UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

BOARD MEETING

Tuesday, November 8, 2005

Renaissance Las Vegas Hotel
Las Vegas, Nevada

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. William Howard Arnold
Dr. Thure Cerling
Dr. David Duquette
Dr. B. John Garrick, Chair, NWTRB
Dr. George M. Hornberger
Dr. Andrew Kadak
Dr. Ronald Latanision
Dr. Ali Mosleh
Dr. Henry Petroski

SENIOR PROFESSIONAL STAFF

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Dr. Daniel Fehringer
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INDEX

1. The drip shield will be constructed from titanium grade 7 (shell), titanium grad 24 or 29 (struts and bulkheads), and alloy 22 (feet). What data are there under Yucca Mountain-relevant conditions for the generalized corrosion, localized corrosion, stress corrosion cracking, and hydrogen embrittlement of the titanium materials and galvanic corrosion at or near the titanium/alloy 22 interface?

2. Under what circumstances or combination of events can low-temperature creep of the drip shield's titanium alloys be a problem?

3. What data and/or prototypes for the drip shields and their emplacement devices are planned to be available in 2006? 2007? 2008?

INDEX
(Continued)

P R O C E E D I N G S

8:05 a.m.

GARRICK: Good morning. Welcome to our meeting. My name is John Garrick. I'm Chairman of the Nuclear Waste Technical Review Board, and the Board is pleased to be back in Las Vegas to hold this public meeting. We will meet here all day today, and a half day tomorrow.

I'd like to begin right off by introducing the Board members. That's something we do at each of our Board meetings, because we often have new people in the audience. And, as all of you have heard before, and know about us, the Board members are part-timers. They all have other activities, for which we have responsibilities. And, in my case, I'm a consultant on the application mainly of the risk sciences to different technologies, like space and chemical, marine and the nuclear fields. My fields of interest and study and application are risk assessment and nuclear engineering.

Now, as I introduce the Board members, I would ask each Board member to raise his hand. The first one I'd like to introduce is Mark Abkowitz. Mark is Professor of Civil Engineering and Management Technology at Vanderbilt

University, and is Director of the Vanderbilt Center for Environmental Management Sciences. Dr. Abkowitz chairs the Board's Panel on the Waste Management System.

Howard Arnold. Howard is a consultant in the nuclear industry, having previously served in several senior management positions, including vice-president of the Westinghouse Hanford Company, and president of Louisiana Energy Services.

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

David Duquette. David is Department Head and Professor of Materials Engineering at Rensselaer Polytechnic Institute in Troy, New York. His expertise is in physical, chemical, and mechanical properties of metals and alloys, with special emphasis on environmental interactions. And, his current research interests include studies of cyclic deformation behavior as affected by environment and temperatures, basic corrosion studies, and stress-corrosion cracking.

George Hornberger. George is the Ernest H. Ern Professor of Environmental Sciences and Associate Dean for Sciences at the University of Virginia. His research interests include catchment hydrology, hydrochemistry, and transportation of colloids in geological media. Dr. Hornberger chairs the Board's Panel on the Natural System.

Andrew Kadak. Andy is Professor of the Practice in the Nuclear Engineering Department of Massachusetts Institute of Technology. His research interests include the development of advanced reactors, space nuclear power systems, improved technology-neutral licensing standards for advanced reactors, and operations and management issues of existing nuclear power plants.

Ron Latanision. Ron recently retired from his position as Professor at MIT to pursue a senior position with an engineering and scientific consulting firm known as Exponent. Ron retains a position as Emeritus Professor at MIT. His areas of expertise include materials processing and corrosion of metals and other materials in different aqueous environments. He chairs the Board's Panel on the Engineered System.

Ali Mosleh. Ali is Professor and Director of the Reliability Engineering Program in the Mechanical Engineering Department at the University of Maryland. He has performed risk and safety assessments, reliability analyses, and decision analyses for the nuclear, chemical and aerospace industries. Dr. Mosleh chairs the Board's Panel on Repository System Performance and Integration.

Henry Petroski. Henry is the Alexander S. Vesic Professor of Civil Engineering and Professor of History at Duke University. His current interests are in the areas of failure analysis and design theory. Ongoing projects include the use of case histories to understand the role of human error and failure in engineering design, as well as models for inventions and evolution in engineering design.

We also have with us today Dr. William Murphy as a consultant. Dr. Murphy is a geologist, hydrogeologist, geochemist, and has worked with the Board many times, and we are pleased to have him.

