my understanding, which was that the waste canisters, the confidence in the cans' performance goes down with increased temperature.

VAN LUIK: Well, that's an argument we probably should have in a meeting dedicated to that with Joe Farmer and others up here. But the reason we went to Alloy 22 is because it is immune to the environments at the temperatures that we expect. There's basically very little difference between the coupon tests in the higher temperatures and the lower temperatures, for example, and we still need to make that case.

This is all preliminary, but this is where we feel the direction is going, and we need to have Rick Craun finish his trade study, basically saying if you go colder, you buy more confidence here, but you're also, you know, excavating more, exposing more people to radon, all kinds of other things. Those things all have to be factored into the final decision, I would think.

KNOPMAN: Okay, thank you very much, Abe. We're going to move along here. Our next speaker is Mark Peters, who will give us a scientific program overview. Mark is with Los Alamos National Lab, but his title is Testing and Engineering Support Office Manager, but most importantly, Mark plays an important role in technical integration in the program among the science, construction and design organizations.

PETERS: Thank you. Can everybody hear me okay?

Thank you very much. It's good to be back talking to you all. Today's scientific program overview is going to focus, as was noted in the introduction, primarily on the cross drift. We have a limited amount of time today, so we are going to focus on the unsaturated zone, and the testing in the underground.

Again, the objective, I want to provide a status on the natural system testing program, focusing on the unsaturated zone. It is a testing overview, but I will refer to the sub-models, particularly in the case of the unsaturated zone model, where a lot of this testing information is feeding into to improve our understanding in the unsaturated zone.

Let me back up one second here. I will talk a little bit about ESF studies, Alcove 1, and then briefly on Alcove 5, the drift scale test, and then move into the cross drift status on the ongoing testing activities, construction and testing activities in the Alcove 8 and Niche 5 area, and also a discussion of the bulkhead investigations that you've heard about the last Board meeting, hydrology, and also a brief update on the organic material that we've observed going behind the bulkheads.

Something you haven't heard about before, some seepage/drainage benches that we've constructed to understand better the fracture hydrolic properties in the Topopah Spring, a brief discussion of some analyses that have been done recently by the U. S. Geological Survey, looking at rock chemistry across the different sub-units of the Topopah Spring, and then finally summing up something that the Board requested, a set of bullets summarizing what we think we've learned in the cross drift, opening up into geology and hydrology and geochemistry.

You've seen this figure before. Just to remind everybody, the ESF, and then the potential repository block here, north is in this direction, the cross drift that goes over the top of the ESF, and over the top of the repository block, talking in the ESF studies mainly on Alcove 1, and the drift scale test in Alcove 5. And I'll have a more detailed layout of the cross drift later in the talk to bring you up to speed on where everything is located in the cross drift.

First, Alcove 1. We've talked about this over the last several Board meetings. Here we're evaluating infiltration and percolation through welded tuffs in the unsaturated zone. This test supports several sub-models, including the UZ infiltration model, the drift scale seepage model, as well as the transport models.

In terms of an update, we're continuing to apply water at the surface above Alcove 1, about 28 meters above Alcove 1. We have introduced, as you know, we put about 10 to 20 ppm lithium bromide in all the water that's used in the underground, but we had increased the concentration of the tracer to up around 500 parts per million, and we were watching how that increased concentration entered into the alcove below.

We turned off that higher concentration injection fluid at the end of January of this calendar year, and we're continuing to collect water and analyze the tracer.

This is a summary of the results that we've seen in the Alcove 1 tracer experiment. Plotted here is date versus bromide concentration, concentration at a given time relative to the concentration that's applied at the surface. So if we have a 500 ppm breakthrough, you'd see a number of 1 here. So we're simply plotting. Let me walk through what you're seeing here.

There's two sets of data. The green squares and the red squares are all data collected within the alcove. So water samples taken from within the alcove analyzed for bromide concentration. Three different model simulations plotted, the blue--this line here, of course, when we turned off the tracer at the end of January. The teal line is a one dimensional injection, dispersion model where we assume that we continuously injected the tracer at the very high concentration. The red line, prediction at 1/7/00, utilizes this green data here and does a prediction for what we thought we would see where we turned it off, when we turned off the increased concentration on January 31. Whereas, the black here called preliminary USGS model is using the same equations, but incorporating all the data.

As you can see, instead of the nice smooth curve, we do see significant flattening, and if we were to say what we think we're going to see, we think we're going to see a relatively slow decline as we go out. So we are seeing the effects of dispersive matrix diffusion type processes in the test.

I should mention that that will be detailed modelling done by Lawrence Berkeley of those test results. This is a relatively simple one dimensional calculation.

Drift Scale Test, don't need to go on on this too long. I will state Jean showed a figure earlier of results that was basically a line along the drift here. I'm only going to talk very briefly about what we've done with the heater power since we last talked to the Board.

A figure you've all gotten used to, total power and a representative thermocouple on the drift wall, it happens to be a thermocouple that that sits about halfway down the heated drift. And a reminder, we were--the target has always been 200 degrees Celsius at the drift wall, and we're just about there. We, in fact, are there at the drift wall. Some of the thermocouples actually went over 200 C. by a slight amount.

So getting to that point, one of the goals was not to exceed 200 C at the drift wall, and if you'll remember, we have the ability to adjust the heater power continuously. So to meet this goal, we've recently turned back the power output on both the wing and canister heaters to 95 per cent of the output prior to the adjustment, and we're monitoring the temperatures on a daily basis to see how that adjustment has affected the temperature at the drift wall.

The next slide shows temperature in degree celsius as a function of time for several thermocouples. Each line is a different thermocouple all along the right rib of the heated drift. There's quite a bit of variability. As you know, there's edge effects as you get down towards the back, towards the concrete liner, and also towards the bulkhead, you get some cooling. The point being we were up around 200 C at some of the hotter thermocouples. This right here is a pretty major power outage.

So you can see we turned down the heaters in early March, and then we had a power outage a couple weeks later, so that's caused us some difficulty in evaluating how things are going. But as we recovered, we're seeing that some of the thermocouples are still above 200, so we are in the process of evaluating when we want to turn that heater power back even a little bit more to try to get to that 200 C.

I won't speak a whole lot more to the drift scale test. Jean talked a little bit about some of the moisture movement evidence. And, again, I'm going to focus more on the cross drift today.

A layout of the bottom part of the ESF and the cross drift. You've seen this diagram before, but I've added some things to the diagram. First off, what's in black and regular text is things that are either in place and completed, or under construction, meaning so the things that are in blue and in Italics are planned, so those don't exist yet. We thought that was important that we point out what's in the plan versus what's actually being implemented in the field.

We also added tick marks here showing the contacts of the zones within the Topopah Spring. So the upper lithophysal is exposed in this section, the middle non-lithophysal in this section, and the lower lith, which is of the most interest, over this large portion of the cross drift. And then lower non-lith all the way up to the Solitario Canyon Fault.

I'll talk mainly today about the Crossover alcove, which is an alcove that's being excavated off the left rib, and out over the top of ESF Niche 3. Niche 5, which is a seepage, where we're doing seepage testing, again in the lower lithophysal. And then the bulkheads are installed, one here about halfway down, and the second bulkhead here down near the fault, the Solitario Canyon Fault.

First, status on Alcove 8. Alcove 8, Crossover

Alcove, you'll hear them called both, it's at about 800 meters from the entrance to the cross drift. It's in the upper lithophysal in the cross drift and it's a test utilizing ESF Niche 3, which is about 18 meters below. ESF Niche 3 is in the middle non-lithophysal, so the contact actually runs about halfway, a little over halfway underneath the Crossover Alcove.

Here, we're after a very similar experiment to Alcove 1, flow and seepage processes, but here we're in potential repository horizon rocks, and we're looking at the scale effects, relatively large scale test, again supporting seepage and transport models in the unsaturated zone.

In terms of status, we've completed--this is just an isometric diagram of Alcove 8, with ESF Niche 3 underneath, we've completed excavating the alcove with an Alpine miner, that's complete. We've drilled the holes up from Niche 3, and we're in the process right now of drilling the holes down from Alcove 8.

I should also mention these blast monitoring bore holes were excavated. They were going to be used when we were planning on excavating the alcove with drill and blast techniques. We since have decided to excavate it with an Alpine miner. This is about 18 meters.

So the test layout is there will be a three by three meter infiltration plot in the floor back in the back of Alcove 8. We'll introduce water with tracer, and eventually probably vary the concentration of the tracer, and then monitor, using these holes, using active geophysics measurements, as well as collecting water in the roof of Niche 3, using collection trays much like you see in Alcove 1.

We excavated Alcove 8, a Crossover Alcove, with water, a limited amount of water, but nonetheless, there was water used. There was a wet area, a wet spot in the roof of Niche 3 that was observed during construction of Alcove 8. We think we've identified the fracture sets that were responsible for the flow, and they will be studied as part of the test. We feel there's little adverse effect on the test from the water loss during mining. We're doing baseline measurements now in those holes that we have and are drilling, so we'll baseline the test, so we're looking at differences much in the way we've done in the Alcove 5 experiments.

There is a small fault, when I say small, less than a half meter of offset, that connects Alcove 8 and Niche 3, and that's going to be studied in detail. In fact, the scoping test that's just about to start in the next couple weeks, primarily driven by demonstrating our ability to recover water, is going to be located over that fault.

Moving to Niche 5, 1600 meters from the entrance to the cross drift. Here, we're in the lower lithophysal unit. There, we're after evaluating drift scale seepage processes in potential repository horizon rocks. Remember, the ESF Niche studies were all in the middle non-lithophysal. Here, we're in the lower lithophysal. This supports the drift scale seepage model.

In terms of status, this is another one of the diagrams showing the layout of Niche 5. It's, again, about 1600 meters from the entrance to the cross drift. It's broken up into two phases of excavation. The first phase is a 15 meter access drift. That excavation is complete. That was excavated with an Alpine miner again.

We then come in and drill a series of pre-niche excavation bore holes, and we've also drilled, not shown on this diagram, three bore holes along the axis of the access drift from the cross drift, and these holes are used for air permeability testing. So we're injecting air, and we're backing out air permeabilities, and also released liquid, basically water with dye, food color dye really. And then as we excavate the niche in Phase 2, we'll then look for that dye systematically to try to identify pathways that control flow, and then also use the air permeability measurements to understand the seepage behavior within the niche.

So we've drilled these holes. We've excavated this Phase 1, and the Alpine miner is in there right now as we speak excavating this second phase. This started late last week. And then there will be a series of bore holes drilled within the niche itself.

In terms of results, most of the results from Niche 5 are primarily at this point air permeability measurements. What I've plotted here is nothing really plotted along the X axis except different locations, and then log of permeability with the mean, this little tick mark, and plus or minus on a standard deviation.

Plotted here are results from three of the ESF niches. So here's middle non-lithophysal. Darcie is right here. So this is one darcie, if you think in darcies. So basically, in the less than darcie range, quite a bit of variation within the middle non-lithophysal.

If you go to the bore holes from Niche 5, you can see that there's quite a bit of heterogeneity, but the permeabilities are equal to or even greater. These are air permeabilities equal to or greater than what we observed in the middle non-lith in the ESF.

Bulkhead investigations. Here, we're evaluating flow and seepage processes. Again, the bulkhead is just beyond Niche 5, so it isolates the lower lithophysal all the way through the Solitario Canyon Fault zone from ventilation.

Remember, we have instruments installed the length of the cross drift systematically, and so we're measuring water potential systematically through the different units and behind the bulkheads without ventilation effects.

So what we're seeing right now is the shallowest depths, the probes that are installed at shallow depths are still wet, showing evidence of re-wetting, because they were dried out while we were ventilating. Whereas, the greatest depths are still drying out, and probably are the source of the water for the wetting at the shallower probes.

The first meter of the rock may still be too dry for seeps to occur. We haven't seen any evidence of drips or seeps from the rock. We have seen condensation. That was discussed I think at the last meeting. But it hasn't been detected within the rock. Most of the condensation current hypothesis is that it's condensing from the air. We think that that's probably due to a thermal gradient.

As you're aware, there's still power being run to the tunnel boring machine, which is parked at the back of the cross drift. So since we've talked last, we are, starting in June, are planning to install a third bulkhead just behind the tunnel boring machine, with insulation on the down tunnel side, and also rewire the lights, because the lights were also wired to the TBM feed as well. So we're going to be able to turn off the lights and hopefully disturb that thermal gradient to try to minimize the test interference as much as we can.

I've already talked through this. This is just an example of a nest of instruments, heat dissipation probes. Here is plotted just time versus water potential. So dry is in this direction. We're drying as you move up the Y axis. These are just five different probes at different depths. You can see this here is the evidence that you're seeing at shallow depths of re-wetting. These deep probes are the ones that have not been disturbed by ventilation, and are showing what is "the ambient" water potential within the cross drift. We've talked before about the importance of that data, in that they were relatives "wetter" than what we had seen before.

Organic material. There's been several species of fungi that have been identified in the cross drifts. They are concentrated near the second bulkhead. They tend to occur on the conveyor belt and the rail ties. Remember, there is wood rail ties in the cross drift. That's a generalization. It does occur in other places, but it tends to dominantly occur on the conveyor and the rail ties. It's, again, concentrated near the second bulkhead, several different species, probably 10 to 15. I want to say four to five different genus, and all told, 10 to 15 different species of fungi.

We are characterization it. We have some preliminary results of the organic material, and we do have plans to evaluate the implications for waste package performance in particular.