Since the Board last met, one of its members, Dr. Daryle Busch, resigned because his full-time responsibilities at the University of Kansas proved to be more demanding than he had anticipated when he accepted President Bush's appointment to the Board. We will all miss his technical expertise, his good humor, and his common sense.

At the beginning of each meeting, the Chairman reads the following statement for the record, so that everybody is clear about the conduct of our meeting, and what you're hearing, and the significance of what you're hearing.

Board meetings are spontaneous by design. Those of you who have attended Board meetings before know that the Board members speak frankly, and openly voice their personal opinions. But, I want to stress that when the Board members speak extemporaneously, they are speaking on their own behalf, not on behalf of the Board. When a Board position is articulated, we will do our best to make that known.

I would like to set the stage for today's meeting. In doing so, I need to point out that the comments that follow are being made on behalf of the entire Board. As many of you know, about nine months have passed since the Board last met in a public session. This hiatus is unusual. Typically, only four to five months separate the Board's public meetings. It might be valuable, therefore, for me to describe what the Board has been doing since it met here in Las Vegas last February.

Shortly after that meeting, in fact, the following month, the Board engaged in an intensive strategic planning exercise to develop approaches for evaluating the technical basis of the work the DOE is doing to support an application for a license to construct a repository at Yucca Mountain. Out of that exercise came a small set of critical technical issues that the Board believed warranted special attention. In no particular order, these priority issues are:

1. The capability of natural barriers to isolate radionuclides;

2. The postclosure risk associated with the proposed repository;

3. DOE's approach to thermal management;

4. The range of possible near-field environments that the engineered barrier system might encounter;

5. A comparison of preclosure and postclosure human exposure to radiation;

6. DOE's plans for the transportation and waste management systems;

7. DOE's design and operation of surface and subsurface facilities.

Once these priorities were established, it became very clear that the Board needed to interact with DOE in a concerted manner that permitted in-depth technical exploration of the issues.

Toward that end, small contingents of Board members and staff held eight fact-finding meetings with the DOE and its contractors between March and September. At those meetings, DOE presented the results of a number of ongoing scientific investigations and analyses. As it is obligated to do under the Nuclear Waste Policy Amendments Act of 1987, DOE presented many preliminary results that were still in draft form. These fact-finding meetings were productive and enabled the Board to engage in detailed and lengthy technical discussions necessary to understand many of the fundamental methods of analyses employed by DOE. Importantly, these fact-finding meetings were undertaken in part to increase the likelihood that the Board's public sessions would have more technical substance. I do, in fact, want to emphasize that the Board intends to hold three to four public meetings, mostly in Nevada, over the next twelve months.

In addition to the meetings with the DOE, for two days last month, several Board members and staff held separate talks with representatives of railroads, truckers, cask manufacturers, transportation logistics providers, and nuclear utilities. The purpose of these meetings was to gather first-hand information from key stakeholders in the waste management system.

One further note. Some of the presentations by DOE later today and tomorrow will touch on key issues that arose in the fact-finding meetings. It is important for DOE to get as much information as possible on to the public record. Toward that end, we have listed in the agenda a series of questions that we have asked DOE to address.

Now, I would like to get a little more specific and share with you some of the Board's views about a few technical issues. The Board commends DOE for continuing to refine and update its model for flow and transport in the unsaturated zone. No evidence has been developed that would call into question DOE's long-held view that flow in the unsaturated zone is dominated by fractures and faults, although the Board continues to question DOE's understanding and assessment of the unsaturated zone beneath the proposed repository horizon in retarding and retaining radionuclides.

DOE continues to investigate the issue of deliquescence-induced localized corrosion at high temperatures. The U.S. Geological Survey has conducted studies to characterize dust in the Yucca Mountain Exploratory Studies Facility's tunnel, and additional efforts are being made by Sandia to characterize atmospheric dust in the vicinity of Yucca Mountain. The Board continues to believe that determining the characteristics and amount of dust that would settle on waste packages and obtaining experimental information--emphasis is on experimental information--on localized corrosion should be a high priority, given DOE's current high-temperature repository design. DOE will be making a presentation on localized corrosion today. The Board intends to probe this integration issue at that time.

DOE has developed a model for localized corrosion of the waste package, caused either by deliquescent salts or by seepage waters. They will be talking about this model shortly. Suffice it to say for now, the Board continues to have concerns about the technical basis underlying the model, including whether the model is consistent with the data that already has been reported.