Moving on to the seepage/drainage benches, something you haven't heard about, I don't believe, before, at least at a Board meeting. I'll show a picture of what one of these looks like. It will become clear. But the purpose is to characterize the fracture properties. So we're doing these systematically within the Topopah Springs. This is a USGS experiment that's being conducted by Alan Flint and his people to characterize the fracture properties, help evaluate seepage and drift drainage.

It supports those two sub-models, and the detailed objective is to spatially correlate the fracture properties to other measured properties. We're doing these primarily in locations where the U. S. Bureau of Reclamation has done detailed fracture mapping, so we can tie that to the geologic observations and also tie that to the systematic air permeability measurements that are ongoing that Berkeley is doing within the cross drift.

Just to show you the locations of the benches relative to some of the other testing, this is cross drift station in meters, and what's plotted here is the percent lithophysae in this gray color. So here's the upper lithophysal, middle non-lithophysal, lower lithophysal and lower non-lithophysal. The Solitario Canyon Fault comes in right at the very end of the diagram. So the percent lithophysae obviously varies in the lithophysal versus in the non-lithophysal zones.

Also plotted is the fracture frequency for ten meter interval of the tunnel. Now, this is a fracture cutoff of a meter or greater. Because, remember, we presented this I believe two Board meetings ago. If you look at fracture densities across the Topopah Spring, but you look at a smaller cutoff, like a 30 centimeter cutoff, the fracture densities tend to be relatively uniform across. These are just the long fractures.

The bulkheads, the two bulkheads are shown in the green lines, and then the bench locations, right now, there's been four excavated. We have not excavated the two behind the bulkhead. They're located at different locations within the middle non-lithophysal and the lower lithophysal at this point.

This is a picture. This is about a foot across here. So what we've done is we've just excavated some benches, kept them as flat as possible. This is simply a ring, and we're simply applying a known head, basically putting a puddle of water in here with a known potential, and watching it drain. And, again, that's being done at different locations within the cross drift.

In terms of results, there's a lot of information on this. I mainly just want to tell you the kind of information that we're collecting and how that might be used. I'm changing units on you, unfortunately. This is conductivity and meters per second. So a darcie in this plot is up in this area here. So this is lower permeabilities, and then this is potential, so saturated is here, basically saturated, so we're drying in this direction.

There's three different model curves. The purple, the green, and this shade of purple are all parallel plate type models that are predicting the change in conductivity versus water potential. There are two parallel plates with different apertures.

Then this middle non-lith matrix curve is a curve calculated based on the matrix hydrologic properties as measured by Lorrie Flint of the U. S. Geological Survey. So this percolation square here is based on the water potential measurements that have been measured in the cross drift. It basically shows that you need to invoke some level of fracture flow within the Topopah Spring to produce that observation.

Also plotted are, in the diamonds, are air permeability measurements from the middle non-lithophysal, the lower lithophysal and the upper lithophysal. And then the Alcove 1 experiment. Again, the Alcove 1 and the seepage benches have a lot of parallels. We're just applying a known potential on top and watching it drain through the system.

And then the yellow circles are results from one of the benches. This bench happens to be Bench 4, which is in the lower lithophysal. So as we continue to collect data, we're going to look for to define the shape of the curve, and then be able to back out fracture hydrolic properties from that data.

One of the other things that's been done recently by the U. S. Geological Survey is looking at rock chemistry. There were 20 systematic samples from the cross drift analyzed for major and minor elements, as well as trace elements. Why did we do this? It was required in order to provide the baseline for external criticality calculations. But it is of interest when you look at the details of the results.

There's a data table in your backup that has all the numbers. I didn't want to inundate you with a table of numbers, but if anyone is interested in the actual concentrations, that's in the backup.

But the basic observation take-home point is as you move across the different zones of the Topopah, there's relatively uniform rock chemistry. And to illustrate that is an IUGS classification diagram. Don't get lost in all the detailed geologic jargon. Some of us like to get lost in that. But the take-home point here is that we're looking at a rhyolite. We've known that. But the field of published analyses for the Topopah Springs falls within this circle here, and the 20 analyses that the U. S. Geological Survey has done actually fall in a very, very tight envelope right over here. There's very little variability in rock chemistry as you move across.

Now, to close the talk, I'm going to have a whole series of bullets entitled What Have We Learned in the Cross Drift. I'm not going to read through them. I don't expect you to read through them right now, but I am going to try to highlight the important ones. We thought it important to get all this down so that you saw all the detailed information on what we think we've learned. Again, broken up into geology and then focused more on hydrology and geochemistry in the last half of the set of bullets.

In terms of faults, no major surprises. Pretty much what we anticipated in the Predictive Report. The Solitario and the Sundance, in terms of location and characteristics, were very similar to what we expected. We did see one fault with about five meters of normal offset towards the bottom of the lower lithophysal, and that fault likely was obscured by alluvium, which is why it wasn't predicted.

Again, the Solitario was within a few meters of predicted location, and orientation and offset were essentially identical to what we predicted. There was only minor physical evidence of water percolation. What I mean by that is as we mined through it, it was damp. There wasn't free water.

There was no significant secondary mineralization. We did observe some minor iron oxides in the fault zone breccias very close to the fault. And we didn't see any significant accumulations, and I underline significant accumulations, of secondary silica or calcite. There is still likely some, but not significant accumulations.

Most of the normal faults in the region, usually the fracturing is concentrated in the hanging wall of the fault. In the case of normal faults, it's a block that's been dropped down. In the case of the Solitario underground, we actually saw a significant amount of fracturing as we approached the fault on the footwall side. We think that was due to a small splay that actually intersects the main splay that we intersected in the underground just north of the cross drift alignment.

So this was somewhat of a surprise. The highly fractured zone was on the order of 40 to 50 meters along the tunnel as we approached the fault. But I will say that in general, there was not much deformation within the rock mass between the major block-bounding faults.

I've already alluded to the fact that we've gotten a lot of information on fracture density in the different zones of the Topopah Spring. We've been able to see the lower non-lithophysal in the underground for the first time, and the fractures and the character of the fractures are not unlike those in the middle non-lith. And the dip of the units has been well constrained now between the Ghost Dance fault and the Solitary Canyon Fault.

One of the, I think, more important points, and one that I know you all are aware of is it's provided our first good look at the lower lithophysal, which makes up the majority of the potential repository.

Another interesting point, we've treated the lower lithophysal as homogeneous with respect to fracturing. But there is some heterogeneity in the fracture, the fracture patterns within the lower lithophysal, and our testing program with systematic air permeability and the bench experiments is going to tie that to the hydrologic response.

The intensely fractured zone. If you remember, in the ESF, roughly over seven hundred meters, from around 4,200 meters from the north portal to about 4,700 or 4,800 meters, in that range, there's an intensely fractured zone very closely spaced, nearly vertical fractures. That doesn't apparently extend to the northwest. The reason we can say that is we did not see it in the cross drift, and it's not exposed within the middle non-lithophysal and Solitario Canyon either.

Moving to hydrology and geochemistry, the chloride data, and again this is distinguished from chlorine-36, systematic sampling of chloride data within the Topopah Spring has been very, very useful in constraining infiltration and percolation estimates heavily used by the UZ flow model in terms of calibrating a flow field.

Of course, the cross drift provides access for sampling of chloride and chlorine-36 and the fracture mineral work that's been conducted by the U. S. Geological

Survey. To date, behind the bulkheads, and also as we were excavating, we saw no active seeps or drips from the rock.

The water potential data we've talked about before. That's in systematic bore holes across the cross drift. They're higher than previously believed. This last sentence here is probably overstated. The water potential data from the cross drift has been incorporated in the flow model, and it doesn't have a major change in the fracture matrix flow versus what we were using prior to that data being collected.

I've already talked about the air permeability measurements, and those are important, bearing on seepage and drainage.

Now, what will we learn? One bullet. It will allow for in situ hydrologic and thermal testing, some of which I've already talked about, in the lower lithophysal in particular. And there will be great value in that.

So, in summary, I hope I've given you a feel for some of the ongoing testing in the ESF and specifically in the cross drift. We continue to address the key processes in the unsaturated zone. And this data and analyses are being utilized in support of the process models, and then PA and design for the site recommendation.

KNOPMAN: Thank you, Mark. Questions from the Board?

Dick Parizek?

PARIZEK: Parizek, Board. Again, I appreciate the quick summary of a lot of very important points. On Slide 10, again I missed the morning presentation, on the heating up, it seemed like you've gotten it warmer than where you were before you had the power outage, and even as you're ramping down the energy.

PETERS: Yes.

PARIZEK: Is that sort of getting the power right, or is there something else going on here? Is it reduced power that was being put--

PETERS: We reduced the power by about 5 per cent. But this particular thermocouple actually recovered to a higher temperature. I can't answer that one. That's a bit puzzling.

PARIZEK: It requires some thought?

PETERS: Yeah, they've all actually gone to a higher temperature. The boundary condition at the bulkhead might--you know, we are removing heat from the bulkhead, so that could be causing subtle differences. But, again, we're still trying to figure out why that is, and then try to adjust it to get it back to 200. But I don't have a clear explanation for that right now.

PARIZEK: Slide 13, you have a cross connection between Niche 3 and Alcove 8, the vertical green and vertical red bore holes. Are they lined? I just began worrying about whether these are pathways for either things to dry out or for moisture to sneak down. Even though your little test plots are small compared to where these are, are they lined?

PETERS: They're not lined. They're plugged here, but they're not lined because we have to run instruments in and out.

PARIZEK: So that could affect flow or drying out?

PETERS: They run, it's hard to tell on here, but they run--the infiltration plot is actually in between here, but once you leave the alcove, it could very well spread, and those could become a factor. They're not lined.

PARIZEK: So it would be possible to have some effect because of the presence of the holes.

PETERS: Yes.

PARIZEK: One other question, and that was why not more secondary mineralization observed in the east-west crossing? Obviously, everywhere else it seems like there's a reasonable amount of it. Here, you talk about the general scarcity of it. Does that mean it was dryer, less water went through that part of the mountain?

PETERS: Or it went through it and it didn't deposit anything.

PARIZEK: Which would be kind of interesting. Or the fractures are newer?

PETERS: That could be, too. I mean, Zell Peterman is here and he may want to comment on that. But I don't think I'm prepared to say a whole lot more than that. It needs to be looked at within the context of what we see in the fractures, and the physae throughout the cross drift, before we could say anything for sure about what it means.

PARIZEK: So far, the observation has been--

PETERS: It's an observation.

PARIZEK: Thank you.

KNOPMAN: Priscilla Nelson?

NELSON: Thanks, Mark. Nelson, Board. I've got three sort of simple questions. One, last time you showed us a number of alternative devices that were measuring water potential. And you've only shown us one this time. Last time, I was looking forward to seeing what happened, because they seemed to be approaching different asymptotes. Is there any update?

PETERS: They were actually approaching each other.

NELSON: Well, one was going under the other one, I mean in terms of the asymptotes.

PETERS: Yeah. What you're talking about is we have behind the bulkhead, a couple stations where we've installed thermocouple sychrometers versus heat dissipation probes, because we were wanting to make sure that the probes were giving us the right answer.

NELSON: One is from the wet side and one is from the dry side?

PETERS: Right. HTPs are installed wet. Thermocouple sychrometers, dry. So they converged. I don't have an update on that, but we considered that within the precision and accuracy of the instruments the same.

NELSON: It would be real interesting to find out more about that, because I think the reliability of the instrumentation is something of great interest.

Regarding your bench test, when these are done in geotechnical engineering, quite often they're double ring.

PETERS: Right.

NELSON: To avoid boundary condition influence, in part, on a test section. Are you running these as double ring or single ring?

PETERS: When you say double ring, what do you mean by that?

NELSON: They have an inner ring and an outer ring, and you're really using the inner ring to measure.

PETERS: These are single ring. I mean, I can't speak to what the limitations are of that. Alan Flint would be able to do that when you see him on Thursday.

NELSON: That's fine. And the last question is do you find any indication that there is an effect of being under the crest in terms of higher water content, more moisture?

PETERS: Water potential, that's not apparent, no. It seems to be relatively uniform. The condensation that we see near the second bulkhead happens to be under the crest. That may or may not mean something.

NELSON: That's where you put the bulkhead.

PETERS: Yes.

KNOPMAN: Paul Craig?

CRAIG: Yeah, Mark, could you go back to Number 32? I want to talk about the last bullet there.

PETERS: Yes, sir.

CRAIG: The last bullet on that one observes that you haven't seen any active seeps. It seems to me there's some very strong conclusions that can be drawn from that, and it's worth noting, especially since we're going to be going up there. Some of the calculations suggest that under plausible conditions, that is, plausible meaning at ranges of the relevant parameters that are reasonable, you could get seeps over on the western end of the ECRB that amount to about a swimming pool a year coming down on top of a waste canister.

PETERS: Right.

CRAIG: A hundred cubic meters a year and up. That's a lot of water. That's a continuous stream. If that amount of water were coming out, that's a stream you would see. You wouldn't miss that.

PETERS: Yes.

CRAIG: So the fact that you haven't seen any seeps or drips allows you, it seems to me, to put some fairly serious constraints on a number of parameters, and those calculations are location specific along the ECRB.

PETERS: Right.

CRAIG: So it's not just a single number. There's a lot of constraints. And it seems to me it's worthwhile showing what those constraints are, because that's the first time you've had the ability to compare the calculations with actual data.

PETERS: Right.

CRAIG: So I contend that the failure to see anything has a very high level of numerical significance.