Many technical issues raised by the Board in the past two years still do not appear to be resolved. For example, the understanding of colloid-facilitated radionuclide transport remains limited, as does that regarding secondary mineralization and co-precipitation of radionuclides. Further, there continue to be questions about the chemistry of the groundwater that could significantly impact how radionuclides are transported and retained.

The design of the proposed repository's surface facility was the subject of a DOE public statement about two weeks ago. The technical defensibility of the previous design has been a subject of much public comment and discussion, including at a NRC/DOE meeting several months ago. The Board looks forward to learning the details of what DOE now intends to do. Here the Board's off-stated concern about the need for improved communication between DOE and the nuclear industry bears repeating. It is important for DOE to obtain the results of the industry's experience with analogous facilities.

At these public meetings, you have heard the Board make frequent reference to the Yucca Mountain Project taking a "systems" approach. The Board uses this term in two ways. First, there is the waste management system. This system consists of a number of elements, which collectively must implement a range of functions: accepting waste at a utility or DOE defense-complex sites; handling, transporting, processing, and storing the waste; and finally, emplacing the waste underground. Second, there is the repository system, which is composed of both natural and engineered elements. Because the elements of both systems are tightly coupled, the assessment of the behavior and performance of one element may strongly depend on or affect the behavior and performance of others. The Board believes that it would be a mistake to attempt to develop either system without recognizing and accommodating these interdependencies.

DOE has developed a Total System Model as a tool to understand waste management system performance (waste receipt to waste emplacement). The Board suspects that the Total System Model, not to be confused with the Total Systems Performance Assessment, may prove to be valuable in analyzing a wide variety of preclosure scenarios. A DOE presentation tomorrow will provide additional details about this model.

Now, you recall me mentioning earlier that thermal management of the repository heat load is a priority issue of the Board. The thermal properties of spent nuclear fuel dictate surface and subsurface facility design. DOE has previously indicated its choice of key thermal criteria--the 11.8 kilowatt per waste package heat load and the 1.45 kilowatt per meter line load. The Board has still not heard a good technical justification for these parameters. It continues to encourage DOE to carry out trade-off and value-engineering studies to better inform DOE's thermal management decisions.

Finally, I would like to make a comment or two about the importance to the Board of achieving a fundamental understanding of the proposed repository performance. Scientists and engineers are typically cautious in advancing claims. They usually prefer to wait until as much evidence as possible has accumulated before committing to a particular position. To borrow from the language that the EPA used in its recently proposed Yucca Mountain standard--this natural tendency is re-enforced when those individuals know that their claims might be challenged in a formal regulatory process.

The Board appreciates the fact that the DOE is in the midst of preparing a license application for its proposed repository system. Not surprisingly, DOE is motivated to advance a licensing case whose main--and possibly sole--objective is to demonstrate compliance with the applicable regulations via an intensely legalistic process. Consequently, when faced with gaps in understanding, "bounding" or conservative approaches are often--but not always--adopted. Examples of this abound, including how the DOE models the temperature dependence of generalized corrosion rates, sorption in the saturated zone, and the containment capability of some parts of the Engineered Barrier System.

The Board remains concerned that by adopting a conservative compliance-focused approach, DOE discounts the importance of letting the public and the general scientific and technical community know what its experts believe are the intrinsic capabilities of the repository design. The Board believes that carrying out realistic performance assessments, perhaps in parallel with its efforts to develop a compliance case, establishes a "baseline" for measuring how "conservative" or "non-conservative" the DOE licensing case might be. To conduct these realistic analyses, scientists and engineers should be asked to give their best assessment of performance-critical parameters. Responding convincingly to that request may require increased understanding of the repository system. And, although some assumptions may be required, they too will need to be well justified if this best assessment is to be carried out credibly.

That such realistic performance assessments have not been conducted to date, compromises the quantification of the performance of the site and it fails to provide a reference for measuring reasonable levels of conservatism. Having more definitive information on the adequacy of the natural system, for example, and the levels of conservatism involved may well provide policy-makers with important and relevant information.

For a variety of reasons then, the Board reiterates its view that fundamental understanding is important and encourages DOE to fill in areas where significant gaps in such understanding exist.

DOE will be making a presentation tomorrow on the conservatisms--and non-conservatisms--contained in its performance assessment. And, that talk will allow the Board to explore how increases in fundamental understanding might provide significant dividends.

Now, let me say a few words about our meeting. We were to begin with a presentation by Dr. Mike Ryan, the current Chairman of the Nuclear Regulatory Commission's Advisory Committee on Nuclear Waste. Unfortunately, Dr. Ryan has taken ill with a bad cold that compromises his speaking abilities, and we are going to postpone his presentation to a later meeting. And, so to prove to me that he was ill, he called me and tried desperately to talk, not very successfully, and convinced me that he indeed was ill. So, our first speaker will be Betsy Forinash of the Environmental Protection Agency. As you know, many of you, Ms. Forinash was the lead official developing the recently published draft revisions to the EPA's Yucca Mountain Environmental Standard. She will be describing what the EPA proposed and why. Next, we will hear from Victor Gilinsky, who will present the State of Nevada's views on the proposed EPA Standard. Dr. Gilinsky was among the first group of NRC Commissioners appointed by

President Ford in 1976.

Over the course of today and tomorrow, the DOE and its contractors will be making six presentations. In addition to the three I briefly mentioned, John Arthur will give an overview of recent developments at the Yucca Mountain Project; Mark Peters will provide a science update; Mike Anderson, Jerry Gordon, and Mark Board will talk about the drip shield installation and degradation. There will be a discussion of localized corrosion of the waste packages by Charles Bryan and Gabriel Ilevbare. Tomorrow's meeting will conclude with a talk on behalf of the State of Nevada by Frank Kendorski. Mr. Kendorski will discuss tunnel stability issues. And, I've already mentioned that there will be a discussion of the conservatisms in the performance assessment, and that will be by Abe Van Luik and Bob Andrews.

Okay, two pieces of business before we start the presentations. First, I'd like to ask all of you to please take a few seconds, I'd better do this myself, to confirm that your cell phones and pagers are off, or switched to the silent model. Second, as always, there will be opportunities for public comment and questions at the end of the meeting today and tomorrow. If you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance to the room. Of course, written copies of any extended remarks can be submitted, and will be made part of the meeting record.

Some have asked about questioning during the course of the presentations. Our preference for that would be for you to write down your question when you think of it, and I agree that there are often questions that are thought of in the course of the presentations, and make those questions available to the back room, and we will see to it that we'll cover as many of those as we can. And, give those written comments to our support staff, Linda Coultry and Davonya Barnes, at the sign-in table.

With the preliminaries now out of the way, we will begin our presentations--almost. Given that our first presentation will not be made, I think in view of the fact that many of the people that come and go here do so on the basis of our schedule, so we want to stick as closely as we can to that schedule, and since our first speaker is not here, I think what we're going to do is call a recess until approximately 9:00 a.m., and that will put us back on our appointed schedule.

So, thank you very much, and we'll reconvene in approximately a half an hour.

(Whereupon, a recess was taken.)

GARRICK: I wonder if we could come to order, please.

Our first speaker will be Betsy Forinash, who is the Director of EPA's Yucca Mountain Program. She has worked in EPA's Radiation Programs for many years, about 15 years, addressing issues, including contaminated sites, radon, and radioactive waste disposal. She has been very much involved with the Standards work, and holds degrees in Civil and Environmental Engineering from Duke University, with an emphasis in radiation sites, and, Betsy, we're glad to have you with us today.

FORINASH: Good morning. It may be appropriate after all that I start off some of the discussions today as a first stop, given that EPA's establishment of the Safety Standards is a key first step in the overall process for evaluating Yucca Mountain's performance, and beginning a licensing evaluation.

I think most of you here are pretty well informed and familiar with the Yucca Mountain issues, so you're very likely to be aware that we have, in fact, proposed changes to our Yucca Mountain regulations. What I'd like to do today is to just give you some background on EPA's role, as a refresher, what we had previously said about Yucca Mountain's Safety Standards, and then an overview of the changes that we're currently proposing, and I'll look forward to the next steps in our process, and finalizing these standards.

I'm sorry. (Pause.)

Well, maybe what I'll do, I think that we're working on getting the electronic version up, but there are hard copies I know in the back. So, I'll work on the assumption that many of you have a hard copy of my presentation, and then perhaps the electronic slide version can catch up with us as we go along.

Would you prefer that I wait? Okay, then let me just move ahead, based on the written version. A quick reminder of the role that the Nuclear Waste Policy Act defines for the federal agencies involved in Yucca Mountain.

EPA's role is to establish the public health and environmental protection standards for high-level radioactive waste disposal sites overall. NRC would license the repository, and oversee its operation, whereas, DOE has authority for construction, and operation of the repository.