PETERS: Agreed. The only caveat I'd put on that, as you know, the influence, the thermal gradient influence that we've got in there may be inhibiting in some cases, so that's why we're trying to do our best to minimize that.

CRAIG: That's right. When you do the experiment right without the light bulbs, you'll be able to make much stronger statements. But you can already make some pretty strong statements.

PETERS: Yes.

KNOPMAN: Dan Bullen?

BULLEN: Bullen, Board. Actually, I wanted to ask questions about the light bulbs, which is Slide 18.

And I guess the question that I ask is a direct follow-on to what Dr. Craig says. And what was the power output of the lights, and if that amount of power has the impact of essentially stopping the condensation or keeping it dry, can you speculate on the long-term performance of a repository that has a very moderate amount of heat?

PETERS: I can't remember the exact--I should be able to know the power output of the lights, but I can't remember, but I'll say this. When they went in in January, I know Alan Flint had an infrared device with him, and he measured the temperature on the transformer of the TBM, and it was up at 32, 33 C. If you look at the rock, it's in order of 27, 28. The lights, he did notice an increase in temperature of a degree or two near the lights, but I can't remember exactly how much power those were putting out.

But in talking to Alan, if we turn the lights off, it would significantly improve our ability to--if you put the bulkhead up and then turn the lights off, that does a real good job of cutting back the power output overall back behind there.

KNOPMAN: Alberto?

SAGÜÉS: Yes. This is on Number 17. I'm curious, is this data going to work their way into seepage prediction models? Would that be an application of those results?

PETERS: Yes, both seepage--yes, that's what they're being collected for, as information to complement the eventual seepage measurements that will be done in the second phase of the niche.

SAGÜÉS: I see. In that case, that is the mean of the log; is that correct?

PETERS: Right.

SAGÜÉS: And, now, are those things supposed to be, like, log normal distributed; that's why you're choosing that particular way of plotting it?

PETERS: I don't think necessarily chosen for that reason. I guess we plotted this log, I could have just as easily plotted as one times ten to the minus twelve. I guess the significance that I was trying to get out of it that I wanted you to understand is that the preliminary results suggest that the permeabilities may be even higher in the lower lithophysal to air.

SAGÜÉS: I see.

PETERS: Than I think we see in the middle non-lith, and that's important for seepage. Higher permeability will tend to lead to less seepage.

SAGÜÉS: Just one very small value will throw your log average way low, and in that case, those numbers may be, if you use a log mean distribution, that may make the average look lower. That's not the average; that's something else.

PETERS: Okay.

SAGÜÉS: And it may be worse than what it looks like there.

PETERS: All right. But there is a lot of also, particularly in this particular instance, there's a lot of variability there, too, as well.

KNOPMAN: We have--do you have any more questions, Alberto? We have two questions from Staff, I believe, and just limit this to about five minutes so we can keep the program going. Dave Diodato?

DIODATO: Diodato, Staff. Thanks again for the excellent overview.

With respect, still thinking about the thermal hydrologic stuff, and the numerical models would suggest enhanced water circulation as a result of heat loading. So in the drift scale test, we have a chance to kind of look at that and see, you know, if that's borne out. So when we had the opportunity to be in the observation drift, we noticed that in the monitoring holes, sometimes there would be spillage right out of water, liquid water, and it would be some small volume. But I'm curious first, how long did it take after heating before you started to notice the spillage in terms of was it a week or was it-- if you look at--

PETERS: I can't remember the number. It's toward the beginning, it's like 6 or 7.

DIODATO: Yeah, seven. Okay. So the observation drift there, all those monitoring holes and--

PETERS: Yeah, we saw the water that's coming out of the hole in terms of out of the collar is this long hole here. Remember, as we were walking down, there's a little bit of water there. Now, we are collecting water from different intervals from these holes on the observation drift. The first water was encountered--it was within three to four months. It's been a while. There's people who could clarify that, if necessary, but it was relatively quickly.

DIODATO: Interesting. And then did you see any slowdown when the power got shut off? Is it sensitive? Or was that such a short time, it was three to four months?

PETERS: I don't think we've got enough data yet.

Where we're collecting water is moving in space.

DIODATO: Right.

PETERS: As the condensation zone is moving. But I couldn't really say, we can't say at this point whether the water is going to change based on the power reduction. It's too soon. We've only sampled water I believe once since we've cut back the power.

DIODATO: Do you have any kind of even a gross estimate of what kind of volumes you're seeing, you know, since this thing started?

PETERS: Let me--

DIODATO: I mean, do you measure the volume?

PETERS: Yes, we measure the volume.

DIODATO: Okay.

PETERS: In a lot of cases, we get on the order of tens of milliliters. But that's probably due to condensation in the tube as we're pumping it out.

DIODATO: Right.

PETERS: When you actually collect water that's not that, you're looking at on the order of a liter, anywhere from liter to two to three liters per interval. We've collected, oh, gee, I haven't added it up lately in the drift scale test. In the simulator test, we got 20 liters from one interval. In the drift scale, it's more than that total.

DIODATO: Thanks.

KNOPMAN: Any further questions?

(No response.)

KNOPMAN: Thank you, Mark. We're now going to continue on in our scientific work, but now focus more on geochemistry. Our next speaker is Don Shettel, who is with the Nye County Nuclear Waste Repository Project Office. He's going to give us an update on the County's work on geochemical and other scientific work.

Let me just say at this point, a reminder, we will have another public comment session at 5:20 this afternoon. So please let us know if you intend to speak at that time.

SHETTEL: Can you hear me? How's that?

I've been chosen to be the designated speaker for Nye County today, so I'm going to briefly talk about an update on our drilling program, and then give you a snapshot of some of our geochemical results to date.

We're in the second year of the drilling program, and summarizing, we have more than 17,000 feet of exploratory drilling completed, 17 weeks and piezometers at ten locations. We have collected geologic cutting samples, geophysical logs, and first water of occurrence from the drilling sites, as well as pump samples of water from the completed wells. Five aquifer tests have been completed, and the County has also supported some aeromagnetic and gravity surveys completed by the USGS.

Phase II started last October. We have one six well completion, one piezometer in spring deposit in Crater Flat, which is the seven well. We're completing the alluvial tracer complex, which is 19, in conjunction with the survey out in Forty Mile Wash. We have three piezometers at the Carrara Fault test site well at 12. And we have casings set for three deep wells for a deep drilling rig which is going to come in in a few weeks to go down to the carbonate aquifer I believe 5,000 or 6,000 feet at these locations. And we have two piezometer wells, 4-A and PB, which I'll talk about a little bit later. These have been in the news recently. And the initial round of water sampling for Phase II is in late May, but this will actually be the third round of water sampling from completed wells during this program. We have completed two in the first year, and the third one starts in a couple weeks.

This is a location map to show you where some of the wells are. The red wells are the wells that were completed in Phase I of the drilling, and these are primarily the ones that I'll be showing data for. We have 1-S, 9-S. I don't have a lot of data for 3-S, the three site is the other--most of the data I show will be from these three sites here.

The second phase we're working on are these blue squares. This well site is being worked on. Test wells have been completed they're working on here. Alluvial tracer complex is going to be put in right here. Monitoring wells I will talk a little bit about right there, just down from Gate 510 on the test site. And then the yellow triangles are wells that will be finished next year in Phase III.

There's one other well that we have some samples from that was--we did a pump test on in July of last year. This isn't the best viewgraph, but the gold mine that recently shut down in Beatty was required to put in some monitoring wells for the Park Service in Death Valley, and the pump test that we did was on this so-called Bond Gold Mining Well 13, which is right here, but all these blue spots out here, which are essentially west--see, here's our Site 1, 9-S, 3-S, 3-D, and the well recently completed this year at 12. The third well, 13, is due west of those, just a couple hundred feet from the California border, and there are a number of other wells out here that are used for monitoring purposes during the well testing in which we hope to sample some later this year as well, especially some I'd like to sample right in the center here between these wells over here and 13 that we have some data on.

I'm going to show you a snapshot of the data we have collected to date, and it's just a snapshot because we're collecting data all the time, and I put very little interpretation on paper because these can change with time. But I want to show you some of the analyses we're completing.

The Research Institute is doing our gross chemistry and metals by ICP. Geochron Lab is primarily doing for us now sulphur and nitrogen, as we're cutting back on some of the analyses that we did on the first water of occurrence from the wells. We found that that water is not as useful as was first thought, other than perched water samples.

Dr. Bowring, through Geochron at MIT is doing our uranium, lead and strontium isotope work on water samples. We've done a lot of gross Alpha and Beta lately through Barringer, which I'll talk about a little bit later. Dr. Zreda at Arizona is doing our chlorine-36 work for us as well as stable chlorine isotopes. I have a little bit of chlorine-36 data today, but we don't have any stable chlorine isotope data yet.

We're using a lab in New Zealand for our radiocarbon, tritium, total dissolved inorganic carbon and stable isotope data, hydrogen and oxygen and carbon, and my colleague and partner in Geosciences is doing, Dr. Morgenstein is doing the petrography and geochemistry of the cuttings. He's giving a paper Wednesday at the Devil's Hole Workshop. I'll touch on a little bit of his work, but really just the tip of the iceberg on that.

Most geochemists use diagrams, but I think that in this case, the pie diagrams give you a little more visual effect. Most of the water that we've found so far is the sodium bicarbonate type, with a few notable exceptions. On the left side, we're showing proportions of cations, and on the right side, proportions of anions. Like I said, the Bond Gold Mining Well, which is west of here along California, is the only water that is a salt, primarily a sulfate type. Calcium is the largest cation percentage, but it does not predominate.

Now, if we go east from the Bond Gold Mining Well 13, we have the Site 1, which are two wells, a shallow well which is 1-S, and the deep well, 1-DX. The area of these pies is proportional to the total dissolved solids. TBS here is about 1,600, and on the 1-DX well, it's a little bit more than that. It's maybe 1,700 milligrams per liter.

The typical of all the other waters that we found, bicarbonate predominates in the anion side. In the shallow wells at this site, we have no predominate cation.

But at the deep sample, we have a sodium predominate, and we believe the Carrara Fault goes through the sites of the shallow samples are above the fault. The deep sample from 2,100 feet and below is below the fault, which is in the hole.

Moving east and down Highway 95 to the nine site, we have four zones that we've sampled in there. The shallow zones at the top, again bicarbonate predominating on the anion side, and sodium primarily on the cation side, and not a whole lot of difference there in terms of the proportions of equivalent parts per million.

Moving further southeast along 95 slightly a few miles or less, the 3-S site, again bicarbonate predominates, but we have a much higher proportion of sodium in the water. So you see there are some differences as we go along the highway, and I'll bring out the reasons for that a little bit later.

A few weeks ago, one of our water samples made the news. It was a fairly radioactive sample. I figured the best way to explain that would be to show all the data that we have collected on that site.

The first line here is the Safe Drinking Water Act values for gross Alpha, the limit for safe drinking water is 15 pico curies per liter. Gross Beta is 50. Tritium, 20,000. Total radium is actually 5, not just radium. Radium, 226 is the primary radium isotope. Uranium isotopes are really included in that gross Alpha and Beta.

The initial sample that caused the furor was this initial drilling sample, which was bailed through the drill string essentially looked like chocolate milk. Nobody in their right mind would normally drink that. But it was a total sample, meaning it was unfiltered, and we got relatively high radioactivity.

Now, these red numbers are actually negative numbers, essentially below detection limit. Actually, a lot of these numbers are below detection limit, but the red ones are the most below detection limit.

A re-analysis--actually, the first analysis was called, somebody called this an error, but a re-analysis of this proved that it was not an error. It was correct. A later sample of this that was filtered showed much lower numbers and within the Safe Drinking Water guidelines.

The survey initially, from a sample initially collected on the four PB site, which is just about 50 or 80 feet away, and about 800--I think it was about 800 feet deep, the producing zone, was 4-PA, is around 400 feet deep.

At the same time, the survey initially found a high thorium concentration of this water of about 30 ppb, but it was a semi-quant analysis, 30 ppb versus two parts per billion uranium. This is somewhat unusual. Usually thorium is less than a part per billion. Uranium is higher. So it was a reversal, which you normally get in groundwater for uranium and thorium concentrations. So there was some interest at this site, so that caused us to look at some other isotopes here.

Later on after the drilling was completed and the wells were completed, we bailed some samples in February. These analyses were all normal. In March, we did some pump tests on these wells. So we collected pumped water samples, and again these were all normal. And since the public was interested in this sample as well, they gave us a sample from the Amargosa Valley School. We ran that for gross Alpha and Beta, and that was normal. Radium was certainly within safety guidelines.

I want to point out this is really a matter of perspective here when you consider that one pico curie is much less than a count per second, if you're thinking in terms of radioactive and taking a geiger counter into the field, or something like that.

When these holes were logged by geophysics, and we're looking at the radioactive in the rock here, the background count was normally less than a hundred counts per second. And so even if you multiply, to get one count per second here, you'd have to multiply this by a factor of ten, or 100 even, and so the only one that gets above one count is actually the initial drilling samples, which essentially have ground up rock in them. And still, the radioactivity is less than the rock itself, so we think that this anomalous radioactivity initially reported is simply the ground up rock in the water that goes away when you complete the well, and the water clears up and/or you filter the sample.

The State Health Department, as well as Bechtel from the Test Site, analyzed unfiltered samples from the completed wells, and they got the same numbers as we got for most of these things. So I think that should be the end of the story on this sample.