The Energy Policy Act of 1992 modified these traditional roles slightly for Yucca Mountain, requiring that EPA develop site specific standards for Yucca Mountain in particular.

In addition, and very relevant to our purpose today, is that it also required EPA to establish a contract with the National Academy of Sciences to obtain technical recommendations on the basis for standards at Yucca Mountain. And, it required that EPA's standards be based upon and consistent with the findings and recommendations of the National Academy of Sciences study.

Based on this mandate, EPA first issued standards for Yucca Mountain in 2001. It covered the time period during storage and management, and as well, after closure of the repository and long-term permanent disposal.

The disposal standards which applied after closure of the repository extended for a time frame of 10,000 years, and required compliance with doses estimated to the reasonably maximally exposed individual, both in cases of undisturbed performance and in the case of potential human intrusion into the disposal system. We also established separate standards for protection of groundwater for the 10,000 year period, which incorporated the same dose and concentration limits for radionuclides that the agency currently applies for drinking water.

In addition to establishing dose limits for compliance over 10,000 years, we also required DOE to continue those calculations beyond 10,000 years, out to the time of peak dose, but we did not require that the doses calculated after 10,000 years be compared to any explicit time limit, or, in fact, that they be considered in any explicit way during the licensing process.

The agency was challenged in court by a number of parties on a number of different aspects of our standards. That case was eventually consolidated with legal challenges to Yucca Mountain related actions, as well, by the Department of Energy and the Nuclear Regulatory Commission, and oral arguments were heard at the beginning of 2004.

In July 2004, the DC Circuit issued its decision. It upheld all the challenges to all agencies and to all aspects of EPA's standards, with one exception. It found that the 10,000 year compliance period we had established in our disposal standards, on its own, was not based upon and consistent with the recommendations of the National Academy study.

What the National Academy had said in its 1995 report was, "We believe there is no scientific basis for limiting the time period to 10,000 years, or any other value. And, we recommend that compliance assessment be conducted for the time when the greatest risk occurs," with the proviso that that be, "within the limits of geologic stability at the site."

In response to that court decision, EPA is proposing to extend the compliance period for the Yucca Mountain Standards to cover the time period beyond 10,000 years, out to 1 million years, which is the NAS's estimated time limit for geologic stability in the vicinity of Yucca Mountain.

And, I want to emphasize as context for what we're discussing, that no other rules in the U.S. for any risks have ever attempted to regulate for such a long period of time. EPA's existing standards for Yucca Mountain, and the standards that we apply at the Waste Isolation Pilot Plant for long-term radioactive waste disposal of transuranic waste cover a period of 10,000 years, which is the longest period that prior to this proposal has ever been addressed, and that still is obviously orders of magnitude less than we're talking about here for this proposal.

In light of the fact that we really don't have national precedence to look at in this situation, we really have two linchpins that we have looked to for models of approaches we might take. We've gone back and looked very carefully at the National Academy's study for guidance on how they think approaches might reasonably be implemented for handling such long time frames. We also have looked very carefully at international regulatory approaches and guidance on radioactive waste disposal, because there are many nations across the world that, in fact, are having to tackle these same issues as they look at how to deal with their own national waste disposal issues.

In developing our proposed rule, we were guided primarily by our legislatively mandated role to protect human health and safety. We also wanted to develop an approach that obviously was responsive to the court decision and the National Academy of Sciences recommendations more directly, also recognizes the inherent scientific limitations in projecting performance and uncertainties that you need to recognize for time periods out to a million years.

We also recognize that our standards are going to be used by the Nuclear Regulatory Commission in a licensing process, so we want to establish conditions that are understandable, clearly understandable, both to NRC, DOE and the public, so that there are clear expectations that are established for how and when compliance is demonstrated.

And, then, finally, I want to note that our proposal is focused on those portions of the standards that were affected by the court's decision. As I said, we were challenged on multiple aspects of our standards, and, with the exception of the compliance time frame, those standards were upheld against the challenges. So, for example, we are not making changes to the definition of the reasonably maximally exposed individual or to the groundwater standards, which were both challenged and upheld in the court's decision.

As an overview of the proposal, I want to emphasize first that our proposal maintains all of the protections from our 2001 rule for the first 10,000 years. We propose added protection by extending the standards to cover the time period for up to 1 million years in the future, and, as well, require that DOE must extend its performance assessment calculations to demonstrate compliance over that time frame. And, that approach is consistent, we think, with the international models and expert technical recommendations, both nationally and internationally, on this topic. And, I'll now describe in a little bit more detail how we approached the two different time frames.