More or less striking things that we found in the data initially was this relationship between dissolved Strontium and Strontium isotopic ratio. When you look at the log of the dissolved Strontium, you see almost a linear relationship here. Samples from one well cluster here, the three site, going west to the nine site, you have here these samples, and the Site 1 furthest to the west along 95, you're up there. And they're all pretty much congregated in terms of the ratio as well as concentration, and we believe that this supports an isolation or a compartmentalization of flow systems in this area that was first suggested by Zell Peterman of the Survey in the early Nineties. And a lot of the other data that I'll show you tends to support this, but this is probably the first and most dramatic example that we saw of that.

Looking at dissolved Uranium versus Uranium isotopic concentration in the water, it's not quite as clearcut as the Strontium data is, but generally you see, and we see this in other samples from Site 3, there's a big difference between the shallow and the deep, relatively deeper part of the aquifer at Site 3. This is a deep sample at Site 1, which is essentially below the fault. The shallower samples above the fault, and then all of the 9-SX samples essentially fall in this little cluster here.

So we think we also see compartmentalization of the flow systems here as well, but we also see some other effects that are borne out in some of the other chemical data as well. And I'll get into some reasons why we have this difference at Site 1, other than being--I mean, essentially it's the fault, but there are some other very distinguishing features about that.

Looking at stable isotopic data for our samples, essentially hydrogen here versus oxygen, the water lines of Craig in the Sixties and modified by Taylor at '74. Some of our early first occurrence of water samples fall up here. J-13 is here. The Bond Gold Mining Well 13 is here. But our early samples are up here. Later on when the wells were completed and we could pump on the aquifers and get good samples, the values fall down here. There's a depth reversal here, but there's a nice progression with depths. You get generally more depletion as you go deeper in the aquifer, or with the groundwater samples, and we think this is indicative of these groundwaters are older, they were recharged at colder climates thousands of years ago, and we'll see that in the radiocarbon data.

This sample here is really labelled 1-DX is really the shallow, the first occurrence of water sample in the 1-DX well, which is really the same as 1-S. But the deep samples in 1-DX plot way down here. And, again, you see there's a discrimination between the--primarily in the oxygen compositions of the water from these three wells, 1-DX here, 9-S and 3-S, I believe is--or this is a shallow one here. A little bit of overlap, 3-S and 9-S over here.

Some of the more interesting data was the sulfur isotope data. Looking at del 34-S plotted against dissolved sulfate here, we have basically three groups of waters. The Bond Gold Mining Well 13 is up here, along with our deep 1-DX samples, and essentially these are very heavy, plus 27. These are essentially paleozoic marine sulfate waters.

The second group, which I call continental evaporites, these are essentially sulfates from gypsum and the soil. There's a very restricted range in sulfur isotopic composition, but a fairly large range in dissolved sulfate, or relatively large range in dissolved sulfate.

And then the third group has a fairly restricted range in dissolved sulfate, but a fair large range in sulfur isotopic topic value. We think this is a mixture of these continental evaporitic type sulfates, essentially fresh water sulfates that are mixing with sulfides that are oxidizing in the rocks, and sulfides are generally depleted way down here somewhere. But when you form a mixture, you get a composition that's between these two groups, so you have this middle mixture, which shows this large spread, relatively large spread in values. And, in fact, when Dr. Morganstein looked at cuttings from 3-D, 3-S, we have sulfides in the rocks as well.

I should point out that some of these other samples here are not part of the Nye drilling program. These were from compilation from the USGS, compilation in 1995. And these are all data that are within an area of about 3 degrees latitude, longitude, centered on Yucca Mountain, so not necessarily right around Yucca Mountain, but within the general area of Yucca Mountain.

An example of our data from New Zealand on radiocarbon, in this case applying against Tritium, they looked at a number of parameters for us. We find our deepest samples here, 1-DX, these are essentially two samples collected at slightly different times, and they show the lowest radiocarbon.

The age range here in radiocarbon in apparent uncorrected ages is 10,000 to 40,000 years. The Tritium values are all fairly low, and we think this is just a natural variation in background Tritium in these samples. But, again, you can start to see discrimination here between the deep sample in 1-DX, the 1-S zones are here, 9-S are here going from deepest to the shallowest zones. And then there's a big difference in the three between the deeper zone--or I should say the deeper zone at three, it's not that deep, but the deeper shallower zone at three, and then the shallowest zone at three show the largest difference for being essentially adjacent aquifers, separated by I believe just a clay sediment layer.

KNOPMAN: Excuse me, Don. Just in the interest so you can plan, we're planning to take a break at ten after

4:00, and I know Board members are going to have questions on the presentation.

SHETTEL: Sure. I'll try and get through this then.

Now, when we compare some of our carbon data with, again, data compiled by the USGS, we have the deep carbon at aquifer from UE-25 P1 is right here, I believe, and then you had samples from around Yucca Mountain. And we got all results that are tending to fill in between, the carbonate aquifer and other shallower zones at Yucca Mountain that are above the carbonate aquifer, mainly 1-S is here. We have four samples here, two samples each separated by six months and they form a very tight cluster.

The Bond Gold Mining Well, which is essentially across the valley, the west side of the Amargosa Valley, and the Funeral Mountains are here. Two samples at the shallower zone of 3-S, six months apart. Deeper zone are here. And then there's eight samples essentially of 9-SX that all plot right in there, and they represent four different zones in that well. But, essentially, they're filling in between--I should point out this is the one DX sample, the deep, greater than 2,100 feet, is almost identical to the carbonated aquifer sample at P1. And other samples, this is the shallow, essentially above the fault, from this sample here. This is 3-X. Actually, as we go east, we have 9-S and then 3-X.

But generally, the point is we're filling in between the deep carbonate sample here and other samples at Yucca Mountain up here. So I think this represents an increasing influence of water perhaps up-welling from the deep carbonate aquifer as we go east towards Yucca Mountain along Highway 95. And there are some reversals, of course, and that's due to the compartmentalization of the flow systems by faults essentially along the highway. That was in radiocarbon.

We see the same type of thing in stable carbon isotopes. The deep 1-D sample is very similar to P-1, and then our other samples at 1-S, the shallower samples at 1 as we go east to 9-SX samples, and then further east, we have the 1-S, and then we get into the normal--I shouldn't say normal--but the other samples around Yucca Mountain that are closer to the repository footprint. J-12 and 13 are here. And this is essentially stable carbon isotopes versus dissolved bicarbonate in the water.

Recently, I received our first chlorine-36 numbers from our samples. Chlorine-36 on this axis versus dissolved chloride here, and if we ignore the Bond Gold Mining Well sample, which is essentially across the valley in the Funeral Mountains, this with this very limited data said we might see a trend here suggesting that the chlorine-36 is decreasing as we get higher dissolved chloride in the water. The error bar is one segment, are over here for these samples. But, again, this is a very limited dataset, but I think we're starting to see suggestions that the samples from these wells are different--essentially the same sites are showing isolated ranges in chlorine and chlorine-36. And, again, this tends to suggest that we have compartmentalization or isolation of the flow systems in this area.

Nitrogen isotopes are used usually in a trace pollution from cattle farms, feedlots, dairy farms, what have you, fertilizers from agricultural, but we don't expect any of that in this area. We think this is a fairly pristine area, and this is not where we're looking at nitrogen isotopes for.

The standard for nitrogen isotopes is the atmosphere, which is essential at zero on this scale here, versus dissolved nitrate. And basically what we're seeing here, the early first occurrence of water drilling samples down here at high nitrate close to atmospheric nitrogen, and as we sample later on in the completed wells, we go to lower nitrate compositions and higher nitrogen isotopic, more enriched values.

Nitrogen isotopes can reflect complex biological processes. We don't totally understand this. However, juvenile nitrogen in the volcanic rocks can be very heavy up here at maybe plus 15, so we might be seeing a contribution here of nitrate from the soil zone with juvenile nitrate from the volcanic rocks. It's just speculation at this point. But at any rate we ought to look at normal gases at some point so we can get an idea of paleo climate in this area. But being that the drilling fluid is there that we're using, we may have to pump on some of these wells a lot to perhaps get rid of this apparent effect of atmospheric nitrogen in the water around the wells, at least that's one idea for that.

Another idea that we're looking at is dissolved fluoride in the water is a possible tracer of flow from Yucca Mountain, and along this respect, I have a contour map here. We have high value at Yucca Mountain. There are high values down Forty Mile Wash, and as we get down into the valley here, there tends to be an increase in fluoride concentration as you go towards Forty Mile Wash, although there are--this is where we're also postulating we have a break-up in the flow systems by faulting, essentially the compartmentalization of flow systems. Contouring is only a way of representing the data, but it's an idea that we're looking at. But it seems to suggest there may be a significant flow down Forty Mile Wash from Yucca Mountain.

KNOPMAN: Don, we are running short. So perhaps if you want to make sure you show you the things that need explanation here?

SHETTEL: Lorrie has looked at the cuttings. One thing I'll show here is in Hole 3, there was a gamma anomaly at about 500 feet that we looked at in the cuttings. This turned out to be a high Uranium concentration. When we dated this, when Lorrie had the sample dated, we got this age of a date. And looking at all the other elements in the cuttings around this particular sample, it seemed to suggest that there may be some kind of solution front or hydrothermal event that occurred here, and we may have something similar to a Uranium deposit in this area.

This plot shows some of the chemistry on the cuttings, and it shows the high Uranium value that was found in the cuttings.

SEM photo micrograph, essentially an almonite drain with some uranonite drains stuck in it. So we do have some Uranium mineralization in these rocks.

I'll summarize quickly. We believe we have compartmentalization of the flow systems in this area. And this has important implications for regional flow modelling. We may look at the distribution of contaminates south of Yucca Mountain. We think we see an increasing influence of the carbonate aquifer as we go west from Forty Mile Wash. Stable isotopes suggest effects of age, climate and elevation. That's pretty standard.

I didn't show any data, but there have been some moderately reducing zones found mainly in the deepest samples of some wells furthest west from Forty Mile Wash, and I just want to point out that although some moderately reducing zones have been found, you have to consider where these have been found and the location. These are deep and they're essentially fairly west where we think most of the flow from Forty Mile Wash is going. So this may have some effect on retardation of any contaminates from Yucca Mountain.

In the future, we're going to integrate more carefully the geochemical data with the geological and geophysical information. I need to get into geochemical modelling. We start sampling in a couple weeks and, again, hopefully we can get into some noble gas geochemistry later if the chemistry of the waters warrant it.

Carl wanted me to, or suggested I talk about the silica cap. Is there interest in that by the Board?

KNOPMAN: Very briefly, but if you can just run through it?

SHETTEL: Twelve years ago in a presentation to the Board, I suggested that there would be some hydrothermal effects from the hot repository. Obviously, this is the waste canister. This is a cross section of the drift. As the thermal pulse moves out from the drift, you have a dry-out zone, but you also have a zone of boiling where you're precipitating minerals, and then where the condensate condenses, you can have dissolution. You also have volcanic glass that may dissolve as well as silica polymorphs that may transform to quartz, and this creates porosity. This looks more like a cloud, but most of this has to occur in the fractures, because that's a predominate area of transport.

But the important question here is the spacing of the drifts. If the drifts are too close together, you can get cementation between them, and then the infiltration could collect here and you could get perched water. Later on when the cooling occurs, these cemented zones could fracture, and then you have the possibility for water coming into the drifts. I think that's all I want to say on that one.

And very quickly, since I thought they were abandoning the hot repository in favor of ventilation, but now I hear we're considering both, a little over a year ago, I did some modelling of geochemical consequences for ventilation of the repository, and this would be below boiling, and this is essentially again a cross section of the drift, vary the skin thickness here, area of infiltration, as well as the amount of infiltration.

And the bottom line here is that it's possible in just a few years to cement up the fractures that would bring water into the open area of the repository that would evaporate and cause some cooling effects. And if you plug up those fractures, then you couldn't rely on either evaporation of the water and your thermal effects calculation, essentially your cooling calculation, so that these models that run on ventilation for hundreds of years, or even tens of years, may not be realistic unless you consider some of the geochemical effects of plugging in fractures. That's all I want to say.

KNOPMAN: Thank you, Don. I'm sorry we couldn't give you more time there.

SHETTEL: That's okay.

KNOPMAN: Do we have any questions from Board members? I actually think we'll want to follow up with you on some of those results off line. There's a lot of material there.

SHETTEL: Yes, I'm trying to get all this data up on the Nye County site.

KNOPMAN: Right. And we appreciate getting that into the record. We'll just need to follow up on it.

SHETTEL: Actually, there is a much longer--I didn't point this out--but there is a much longer paper on this on our company website at that address you'll find at the bottom of your page.

KNOPMAN: Okay. We did get one question from the public. And hearing no questions right now from the Board, I'll ask this on behalf of someone in the audience.

Based upon the phenomenal press coverage of the initial drilling sample results and the absence of any coverage of the filtered data, will Nye County adjust their procedures for releasing data in order to preserve their credibility to provide unbiased early warning?

SHETTEL: That's a question more properly put to my higher-ups than me. I just report the numbers to the technical contacts of Nye County.

KNOPMAN: Okay. I encourage the individual who asked the question to follow up with other Nye County people then if they want to know the answer.

Okay, we're going to take a ten minute break now, and we're going to hold to that. Our session immediately thereafter is going to take some time, and we want to make sure we have plenty of it for questions, and have a public comment session.

(Whereupon, a recess was taken.)

KNOPMAN: Can we get started now?

Our last set of speakers for this afternoon are going to talk about some recent chlorine-36 studies and analyses, as well as some other isotopes.