First, our proposal maintains all the protections of the 2001 rule, which means that we retain a dose limit of 15 millirems per year for the first 10,000 years after disposal, which includes the time when the radioactivity is at its highest. And, for this length of time, the RMEI is protected at the level of the most stringent regulations that are in effect in the U.S. today.

We maintain this 15 millirem limit for the longest time period that we felt was scientifically justified. In 2001, when we issued our rule, we discuss extensively why we thought it was unjustified to extend the application of this limit beyond 10,000 years, and those reasons still hold true.

Importantly, by retaining the 15 millirem limit for the first 10,000 years, we ensure that people living near Yucca Mountain in the future will have the same level of protection provided to those who will be living near the Waste Isolation Pilot Plant, and if there are any other disposal systems that might be developed in the future.

The separate groundwater standards, including the drinking water limits that we use today, are retained without change for the first 10,000 years, but not extended. And, again, I can't emphasize enough the context that we're looking at here. I think, you know, when we talk about 10,000 years, people have become a little bit immune to what it really means, because it's been said so many times. We certainly hear people say that because we're not extending 15 millirems beyond 10,000 years, we're not providing long-term protection.

And, I think we would take exception to the claim that 10,000 years is not a very long time. It's twice as long as recorded human history. I think the oldest example that most people can think of that they conceive of ancient structures like the pyramids and stonehinge, are at most about half as long back in history as 10,000 years would be. So, it's very important to keep the time frames that we're talking about in perspective.

We do propose to extend the standards beyond 10,000 years to cover the time period out to a million years. For this time frame, we propose a dose limit of 350 millirems per year. And, again, to provide some perspective, a million years is 25,000 generations into the future.

For these time frames, we've set a dose limit that ensures that total radiation exposure to the RMEI will be no higher than natural levels people already live with routinely in other parts of the U.S.

The million year time frame includes the time that, as NAS recommended, when the highest radiation risks are expected to occur from the site, and I'm sure you all are probably more familiar with this than most, but DOE's projections at this point show that peak dose would be expected to occur several hundred thousand years into the future. The 350 millirem per year dose limit would cover all transport and exposure pathways, including groundwater.

We also provide some specification on how DOE needs to conduct the performance assessment analyses covering the full million year time frame. Just as for 10,000 years, DOE needs to conduct a probabilistic assessment to consider how Yucca Mountain would behave under a wide variety of conditions, and consideration of both natural processes and disruptive events that could affect the containment capability of the system.

We retain the probability threshold that we had established for the first 10,000 years, which is that DOE must consider any potentially disruptive events that have a probability of occurrence greater than one in 100 million per year.

In establishing the features, events and processes that need to be considered, we think that events and processes that are important for the first 10,000 years may continue to be important for a significant time after that, so DOE needs to continue to consider those over the long time frame. And, in addition, there are several disruptive events and processes that we think could be particularly important over the long time frame that we specify DOE needs to examine in particular. And, these are seismic events, igneous events, climate change, and corrosion processes.

DOE needs to examine the probability and effect of these events and show that they can meet the dose limits, even taking into account the possibility of these events occurring.

We have several other aspects that affect how DOE considers the dose limits. First, we're requiring DOE to use updated scientific factors in calculating the dose. This is a replacement of ICRP 26/30 with the ICRP 60/72. In comparing against our dose limits, we require that DOE--we propose to require, that is, that DOE use the median value from among the dose estimates that it generates. This is a departure from the 10,000 year time frame where we will continue to have DOE use the mean in establishing compliance with the dose limit.

And, our purpose in looking at the median rather than the mean over the longer term is that we want to judge Yucca Mountain's safety using the most likely performance of the disposal system. We're concerned about having high doses from very unlikely events drive the compliance assessment. So, in this case, over the longer time frame, and in light of the greater uncertainties and the nature of uncertainties we're looking at, we believe that the median is a better indicator, and it will be less affected by outliers.

The proposed rule was published in August of this year. We are currently in the midst of a public comment period that began upon publication of the proposed rule. The comment period is scheduled to close on November 21st. We have held hearings in several locations in Nevada, and also in Washington, D.C. during the comment period. And, I think the first time, as well, rather than simply a panel hearing comments, we provided some opportunity for question and answer and dialogue, which I think was constructive in helping people understand our perspective, if not agreeing with it altogether.