We have two speakers. Bill Boyle will start things off and then turn it over to Marc Caffee. Bill is a senior policy advisor in the Office of Licensing and Regulatory Compliance, and Marc Caffee is with Lawrence Livermore Labs, is a research physicist.

Bill?

BOYLE: Thank you. And thank you all for being here. Marc and I will both speak, and I'll be brief and provide just an introduction and perhaps a wrap-up at the end.

KNOPMAN: Excuse me. Hold on one second, Bill.

BOYLE: Okay.

KNOPMAN: If you still have conversation, feel free to go outside and continue it.

BOYLE: I'll save most of the time for Marc's presentation of his results and any discussion of those results.

I assume most of the audience knows why the project has measured chlorine-36. But just in case, I'll give a non-expert synopsis.

Chlorine-36 is one of many naturally occurring radioisotopes used for age dating. Its abundance was changed by nuclear weapons testing in the 1950s, creating what's referred to as a bomb pulse, an increase in the amount of chlorine-36.

Measurements of chlorine-36 at Yucca Mountain have been interpreted to have this bomb pulse. These bomb pulse data are then used as evidence that there are fast flow paths in the unsaturated zone at Yucca Mountain. That's the synopsis, and now I'll briefly describe the project's measurements.

The project's original chlorine-36 measurements were made by Los Alamos National Laboratory. As you can see, Marc is at Livermore and Zell is with the United States Geological Survey. And their measurements are referred to even in this talk as the validation measurements. Now, why were these validation measurements made?

Well, a series of reports were written by the Geological Survey that seemed to describe a comprehensive history over geologic time for the unsaturated zone at Yucca Mountain. This history was based upon integration of many independent datasets. Not surprisingly, not every dataset that was used to develop the integrated history flanged up perfectly.

One of the datasets that did not flange up as well as other datasets is the chlorine-36 results from Los

Alamos. In discussions about why there might be this difference between the chlorine-36 dataset and the USGS history for the unsaturated zone, it was decided to follow a standard scientific practice and have an independent lab make measurements, which led to Livermore and USGS involvement.

The measurements are the subject of Marc's talk. I imagine at the end of Marc's presentation, a question will be what's the next step. But to keep the presentation in sequence, I'm going to turn it over to Marc now. But I'd like to reserve a couple minutes at the end to address what's the next step.

CAFFEE: First of all, I'd like to thank you for providing a forum to present these results.

KNOPMAN: Excuse me, Marc. You may need to move that up a little higher.

CAFFEE: Is that better?

KNOPMAN: Yes.

CAFFEE: Well, first of all, I'd like to mention that this is a true collaborative project between Livermore and the USGS. Without it, we couldn't have done it, as you'll see as I present the data.

The first thing I'd like to do, though, is just review a little bit about chlorine and chlorine-36. First is called Nuclear Chemistry of Chlorine. Chlorine comes in two stable isotopes, chlorine-35 and chlorine-37. Of these two, chlorine-35 is dominant. As far as the geochemistry of chlorine goes, it's a rather boring set of isotopic ratios. Any place you look in the earth or the terrestrial system or for that matter, on the moon or in meteorites, you don't see a whole lot of variation between the natural abundance of 35 to 37.

That can't be said, though, for chlorine-36, which is a natural occurring radioactive isotope of chlorine. It has a half life of 300,000 years, and it decays by beta decay to the noble gas, Argon 36. Now, the agent for the creation of chlorine-36 is both terrestrial and extra-terrestrial materials is energetic particles.

The source of these energetic particles, and you can see that this story goes all the way back and has an astro-physical connection, the source is high energy events in the Milky Way Galaxy, and this is a Hubbel space telescope picture and it shows an x-ray image of an expanding shock wave, and this is probably the site of the acceleration of those particles that ultimately create chlorine-36 that we measure in the terrestrial system.

So here we have the acceleration of protons to billions of electron volts. They traverse much of the galaxy to get to our solar system. They get to our solar system, they have to swim upstream against the solar wind.

The solar wind cuts off the low energy component of the galactic cosmic rays, gets to the earth, and than at the earth, the magnetosphere cuts off yet another component of the cosmic rays, and then finally we have protons impinging on the other layers of the atmosphere. These protons do several things. They, through a series of reactions that are very much like billiard ball reactions where you have the cue ball hitting the unmolested billiard balls in the center of the table that cause everything to go every way, you have reactions where the protons hit the argon in the atmosphere, and you can make chlorine-36 that way. But then you also have a tremendous secondary cascade of neutrons and other elementary particles penetrating the entire depth of the atmosphere, and indeed making it all the way to the surface of the earth.

So in the natural terrestrial system, the largest source of chlorine-36 is production in the atmosphere. This is exactly analogous to the production of carbon-14, which is one of the heavier used chronometers available to geochemists. This chlorine-36 is eventually either attached to aerosols or just rained out directly as rainwater, and it ends up on the surface of the earth.

Now, it's also possible for these neutrons to penetrate to the surface of the earth, and you can make chlorine-36, and you can make a whole host of other radioactivities in the upper couple of meters of the surface of the earth. And this happens at a rate of tens of atoms per gram of rock per year. So it's a very sparse process, but these products can all be measured with a technique called accelerator mass spectrometry.

In addition to that chlorine-36 that you make in the atmosphere and in the surface of the earth, all throughout the earth, anywhere there's uranium and chlorine, you also make subsurface produced chlorine-36. And this arises again from energetic particles. When uranium decays, when chlorine decays, you have neutrons, alpha particles, and these ultimately create through a process called neutron capture, chlorine-36. You have a neutron hitting a chlorine-35 atom. It just keeps the neutron, and you have chlorine-36.

In addition to these natural sources of chlorine-36, there are man made sources of chlorine-36, and the one that is of concern to us today is that chlorine-36 that was produced in nuclear weapons testing in the Pacific.

So here you have a tremendous source of neutrons. The neutrons are captured by the chlorine in the marine environment, through this gamma ray action. The whole basis gets kicked up into the atmosphere and it's recirculated throughout the entire northern hemisphere, and over a period of years, it just simply rains out onto the surface.

Here's a diagram of the atoms--the deposition of chlorine-36 in the dye free ice core. The dye free ice core is the ice core at Antarctica. And you can see that from about the early Fifties through the early Sixties, there was a tremendous increase in the deposition of chlorine-36. And this was true throughout the northern hemisphere and the southern hemisphere.

So if we want to measure chlorine-36 today, we're likely to have chlorine-36 produced by three different pathways. One of them is the bomb pulse chlorine-36, which I just mentioned. It's characterized by extremely high ratios of chlorine-36 to chlorine. Okay? And here I've arbitrarily said greater than 1000, but in fact in the ice core, it's greater than 10,000.

We also have that chlorine-36 that is in rainfall and precipitation, and that has a ratio of about 500 by 10 to the minus 15 in this particular area. And this ratio varies as a function of distance from marine environment.

And then, finally, we have the chlorine-36 that's produced in the subsurface from uranium and thorine decay, and depending on the concentration of uranium in the rock that we're measuring, this ratio can be anywhere from 20 to 50 by 10 to the minus 15.

So there's three likely sources of chlorine-36 in our samples. And so it may not be possible to uniquely go back and deconvolve any given isotopic ratio into the three possible in members, but what is possible is to look at the chlorine isotopic ratio and see if there are exceedingly high ratios. If there are exceedingly high ratios, then we know that there is bomb pulse chlorine-36 present.

So Bill gave an introduction here. The point of this study is to validate previous work done at Los Alamos. And so for this study, we decided to take a slightly different approach. We just started from ground zero, and did the whole thing, collected new samples. And the idea behind this was to not only measure chlorine-36, but also to measure tritium in all of this.

Our sampling was done a little bit differently from the Los Alamos sampling where they looked at features in collected samples. We went to the Sundance Fault, went on either side, and just collected a sample at regular intervals of five meters. We collected two inch cores, and the cores were drilled to a depth of four meters. So the deepest sample was reserved for the tritium measurements, and then the next slice up from the tritium measurement sample was reserved for the chlorine-36. So we're well away from the ESF wall where there's been all sorts of alteration taking place. And all samples were cataloged and stored at the sample management facility before they were shipped to Livermore.

Now, in concept, this experiment is very simple. All we want to do is measure the chlorine-36 to chloride ratio in all of these samples, nearly 50, of which we have completed around 25 to 30, and see if we have high chlorine-36 to chloride ratios. If we have those, we take the results as validating the previous results. If we don't see that, then we know that something is going on.

So to make this work happen expeditiously, and because the ratios are so high, and because they're not difficult to measure with an accelerator, we just devised a sample preparation method that was pretty simple.

The assumption that we make here is that since the bomb pulse, if it's present, is the last chloride to end up in this rock, it's probably going to be some of the first that comes back out, so a simple leaching process is what we used. And towards that end, we developed a process in which each sample was treated exactly the same. So each sample would be crushed, leached, and then have the exact same extraction chemistry performed on it.

In brief, the sample preparation is to crush the sample in a hydraulic press, sieve it, and then we select the sieve size fraction that is between 1 and 2 centimeters. This size was based on the idea that we wanted to maximize the amount of fractures that would be leached, and minimize the amount of chloride that's indigenous to the rock that would be released in the crushing.

Typically, from a 1 1/2 to 3 kilogram size fraction to start with, the yield into the 1 to 2 centimeter size fraction was about .7, or 70 per cent. This sample was then mixed with ultrapure water. It was put in a large container, and this container was then put in a rotating cylinder, and it was rotated for exactly seven hours. The choice of seven hours was based on some scoping work that we did that seemed to indicate that chlorine-36 was released up to six hours. The other reason for picking this is it's reproducible. Someone could come in in the morning, turn the agitator on, or mix the samples with water, turn the agitator on, and have it go for seven hours, and turn it off before they go home, so we don't have a situation where some samples have been leached for ten hours, some for 24 hours, some for over the weekend.

Then we take the water, and I hesitate to even call it water at this point, it looks more like mud, and we filter it and get it down to a clear solution that has been filtered to .45 microns. All this was done in accordance with technical implementing procedures that were developed for this work at Livermore.

Once we have clear water, it's not a difficult step to isolate the chloride out of this water. So after we removed some samples for archival purposes and had what we call a chlorine carrier, archived some more aliquots. We pumped the leachate through an anion resin which collects all anions. This concentrates the chlorine from four liters of water down to about 40 mls. of water. So we elute the fractions that contain the chloride, then we simply precipitate the chloride and silver chloride.

At this point, after quite a few more rinses and a few other steps just to increase the purity of the chloride, it's ready for accelerator mass spectrometry.

This is a cartoon of the Lawrence Livermore National Lab accelerator mass spectrometer. This facility has been in existence for almost ten years now. It's a multi-isotope facility. We've measured carbon, beryllium, voluminum, chlorine-36, calcium-41, iodine-129, and several other nuclides there.

Typically, we measure about 20,000 samples a year, and for chlorine, we measure about 1,000 chlorine samples a year. The way AMS works, AMS is a method by which you can measure small amounts of atoms, so it's not a cationic technique. We count the atoms that are characterized by isotopic ratios less than 10 to the minus 10. So a normal mass spectrometer can measure an isotopic ratio into the 10 to the minus 6, 10 to the minus 7 range. Beyond that, you start having all kinds of instrumental artifacts that preclude the measurement of a really low isotopic ratio.

The technique is based on the injection of a negative ion into an analyzing magnet, and then subsequently to that, into an old accelerator. It doesn't have to be old, but ours is old, and it's a Fifties vintage accelerator. The terminal voltage is anywhere up to 9 megavolts, and then the ion is stripped at the terminal. It's run in the 8 plus charge state, so we have almost 9 megavolts going in in a negative one charge state, 9 coming out in the 8 plus charge state. So when the chlorine comes out, it has in excess of 70 million electron volts. So it's not relativistic, but it's getting close.

We go around several analyzing magnets to reject other species that have the same rigidity or momentum to charge ratio, and we select--we reject everything that doesn't have the same velocity as the chlorine, and finally we measure the chlorine-36 in a DEDX detector. Chlorine-36 is stopped in an area of about a foot. It's in this area that we can separate further contaminants.

For example, sulfur-36 is a constant worry when you're measuring chlorine-36. There's no amount of mass analysis up here that will separate it. So we have to rely on good chemistry, and then separation in the DEDX detector to separate the chlorine-36 from the sulfur-36.

So these are the results, and these are the surprising results. Now, again, on the X axis, I have the location in meters in the ESF, and on the Y axis, I have the chlorine-36 to chloride ratio in units of 10 to the minus 15. And up here, is a rather arbitrary, but cutoff, for bomb pulse where we say if anything has a ratio of greater than 1200, and this was what was done in the previous work, we will say that there's evidence of the presence of bomb pulse chlorine-36.

This line indicates the range that we expect for present meteoric chlorine-36 to chloride ratios. And as you can see, all of our ratios, except for a couple, or one primarily, are below 200 by 10 to the minus 16. So there's a consistency here. There's some samples in this area that we have not yet measured, but we should have those measurements in the next month or so. But in general, all of these ratios are very low.

This gives you a comparison with the previous Los Alamos results, and here again, down here is a dash line representing 1200 by 10 to the minus 15. So there's many ratios that are higher than 1200 by 10 to the minus 15. In addition to that, there's a number that populate this region between 500 and 1200.

This just gives you an increased magnification of the Los Alamos results, and here along the Sundance Fault, you see ratios ranging anywhere from 500 up to 4000, and this is the area where we've sampled. And I will emphasize that to date, we have not seen the same thing.