Once the comment period closes, we will do an analysis of the comments to determine whether we need to do any additional technical work, how our approach might need to be modified to respond to the comments. So, until we look at those comments, we don't have a schedule for finalizing the proposal. But, I do urge any of you that are interested in this topic to visit our web page. You can view the full text of the proposal and our preamble, which discusses our reasoning and the sources that we look to in much greater detail. You can submit comments either in writing, on line through the web page if you're interested, and all that information is on our website. So, I would encourage you to visit that.

And, now, I'll take questions if you have any.

GARRICK: Very good. Thank you very much. Okay, we'll have some questions from the Board.

And, to kind of lead it off, in your review of background information for establishing the new standard, I noticed of course that you sought all of the important documentation on that, including the National Academy of Sciences report. But, I was wondering, because these things are so subject to interpretation, and there's so many different spins that you can put on any given result, did you have direct contact with the committee members in this last go around, realizing full well that the committee has long since been dissolved, and is out of business?

But, at the same time, it occurs to me that there's great benefit in talking to the scientists involved in writing the report, since this report has become so fundamental in what you're trying to do. And, I say that because in talking to some of the committee members, you get different impressions about what was meant by different conclusions in the report. Did you make any direct inquiries with members of that committee?

FORINASH: No, is the short answer to that. I think the fact that the committee has been dissolved is one big challenge of doing that. Another big challenge to doing that is we're really bound by what the report says. And, we have done some interpretation of that report on our own, and sort of with input from the committee in 2001 when we determined that having a 10,000 year compliance period when less could be considered to be consistent with the intent of the committee. And, clearly, the court didn't see that as being particularly relevant. So, I think in this case, we were really focused on trying to make sure that while we understood some of the spirit of it, we were very carefully to be sure that we're complying with the language that's in the report.

GARRICK: Dave?

DUQUETTE: Duquette, Board.

If you'd turn to your Slide Number 7, please? Perhaps just a minor comment, but your bottom bullet says your proposal focuses on those elements of our rule that were affected by the court decision. Courts have a tendency, in my opinion, to interpret things very narrowly. Is EPA proposing to only stay within the court decision? That is, are you letting the court make the decisions for EPA? Perhaps I should rephrase my question. Perhaps it's just the way you've expressed your bullet item. But, the fact of the matter is that I would think that EPA would look beyond just what the court had decided, and that you weren't being restricted in your studies or your analyses or your focus on just something that was put forward by the court.

FORINASH: I mean, yes, we did look beyond simply the court decision. Clearly, we did, because otherwise, we might just have taken the approach of changing nothing but the time frame, and leaving other aspects of the standard unchanged. And, we didn't do that. We extended the time frame, but we chose to apply a different dose limit to the longer time frame, and to specify some aspects of how DOE ought to approach its performance assessment with its longer time frame.

So, we did look at things that we felt were linked to the extension of the compliance time frame, and needed to be adjusted in light of that. And, in our analysis, we said, well, for example, you don't have more information about the evolution of, you know, exposure scenarios and demographic changes, and such, beyond 10,000 years than you do before 10,000 years, so, the reasoning that we used to establish the RMEI for 10,000 years, we think holds true beyond that time.

For the groundwater standards, that was something that we chose to do that the National Academy of Sciences did not address at all in its recommendations, except to say that they thought it was unnecessary to have separate groundwater standards. And, so, in regards--we ought to maintain what we had established as consistent and comparable to the protections provided, but for the longer time frame, our pathway dose limit would be adequate. Does that help?

DUQUETTE: Yes, it does. Thank you.

GARRICK: Ron, and then Howard and Ali and Thure.

LATANISION: Latanision, Board.

Slide 12, please. I'm interested in the very last item on this slide. DOE must show Yucca Mountain can safely contain waste. What is the practical implication, from your point of view, what metric or by what means will that part of your proposal be implemented?

FORINASH: By showing that they can meet the dose limit.

LATANISION: But what's the metric, or who's the judge of what mechanism will the EPA be looking for in order to make such a judgment?

FORINASH: Well, EPA doesn't make the judgment about whether or not DOE demonstrated compliance. We have basic parameters of what types of events need to be included in the performance assessment, establish the dose limit, say that you are--the median, depending on the time frame for judging compliance with the limit, and then DOE needs to build its safety case, and the Nuclear Regulatory Commission ultimately will be the one who determines whether the performance assessment and license application--

LATANISION: That was really my question. So, you would be looking to external sources for some guidance on that issue?

FORINASH: The NRC implements our rules.