So just to summarize the results, we've detected no evidence of bomb pulse chlorine-36 in the samples we've measured so far. So based on that, the chloride that has been extracted from the samples that we measured appears to be old. Okay? And the basis for that is that if we assume the meteoric input to be 500 by 10 to the minus 15, one way that you can drive it lower is through decay. So if decay is the process, then the chloride that we have sampled is old, and it's old of about the same age as the chlorine half life, chlorine-36 half life.

The other thing is that we do not observe any of these chlorine-36 ratios that reside in this region between 500 and 1000.

This is some rather old data, but it gives a picture, these are contours of the chlorine-36 to chloride ratio in Continental United States, and you can see that close to the ocean, we have ratios of 20 by 10 to the minus 14 where stable chloride dominates the ratio. As you move in and you are less influenced by the marine environment, you get radios that are higher, until in this area, you get 500 by 10 to the minus 15.

So whatever the mechanism for the elevated chlorine-36 ratios in the Los Alamos study, whether it's climate change, whether it's increased production rates, we don't see that effect in the samples that we've measured.

Okay, how robust are these data? What could go wrong? I'm working my way towards trying to come up with some sort of an explanation for this.

Now, we've also measured tritium, and these measurements were made at Florida State University, I believe, and in all the samples measured to date, there's less than 1 TU. And this line corresponds to 1 TU. Anything below 1 TU is below meaningful detection level. So, so far, we've not seen any evidence of bomb pulse tritium in these samples either.

Now, the lack of tritium does not mean that there couldn't be bomb pulse chlorine-36 there. So since the processes of transporting these two radionuclides are slightly different, it doesn't necessarily follow that we could say that this is a direct confirmation. But it's comforting that if there's no chlorine-36 in these samples, there's also no tritium.

Okay, continuing on this theme of how robust are these data, in terms of corrections to the data, any corrections done to these data are small. Blank corrections don't change the ultimate ratios any. As a matter of fact, corrections tend to lower, rather than raise, the final ratios. So there's very little in the way of ways to increase these ratios any.

Finally, when these samples were run, they were run with many other samples. When we run chlorine, we tend to run in groups of 64. There are 64 standard, secondary standards, blanks, and research samples all together. On this particular we had many samples from calcites from Paul Starks in Italy, and we've already run some of those samples, and we've already looked at the data on those samples and we know that they made perfect geologic sense. That's not to say that you can guarantee other results. However, there's no systematic problems that we've picked up with any of the measurements that we've made at the same time as the Yucca Mountain measurements.

What factors could account for the difference? And I guess the first thing that I should say is that even though we've completed many of the samples that constitute the validation set, we haven't finished yet. We may yet see it. It's possible that the next ten samples that are measured, all ten will come back with ratios of 2000 by 10 to the minus 15. I can't say that that hasn't happened. So I want to emphasize that our work has not proven, demonstrated or by any means the absence of chlorine-36.

So now we move to what could account for the difference. Since this was an independent study, I suppose it's not so surprising that there are differences. I'm a little surprised by the magnitude of the differences, but we did process these samples, the processing was done in a slightly different way from the Los Alamos process. So it's possible that we've selected phases, our sample processing has high graded phases that do not contain the bomb pulse chlorine-36, or that we simply haven't released those yet. Or it's possible just in the way that we did our sampling, every five meters, going on a program like that, that we just selected against sample locations that would be high graded with the bomb pulse chlorine-36.

So what do we do next? Well, I think there's several things that we need to do. One of the things we could do is we saved all of the dregs from our samples, we have the fine fractions yet, we have other sample yet, we could go through and extract the remaining chlorine-36 from these samples, and we could crush them finer, we could leach them more, we could do many things with them, and see if we find bomb pulse chlorine-36 in these samples.

I think, though, at this point, now that Los Alamos has done extensive work here and has a large measurement database, and we have a much smaller database, but they don't agree, it probably makes sense to start thinking about inter-laboratory comparisons in some fashion. This is not necessarily a simple matter, because the rock is a heterogeneous material, and obtaining a true aliquot is going to take some work, but I think that that's something we could do. We could process enough rock and we could share that rock. We could exchange leachate. We could do a number of things. And first of all, eliminate the possibility of any inter-laboratory biases.

And I think with that, I'll stop.

KNOPMAN: Bill, do you want to pick up now, or--okay, just identify yourself again.

BOYLE: Bill Boyle, DOE. Good international cooperation. So we don't have to keep switching back on the microphones, I just wanted to bring up the question I had posed earlier that people might ask now, what's the path forward, and Marc has identified some of them. But just to recap some of the other things that Marc mentioned, he's not even done testing his initial set of samples. But the most interested parties in these results have been in communication with each other, Zell Peterman and June Fabryka-Martin, and I think that the first step in the path forward is to continue the discussions, let Marc finish his results, and I'm sure as time goes by, a reasonable path forward will be found.

That's all I wanted to point out to people. Marc's most recent results are only a week old as of last Friday. So I don't think everybody has had a chance to digest all the results and differences.

KNOPMAN: Thank you. Before turning to Board questions, and I know we have several, I'd like maybe, if no one has an objection, to ask June Fabryka-Martin to come forward now, if you're willing, and just perhaps respond in brief and offer your insights so far on the results.

June is with Los Alamos National Lab, and conducted the initial studies of chlorine-36 in the ESF.

FABRYKA-MARTIN: I guess I can point out or make a points here while the crew here is moving things around. One is there are many differences between the way the validation study proceeded and how I proceeded, all the way from how the sampling sites were sited, for one thing. Where we bound bomb pulse chlorine-36 was almost always in locations that I call feature based, where we were actually looking at the wall. We could see what we were sampling. If it was a fracture, then we would collect our sample parallel to that fracture so we could maximize the amount of fracture surface we got.

In contrast, these holes for the systematic study were more systematic. Even though they were within a narrow range of a couple hundred meters, it was like every five meters through that interval wherever that five meter point would fall. And also think of the bore holes probably intersecting the fractures at right angles, so that the proportion of fracture surface that's exposed in any given sample is probably fairly small. That's one difference.

And also there are about three differences between Marc's processing method and mine that I wouldn't think would be important, but still, you know, it's probably significant we should make note of it. One is the way he does the extraction. I just throw my samples in a soup pot actually, and stir them. Then they're covered in between the stirring. That will be a minimum of 48 hours, but we don't get upset if we go over a long weekend or something either.

And then we monitor chloride/bromide ratios to make sure that we're not releasing excessive amounts of what you were calling the indigenous chloride, as well as having construction water contamination present.

We don't use anion exchange resin. I know that's caused problems with contamination in the past. I think that's been solved now in the past few years. Instead, when we get our four liters of leachate, we evaporate it to concentrate it, and then proceed from there.

And then, finally, when we measure the chlorine-36 to chloride, or rather, when the AMS facility measures it for us, they measure the ratio directly on the accelerator. Whereas, Marc measures chlorine-36 separately, and then combines that with a measurement of chloride concentration to get a ratio.

So none of those things, with the exception of the siting of the sample locations, I would not expect any of those things to cause as significant a difference as what Marc has seen. But even so, it's things that we have in the back of our mind and things that we discuss among ourselves.

The original intent was Los Alamos was planning to analyze on the order of 15 per cent of the validation bore hole samples. We didn't think it was worth the investment to do more than that, because we did not really expect to see very large differences between these two datasets. These are data I got back in last fall, and I haven't done anything since then, but we expect to get a whole slew of results over the next month and a half.

As you can see, the ratios we've been getting range from between about 500 up to about 940, which is right in keeping with what we've had before. And here, I've plotted them relative to our previous results. The samples that are in red are the ones that we did, and although none of them were the so-called unambiguous bomb pulse level, that means above 1200, they were nonetheless within the zone of variability that we were seeing throughout that part of the tunnel.

I guess I should explain some more of the different types of symbols here. The original samples, the ones that started causing all the furor, are the ones that are plotted either in white squares or black squares. The black squares are what I call systematic samples that basically we collected a sample every 200 meters originally, and then went to ever 100 meters as we got further into the tunnel. And as you can see, very few of them got very high, or what we would call unambiguous bomb pulse indicators.

And the ones that are open squares are ones that we call feature based where we were seeing what we were sampling, and that's where almost all the bomb pulse signals were seen.

The green squares are ones from the so-called north ramp and south ramp bore holes, where we were able to extract enough water by centrifuging the core to actually use that water, core water, to prepare samples for chlorine-36 analysis. That's the Cadillac approach, but it's rare to be able to extract that much water from this tight rock. And they were largely consistent, too.

Now, if you were to plot Marc's results on this same plot, they would be, let's see, that's 500, they would be down about here. So we have almost an order of magnitude difference between our sets, and we both feel the same way about it, I think. We're both pretty baffled because we both respect each other highly. We've been in this line of business for longer than either of us I think care to admit.

Now, one thing I would like to point out, and this is my last overhead here, is they keep on talking about it's the Los Alamos results, as though I personally am responsible for every sample. And two points I'd like to make here is I'm not the first PI on this project, for one thing. The first PI was, well, really Kurt Wolfsberg, if there's anyone in this room who remembers Kurt, and his daughter-in-law is my technician on this project. He really started it, and I don't even know how far back it went. And at that time, the samples were all prepared at

Hydro Geo Chem in Tucson. They were measured at the University of Rochester.

And then Kurt gradually turned over the project to Ted Norris, who was my immediate predecessor, who continued all the sample processing at Hydro Geo Chem. And even at Hydro Geo Chem, there was--neither I nor Ted really ever go in the lab, or went into the lab in Ted's case. It's all done, all the sample processing is pretty much done by technicians and people that they supervise. I really don't have much to do with it.

But the point I wanted to make here is that the lab supervisors, the people who do the analyses, have been probably about ten different people through the years. So what Ted found was bomb pulse in UZ one cuttings, bomb pulse in G tunnel, apparently associated with a fault. He was the one who came up with the first measurements of the in situ ratio in the tuff from Yucca Mountain, and also showed what the background ratio--showed bomb pulse profiles.

The point I want to make here is all I see when I took over the project is just filling in his initial outline. I don't see anything that's out of line with what he produced.

The other thing I want to say is we stayed with Hydro Geo Chem processing the samples at their site using different labs for the analyses up until Scott Wightman came over to Los Alamos in '94, and everything from '94 on has been processed at Los Alamos. And I even did an inter-lab comparison when I first came on board on this project involving Livermore with I think Marc, John Soloman, University of Rochester, and Purdue, and what we did was we sent them silver chloride, not raw samples to be processed, and that inter-lab comparison was acceptable. It wasn't stellar, but it was acceptable.

I think that ends all I wanted to say, was that it's just not one person that's produced all these results. It's a history of many people being involved.

KNOPMAN: Thank you, June. If you'll kind of stand by as questions arise, maybe you could kind of park yourself near that other microphone there?

Dick Parizek?

PARIZEK: Yes, Parizek, Board. I have slightly different questions. I didn't realize you'd be here and have a chance to also speak, because the first thing is maybe you're locked up somewhere and not allowed to give a dissenting opinion. But obviously there's something very important here. Either the news is good, or the news is bad. And it's good in the sense of it's old water. But maybe it's the old machine that can only find old water. It's a question of whether the techniques are such that it's less sensitive than what you're doing. So I'd kind of like to know about that. If he came to your lab and used your procedure and you went to his lab and used his procedure, would you find his results and he'd find your results? There's a way to find out if it's a lab methodology.

FABRYKA-MARTIN: Well, actually, you do your own work, don't you?

CAFFEE: All the chemistry is done in our chem lab at Livermore, and the measurements are done at the accelerator at Livermore. So it's all done internally to Livermore.

PARIZEK: Yeah. Really, there's got to be some explanation. I mean, there are possibilities his spacing at five meters is so coarse, and not too many samples to date and, therefore, statistically he missed it, because even in your case, you show a number of no hits as you kind of wander down, except a lot of his are too low compared to your non-hits.

FABRYKA-MARTIN: Right. I would design a project a lot differently, even from this stage forward. But this is a G.S. Livermore project, but I think Marc's suggestion of taking a so-called internal standard as a first step makes a lot of sense. I mean, that would make sense in any case.

PARIZEK: Yes. And there's no way you can contaminate--maybe your lab is sloppy and you got yours all contaminated.

FABRYKA-MARTIN: We work in something that's not quite class 100 lab facilities, but it's a fairly new building, it's kept under positive pressure from the lab to the hallway, from the hallway to the outdoors, filtered air that comes in. And our blank I guess is really convinces us. We do swipes that show that it's clean, and then when we do our sweeps, we always have a top that has a little bit of DI water in it that we process along with all the samples that gets evaporated just like the samples, and then gets sent off for analysis just like the samples, and it's never been high.

CAFFEE: I guess I would just say that I don't really see how contamination would be a good explanation for these results. From the point of view of our results, since they're low, you can't take chlorine-36 out. Okay? It would be hard to have something that going into our lab had a ratio of 2000 by 10 to the minus 15, and then you take out the chlorine-36. Now, you could dilute it with a massive amount of de-chloride, but we would pick that up when we do the high end chromatography. So we would know if that happens, and that's never happened in any sampling. So I really think that there's probably something real here.

FABRYKA-MARTIN: That's why I made that point about work being done at Hydro Geo Chem in Tucson for so many years. There's a completely different lab, completely different people, and yet consistent results, even though it wasn't ESF, it's still they did the shallow neutron hole samples that we were seeing the bomb pulse in a lot of those.

PARIZEK: So now one suggestion is to go to a neutral site, such as Ice Core. You have done Ice Core? You said those are very high concentrations?

CAFFEE: Thousands of them.

PARIZEK: Yeah. And so you find in Ice Core, high values. And, June, have you done Ice Cores?

FABRYKA-MARTIN: No.

PARIZEK: So you don't know whether you could find his chlorine-36 in Ice Cores or not? I'm just trying to look for some way--

CAFFEE: I know what you're saying. While it's true with the Ice Core, the Ice Cores, as it turns out, is where we learned to do the chemistry of the anion chemistry, because you have to melt so much ice core that it's just not desirable or feasible to do an evaporation process to get chlorine-36 out.

FABRYKA-MARTIN: Right.

CAFFEE: So that's where we learned to do the anion process. But I think what needs to be done probably, and what's eventually going to shed some light on this, is understanding the systematic differences in the sampling protocol, and maybe the differences in what goes on in our labs in terms of the leaching process. You know, I just can't help but believe that we're accessing different reservoirs, if you will, of chlorine in these things, and that accounts for the difference.

PARIZEK: It's extremely critical to get this right, because the public confidence in the program would be taking a hit here, I think, because it would look like--

FABRYKA-MARTIN: Maybe in either case, however it turns out. I don't know.

PARIZEK: If you work it out right, figure out why the difference, then maybe the credibility, everybody would be happy. But to throw it away to say, well, all of that data is not valid, would create a real problem right now. I mean, you really have to figure out how to proceed with this. The path forward guidelines I think we ought to hear, or some day we ought to hear how you visualize doing this.

KNOPMAN: Jerry, did you have a comment?

COHON: Yes, following up on this last remark by Marc with regard to protocol, and a simple minded question. Do you use the same size fractions? And if you don't, could that matter?

FABRYKA-MARTIN: We use what's between 2 millimeters and about 2 centimeters. So we sieve--we break it down and then sieve it to get rid of the stuff left smaller than 2 millimeters, and that's mostly to minimize the amount of indigenous chloride that we get in the samples.

COHON: So they have a lot more fines than you do?

CAFFEE: We go from 1 to 2 centimeters.

COHON: Could that make a difference?

CAFFEE: That was one of the bullets up there I think, is we go back and look at our fines and see if there's something in there.

COHON: How could that make a difference? I mean, how could that explain it? What's the physical explanation?

CAFFEE: Well, right off hand, if you asked me before we had made the measurements would that make a difference, I would have said no, that won't make a difference. Now that we've made the measurements and we're looking for some explanation, I'm not quite so confident in that. But I still don't have a good explanation for it, but you know, maybe later on, I could give you some tip of the tongue ideas, or some things that come to mind. But I wouldn't want to speculate on that.

KNOPMAN: Norm Christensen?

CHRISTENSEN: Christensen, Board. I think clearly there's either an issue with sampling or an issue with analytical approaches, and I have every bit of confidence that these can be sorted out. And I agree with Dick that I think that they're very important.

I'm sitting here thinking about why do we care so much about this? And, of course, we care because this really tells us a lot about how fast fast flow is. It is, in fact, we would expect where we see this to be very feature oriented, and I wonder in looking to the future of however this gets resolved, if we really shouldn't be focused on issues of pattern here. At least from my standpoint, that's why this becomes really, really critical. We know there are fast flows and fractures. What these data seem to tell us, at least when we were looking at them associated with the fractures, is this stuff really zips through the mountain in those fast flows. And so having that resolved, I think that is the most important piece of information from these data, if I'm not mistaken. I'd like to throw that out and have anybody comment on that.

KNOPMAN: Mark, June, Bill, any one of you?

CAFFEE: Well, I guess what I would say is if we try to--what you're really trying to do is reconcile both datasets. Let's just imagine that we tried to do that, and we said that in these features that June sampled, there is indeed bomb pulse chlorine-36 coming down there, and it's getting down there very rapidly. Now, that would be--you then looked at some of our measurements where we didn't do anything that was feature based, we'd say that that signature is imprinted on some sort of a matrix where you had very old, very non-exchangeable chlorine. Now, that may be totally wrong to think that way. We have to do more measurements to try to understand that. But I can't help but believe that if that isn't the case, that's important. That's an important thing, I suspect, for the mountain.

CHRISTENSEN: I guess what I'm suggesting is I would like the--it is the feature based chlorine-36 that is most interesting in the sense that that's where we expect stuff to move quickly. And we have no data at the moment of whether that can be reproduced, because it hasn't been sampled, number one, and it hasn't been analyzed. There's only been really one measurement that's been focused around the features where we expect to see fast flow.

So we have the one set of data, but these data, in some sense, aren't necessarily relevant to the fast flow, and that's--so what I'm asking is if we're going to have a validation dataset, it seems to me that we really want at least part of that to be focused on the sampling procedures that focus on the issue of why chlorine-36 is important, and that's because it zips through the mountain.

FABRYKA-MARTIN: When we first got these results, one of the first things I did was bring a modeler into the project, Andy Wolfsberg actually, another Wolfsberg also related to Kurt, his son, because I was wondering, well, are these physically possible. There's no way we could consider or conceive of large buckets of water making it down in a little parcel without being diluted out. And so I gave him an input function for chlorine-36, and he used Alan Flint's infiltration map and hydrolic parameter sets that were accepted by the project, and found that you could indeed account for the ratios we've seen, but it could be explained by just very small proportions, like on the order of 1 per cent or less of the water making it, or the chlorine-36 making it down to the depth that we measured.

So it doesn't necessarily mean large volumes. It just means that there's a, you know, at least a small part that survives that pathway. And so it has major implications about matrix fracture interactions.

What makes it a little bit difficult is it's not really a--it shouldn't have any correlation with flux necessarily. A high flux region still would not have bomb pulse because, you know, it all has to do with probably along a connected fracture pathway all the way from the surface, which is really fairly rare except around faults.

We also have done a statistical analysis of the distribution of our signals relative to distance from a fault, and so forth, at least we did a first cut.

CHRISTENSEN: I realize the flux is sort of a different issue here altogether. But the important thing here was that we could have very rapid travel times for molecules of water from the surface down to that level.

FABRYKA-MARTIN: Right.

CHRISTENSEN: Now, the fact that the background data for these two datasets is different is, of course important, and I'm not trying to play down the differences, but rather to say that the validation that I would have liked to have seen was one that did replicate the sampling, and particularly focused on the question of fast flow.

CAFFEE: I guess in answer to that, I think that that would be a good thing to do now, but when we started talking about this, one of the things that we wanted to do was try to do something that would be systematic, reproducible, and also a study in which we could measure the tritium.

So just going to the surface was one which would not allow us to measure the tritium. We needed to have a core to go back and measure the tritium. So at the time that this study was planned, that was something that we considered important, so we wanted to get back away from the tunnel wall.

FABRYKA-MARTIN: They did also plan to measure I-129 and tried technetium-99, and there is radium/uranium disequilibrium was planned, too, by the Survey.

CAFFEE: And this is part of this where do we go from here. But chlorine-36 is not the only tracer that we could measure. We could measure iodine-129 on the accelerator also.

Now, a year ago when we started this, we were rebuilding beamline to measure iodine-129, and so that was something that we had made some measurements and that we were undergoing an increasing capability to be able to make those measurements better. And it's just been in the last two months that that beamline is reconstructed and ready to measure iodine-129.

So in the meantime, we've also developed chemistries for extracting iodine-129, so this is something that some years ago, was not feasible, but now because of advancements required by the programs, we could do. So if you had a situation where you measured chlorine-36 and iodine-129, both produced by bombs, then you'd feel pretty good about it.

KNOPMAN: Okay. We have questions from John Arendt and Alberto and Paul Craig, and we have about five minutes left before our public comment period begins. We're going to try to stick with that. John?

ARENDT: Arendt, Board. I guess there's several problems, and all of it has to do with procedures. The first is do you have a sampling procedure? I notice that Marc had indicated all the procedures that you used in the chlorine-36 analyses. Do you have a sampling procedure? Do you have a sampling preparation procedure? Do you have an analytical procedure? You need all three of those.

I noted that on the viewgraph that you had, you indicated all of the people that had been involved in chlorine analyses. That doesn't tell me very much, unless I knew what each of the procedures that each of these people had used.

FABRYKA-MARTIN: DP-92, DP-89, DP-88 and DP-95. Of course we had procedures.

ARENDT: Yeah, what are these?

FABRYKA-MARTIN: We use a notebook procedure for sample collection, but we have criteria laid out, and that's how the samples were identified in the field. Okay? Because we had a structural geologist, so we have a sampling procedure, but it's very general.

ARENDT: That may be the problem. They're general.

FABRYKA-MARTIN: I found bomb pulse. He didn't. What do you want in that--

ARENDT: Have you looked at each other's procedures?

FABRYKA-MARTIN: Marc based his procedures on mine. He took mine and edited them to fit his.

CAFFEE: The procedures are not dramatically different really.

ARENDT: They're not?

CAFFEE: Except that we do have the USGS developed procedures for the coring, so we do have procedures for the coring. The procedure for precipitating chloride is one that every lab in the world uses, basically the same procedure. The only really discernable difference is that we use an anion on the resin to concentrate the chloride, and we developed the procedure for that.

ARENDT: But the technicians have these procedures.

CAFFEE: Yes. For us, there's a flow chart that's much more detailed than what I showed you in the slides, but every box has a check point on it, and every box has to be done before the next thing is done.

ARENDT: Well, based on what I've heard here, I would look at those four things, the sampling technique, the sample preparation, and the analyses, and I'd look at the procedures in detail, and I would make sure that they were being followed. You might even exchange samples.

CAFFEE: I think that's a good suggestion. I guess all I would say is that I believe that June probably followed her procedures, and I know that we followed our procedures, but we'll check it out.

ARENDT: But it might be a problem with your procedures. Have you examined each other's procedures?

FABRYKA-MARTIN: I sent Marc my procedures, and that's how he--he edited mine in order to come up with his.

KNOPMAN: Alberto?

SAGÜÉS: Something very quick. This is a gross difference in results. If you look at the bar counts, let alone the presumed pulse areas, you're getting results which are ten times less than yours. Why not get in a sample and split it and check it in both laboratories. I guess that John mentioned this, but I don't quite--normally, one doesn't look for all these really sophisticated explanations until the very gross and obvious test is done. Why haven't--

FABRYKA-MARTIN: That was my suggestion when we first started talking about validation studies, and the comment that I got is they didn't want my handprints or fingerprints on any part of this. They wanted to start from scratch.

SAGÜÉS: Yeah, but doing this is like going to a patient and extracting two different blood samples and sending them to different laboratories. Right there, one may already be wrong; right? Because maybe the sampling procedures--so why not take in one sample and split it, and that would solve it in what I presume would be a reasonably short amount of time. And then if the things come the same, then we have to wonder about all the other things. But until that simple check is done, which is a common sense thing to do, and we do it all the time in our experiments whenever we have an unusual analytical procedure, I think that all this other speculation may be put to rest perhaps.

FABRYKA-MARTIN: I agree totally.

KNOPMAN: Okay. Bill?

BOYLE: Yeah, just a quick point. I want to remind people that Marc's results are a week old as of last Friday, and I said there would be a lot of discussions for the paths forward and I appreciate this that, you know, people are giving insights like splitting core. A path forward will be found and hopefully it will be simpler rather than more complex.

CAFFEE: I did want to make a comment on the intercalibration. We've split meteorites, lunar samples, granites, you name it. All of these things have been measured at a variety of laboratories. We've done more laboratory inter-comparisons than you can shake a stick at. Okay? And most of these have been done with Livermore and Zurich, and more recently, other laboratories. So for most of the isotopic systems that we deal with, we've done many intercalibrations.

Now, it's true enough that we haven't done a Yucca Mountain calibration, and that was one of the things that I think is obvious that we have to get a sample that's like that mountain and try to see if we can make an aliquot and measure it and get the same thing.

SAGÜÉS: Right. But it looks like we have a problem here between two different laboratories. That would be the most obvious explanation as to this issue. I don't think that simple measurements are going to help very much with different samples. There is a huge difference in here. This is a big difference. The problem is going to be something at the fairly gross level, at least those would be the very first things to look at, I would think.

KNOPMAN: Okay. One last question from Paul Craig, and then we're going to wrap up this part of the meeting and go to the public comment.

CRAIG: Okay. Well, we're at the stage where everything has been said, but not everybody has said it.

This is obviously important for everybody, and what I'm curious about is the process that you set up for going the next step, the timing of that process, and most importantly, the resources and the priority that is given to resolving this by the Program, which I hope are exceedingly high. But I'd like to hear that confirmed.

BOYLE: Bill Boyle again, DOE. I don't think that process and timeline has been laid out yet, given the recency of the results. I mean, even the PIs are still trying to figure out some of the differences.

CRAIG: Well, let me then give you the last part of it. Is DOE committed to putting in the resources to get this resolved expeditiously?

BOYLE: We'll see. That has to be discussed. I would like to see it resolved, but I don't have DOE written across my shirt here. I won't commit the Department.

FABRYKA-MARTIN: Do they want AMRs, or do they want this resolved?

CRAIG: This probably should not go through the QA process right away.

KNOPMAN: Okay. On that note, here we go. Russ?

DYER: Let me add a little to that. This is Russ Dyer, the project manager at Yucca Mountain.

Since it was pretty much my idea to do this to start with, I want to see it through. Yes, we have an interesting discrepancy. I'd like to understand what the reason for the discrepancy is. It may be that we're seeing a little bit of fast paths, and maybe some background. But we would like to understand what's going on here.

KNOPMAN: Okay. I want to thank Marc and Bill Boyle and June for participating in this last hour discussion. It was extremely illuminating for us, and we'll look forward to following up at our next Board meeting.

COHON: Thank you, Debra. We turn now to our second public comment period. We have three people signed up, Judy Treichel, Earl Dixon and Sally Devlin.

We'll start with Judy Treichel. Judy?

TREICHEL: First, I'd like to tell the Board just how thrilled I am and appreciative that you brought the visitors here from Sweden. It was--while I guess it may be a little cruel to those of us who are in the public advocacy game to hear from someone who has a veto in his back pocket, but I think it was wonderful, and I would like to be assured that all of you heard so carefully what they said, and also the wonderful paper that they produced that really spells it out exactly the way it is.

I think the argument that we've just heard, or the discussion, was fascinating, as well as some of the presentations that you received in which things change so fast and almost overnight in this process, and yet we're going a hundred miles an hour on a schedule toward a site recommendation considerations report. And when discussions like the one that just got done are still going on, and there are a lot of other things like the chart that Rich Craun showed, showing how many problems get solved if you wait some time, and I don't think necessarily you want to do that waiting in the desert next to Yucca Mountain. But there are so many unanswered questions, and it's all in the name of flexibility, and flexibility kind of sounds to me like they're making a lot of guesses and they want to be able to keep guessing just as long as they can, because that works pretty well and it allows you to keep changing things as you go along.

On the SRCR, as it was explained, it's to show compliance with all of the rules. None of those rules exist right now, but yet this thing is going down the track as fast as it can towards that SRCR. We don't have any guidelines. We don't have the licensing rule. We don't have the EPA standard, although I understand that's coming fairly soon. But to show compliance with things that don't even exist when, by contrast, if you look at Sweden, and maybe some other countries, first they came up with the procedure that they were going to use, who played what role, how it all worked together, how you get people working together, how you get either volunteerism or certainly acceptance, and then you decide what method you want to use. You look at a whole lot of them.

And what this program has is a site. Well, and it also has a schedule along the wall. And everything is being made to fit that. And for the guidelines, 960, and for the licensing rule, 63, I attended all the hearings. People were furious. People were outraged. People said absolutely not. They absolutely disagreed with those proposals, and now we see, when we see the presentations, that everything is coming together so that we comply with those proposals, which aren't final, which nobody can really count on. And I think it's just so frustrating, and I know that people are getting angry. I get more angry calls now than I ever did before, and I think that's sad. It's frustration. There is nothing people can do. So I think you're going to see more of that.

The fact that we try to assume, or that people on the project try to assume that they know all of the answers better than future people might know them is really quite arrogant. And I think it just provides sort of silly justification for continuing to play ball with the nuclear industry.

The only final thing that I would say is that I was sort of taken aback when Dr. Itkin said that he thought the world was looking to the U.S. for leadership. I think when it comes to the nuclear waste game, I'd like to look a lot of other places first before I wound up looking at this one. This one has a lot to learn. They don't have much to teach.

Thank you.

COHON: Judy, could I ask you a question?

TREICHEL: Oh, yeah.

COHON: In commenting on Rick Craun's presentation and your observation that problems get solved by waiting, you made the remark, which might have been an offhand remark, about I'm not sure you want to do the waiting in the desert at Yucca Mountain.

TREICHEL: That's right.

COHON: Is there any technical things you had in mind in saying that, or was it you just don't want it there?

TREICHEL: Well, I think it's a terrible mistake. I think if this program slowed down the schedule where by, God, we're getting that SRCR out in November, I mean, to be even considering, it's a considerations report, to be considering a site recommendation with the sorts of discussions that you're having now is crazy. So it may not play out.

COHON: No, I got that. I got that point.

TREICHEL: Why would you transport all of this stuff to here?

COHON: Okay. Well, let me--suppose you had a plan that said for the reasons that were discussed, because you want to create a cold repository, you're going to store it on the surface, you're going to stage it for some decades, now I can understand why you would oppose that. But I was wondering if there's any technical basis as to why you wouldn't want it--why we should not want it to be sitting in the desert at Yucca Mountain on the surface.

TREICHEL: Well, I think seismicity is a problem for something that's sitting here on the surface, and I think once again, you don't have any sort of acceptance by the public here, and they already feel that they've been ambushed, so they're probably not likely to go with this, and it's going to be plagued with problems.

COHON: Okay. I just wanted to know what was behind it. TREICHEL: Okay, thanks.

COHON: Thanks. Earl Dixon?

DIXON: My name is Earl Dixon. I was here in January and I talked about what, Board Members? A related issue to Yucca Mountain, but it's up the hill a little ways. Let's look at some things in common. Tritium, chlorine-36, plutonium transport on colloids, regional model, boundary conditions for the site scale model, perhaps the

4 millirem per year groundwater standard. Are we getting thermally warm? The Test Site. Does this Board consider that contaminant hydrogeologic information important to this project?

COHON: Yes.

DIXON: Yes? Then we're getting somewhere. We've seen how--I mean, Yucca Mountain was not even looking at plutonium transport on colloids, were they, until Tiebow, Bennum, all of a sudden we found this stuff 5,000 feet away in 25 years.

What I'm trying to get at here, Ladies and Gentlemen, is we've got an existing problem in this state. Sometimes I'm confused as to why the state doesn't bring it up when it should. It seems like it's okay to put up with the existing contamination, and yet we're focused on the future. Nye County has an early warning drilling program, which technically is very sharp, doing good work, but the hazard is not in the ground yet.

We have a large volume of existing contamination that ultimately discharges to Death Valley, follows some of the same flow paths that Yucca Mountain contaminants would follow, yet we don't have an early warning drilling program for that project. We don't know the speed, the velocity, the contaminants of concern. Tritium is not the only one out there. It has the highest inventory, but it's not the most hazardous. Strontium, plutonium, neptunium, they rank pretty high when you start looking at the effective dose.

So the point I would like to make to the Technical Review Board is is it possible you could look into that body of information up the hill, or the project and where it's going, to benefit this one? We could learn things from that project about radioactive migration. Things have been in the groundwater a long time. Your program is in the future. Even Nye County said that--or one of the commissioners said that the NTS is more of a problem than Yucca Mountain. But there seems to be an absence of activity on that one, except for the Department of Energy.

Why is the NTS not on the superfund list? Does anybody know? It's not supposed to be. It might jeopardize Yucca Mountain. Is that the reason? We don't know. Can't get the document.

That's all I'm saying, is just that we have a problem already in Nevada. We don't understand it very well. We need to collect information for that one at the same time. It's all flowing toward Beatty, Oasis Valley, Amargosa, and if we're going to bring in Yucca Mountain and we're going to do it right, then we need that information from NTS.

So I'll be back next time and we'll have the same question. I appreciate you logging it in the notes, but this is something I'm going to keep working on, because we're not doing a good job. We've been waiting for 25 years for the answer on the NTS, and we still don't have it. We're spending a lot of money on that groundwater issue, and we still don't understand it.

Thank you.

COHON: Thank you, Mr. Dixon. Let me just clarify one thing, though, you're always welcome to come back and keep talking about the Test Site, the Board's sole focus is on Yucca Mountain and the waste management system related to spent fuel and high level nuclear waste.

Our interest in the Test Site as Boards is in what it can teach us about Yucca Mountain. So that's specifically why we should be interested and why DOE should be, as well. Now, the problem of the Test Site is not our job. That's not to say--I'm not trying to minimize its importance or to say what should be done, that's just not within our Congressional mandate.

Mrs. Devlin, you're up.

DEVLIN: Again, I want to say thank you all for coming to Pahrump. I hope next time that you come it won't take you three years, and I sincerely appreciate everybody who came undressed, and I hope the next time you come, everybody will be undressed and that you really believe what a lovely, relaxed community that we are.

And talking about being undressed, not 28 miles from here, if you go down 372, is the Tacopah Hot Springs where you don't have to wear any clothes. The men's and the women's spas are 90 degrees and 104 degrees, and they're quite separate and they are lovely. So whatever you will do, we have something to offer you.

Again, thank you, and I hope you come again very soon.

I have to make my comments on certain things, and that is, again, I didn't hear anything about my bugs. Now, how can you talk water without my bugs? But nobody talked about my bugs and you know they're terribly important. You can't talk about canisterization because my bugs love the canisters. I've been sending all these articles on how my bugs love metal, they love dirt, they love everything, and as you know, 24 colleges are doing work on them. And so I think that is very major and a great deletion. The colloids again the same thing.

And I understand your mandate, Jared, on Yucca Mountain being separate from the Test Site, but one of the things my enemy, because he's going to write the report to the Congress, so I've always called Abe my enemy, and yet he gives me all the ammunition that I needed for the

Congress, and here it is in black and white, and I'm so proud of you and thank you. A repository should not present public health risks unacceptable to current generations. And you heard the word current, which just emphasizes my point that you're going to kill us all, because it's only going to be current. And when you're with a semanticist like me, you'd better be very current. Excuse the pun.

Anyway, what I'm saying is I am going to look to you because, again, as Earl said, we who live in the shadow of Yucca Mountain and NTS object thoroughly to this dichotomy between your thing and their thing. All their poisons are going to come together at Yucca Mountain, and we don't have a medical facility. And I think now that Abe has given me the words and the verbiage, it is most important that we put something together on this medical horrendous situation that is so dangerous.

The other thing that I have to say is, again, on the canisterization, the costs are much to low. If you're going to order 20,000 canisters, which is the number for the amount of waste, your numbers are much higher. If the overpacks are 9 million, or 8 billion, whatever they said, those costs of the canisters will be much higher.

The other thing is how do you get the canisters and the stuff into them? Remember at the last meeting, I showed you that Fleur Daniel report where they gave them an extra billion dollars. They don't know how to do it. They don't know how to get the rods out of the water. They're all corroded. They're all falling apart, and they've got a major problem.

I don't think money solves health problems, or technical problems and this sort of thing, and I think it's terribly dangerous.

The last thing I have to say is I'm going to ask your help on this medical problem, Abe, and I hope that you will do something along with Dr. Cohon, and let's get something going here. I have presented to the state everything from Iowa. Dr. Bullen opened my eyes and my brain about virtual medicine. You're talking an area where the Congress just passed a bill that if you're not within 300 miles of a hospital, you don't qualify for health care. Well, we're 60 miles from the hospital, or 80 miles, or 120 miles, or 200 miles, or more now, and we don't qualify. And yet as you know, we're snowed in, flooded in, forest fired in, and so forth, so we have nothing medical here.

Our critical care unit was a political thing. It's open from 7:00 until 7:00 during the week, and sometimes during Saturday and the rest, we have nothing. And where is all this stuff going through here? Where are the people going to be? I keep telling you the number 120 to 150,000. You've begun to really visualize the growth here.

Our County Commissioners have allocated 59,000 parcels, just two and a half times that number, and you have what our population will be. We are 364 square miles. The Test Site is 1,375 square miles. How far are we from it? Where is the nearest medical facility? There is nothing at Nellis. There is nothing at the Tonopah Test Range. There is nothing at the Test Site, and there is nothing in Nye County, and we are the largest county in the nation.

So, again, I have my appeal to you. I want to communicate. Everybody can have my card and we'll talk, because something has got to be done on this. Nationwide, you're talking 43 states you're going to kill with this stuff, so let's get going here, guys. I'm getting older. Remember, I'm dead. When you're over 70, you don't count with DOE.

COHON: DOE will kill me, but I just gave Mrs. Devlin Page 20 of Mark Peters report. He didn't talk about bugs, but he talked about fungi.

I want to thank all of the speakers for their excellent presentations today, and I think they were very good presentations.

I'm sorry, I should ask. Were there any other members of the public who care to address the meeting?

(No response.)

COHON: Again, let me thank the speakers, all of them. You all did a wonderful job. I want to thank especially our visitors from Sweden for travelling all this way, and for giving us the benefit of their insights, which were very valuable for all of us.

I think that this is an interesting time for the program. When has that not ever been true? But it gets ever more interesting I think as we approach some significant deadlines and milestones. We see a lot of focus, some very interesting presentations with regard to design and the design process, and a very promising opportunity I think for linkage now to the science with regard to uncertainty and its characterization and how that can link to the design process. It will be interesting to see what DOE does with this possibility.

The science of course marches on, and we saw this very interesting controversy about chlorine-36, and the resolution of that will be important indeed I think, and the other science moves on as well.

I want to thank our colleagues who organized this meeting, especially Carl Di Bella, who was the technical staff and the lead on this. He did a wonderful job of packing, I think, all that could possibly be packed into a one day meeting, and doing it just right in terms of the pacing and the combination of things that we talked about.

And I want to thank the two Lindas for their great job of staffing this and making it happen in Pahrump, which is a wonderful place to be, but can present logistical challenges, shall we say. No?

DEVLIN: No.

COHON: Now that we have two traffic lights.

DEVLIN: We have almost four lanes all the way, and we are not as far as Beatty.

COHON: I just want you to know on the way back from lunch, we missed both lights. This is a Pahrump traffic jam.

It's always a pleasure to be here in Pahrump. Thank you, Mrs. Devlin, for being here to welcome us and for participating. We look forward to seeing you at our next meeting in August in Carson City. We're looking forward to that.

We are adjourned. Thank you.

(Whereupon, at 5:45 p.m., the meeting was adjourned.)