LATANISION: Yes.

FORINASH: That's how Congress has laid out the agency.

LATANISION: One final question. On Slide 14, I'm curious about the scheduling of the last bullet, once again. As a practical matter, if you were the DOE and you don't have an indication of a schedule for a final ruling, yet you do want to submit a licensing application, how would you proceed? I mean, this seems to put the DOE at a tremendous disadvantage. Should they proceed? What would be your recommendation?

FORINASH: You'll have to get DOE to speak to that. My understanding is that the latest public statement, that they themselves have not declared a schedule for submitting a license application. But, I can't speak--

LATANISION: All right. Well, maybe I'll ask someone from DOE when the moment arises. Thank you.

GARRICK: Howard?

ARNOLD: Arnold, Board.

I'm interested on 13, the use of the median, you stated it's to avoid undue influence from statistical outliers, as I understand it. Do you have a feel for the practical difference that gives in terms of, perhaps, percentage between the median and the mean?

FORINASH: We haven't done explicit calculations yet. In looking at some of the performance assessment calculation, what it says in evaluating what statistical measure we should use for judging compliance, what we were seeing was the use of the mean could be as high, in some cases, as the 80th, or even above, percentile. And, that was something that we didn't think was a reasonable test, especially over these very long time frames, where you really have no way of verifying, for example, you know, what the range of climate conditions are that might occur that could affect the disposal system, or reasonably taking into account biosphere changes and lifestyle changes. So, we really think that something at the 50th percentile, I mean, that's where you want to be.

The State of Nevada may be talking more about that, In our public hearings, I think they've done some calculations. I don't know what the precise basis for that is, but they estimate that the mean would be about--the mean doses are about three times higher than the median doses.

ARNOLD: Thank you.

GARRICK: All right, Ali?

MOSLEH: Mosleh, Board.

And, my comment is along the same lines regarding the use of the mean versus median. I'm wondering if actually this was reviewed and discussed by decision analysts, people who look at the consistency of the use of being--switching to the median, especially when the argument is the impact of uncertainty, where you look at the uncertainties that are significant, the mean value is viewed normally as a natural summary of uncertainties, and often, the basis for positions that are based on point estimates.

When you go to median, that really calls for consideration of the entire distribution, because median really means something with the distributor quantity, more so than the mean, which provides a summary. So, I'm wondering if decision analysts really looked into this.

FORINASH: We did. I mean, what we were doing was taking a bigger picture view of what we wanted to accomplish with compliance over this very long time frame. And, we did get international models or a number of different approaches that are taken. In some cases, for example, they go to more considering, the French and the Belgium, for example, have put out some documents where they look at best estimate calculations, and essentially move away from probabilistic calculations over the very long time frame. We looked at whether that might be some way of indicating, you know, providing a more robust indicator, rather than having debates about how details of the distribution might affect the calculations.

We didn't think that moving away from probabilistic analysis was a good idea. But, we also think that using a 90th percentile indicator when you're talking about--it's also not a reasonable indicator of performance. That really is indicating that you're letting more extreme scenarios drive compliance. So, we wanted something that was a relatively robust indicator that would not be significantly moved by one or two results where you have a combination of very unlikely events. But, perhaps in our assessment, the median was--I'm not sure that that accomplished that. Those two specific issues, I think that you would want to point out, we'd be happy to have comments.

MOSLEH: Did you consider using median also for the first 10,000?

FORINASH: We did consider it. There's some discussion in our 2001 rule about whether that would be appropriate. In the WIPP regulations, we actually have a requirement that says that you use the mean or the median, whichever is higher. In the 2001 rule, we decided that the mean would be expected to be higher than the median, and in the instance of the 10,000 year scenario where your bounding analyses are more easily bounded and quantified, we thought that that was appropriate.

We also think it's appropriate from a policy perspective to maintain that comparability between the WIPP regulations and the Yucca Mountain regulations for that

10,000 year time line.

GARRICK: Thure?

CERLING: Cerling, Board.

I'm just wondering if you could comment on the comparisons to other countries, the international comparisons for both the 10,000 and the million year time frame?

FORINASH: Certainly. We looked across a wide variety of programs or statements from the IAEA, from the Nuclear Energy Agency, the OCD, from ICRP, and statements they've made in various contexts. I would just say that it's hard to draw precise parallels between different countries that purchase, the disclaimer up front, because every country has nuances in how they account for their own legal sector, or legislative mandates, cultural. However, there are some patterns that pretty clearly emerge when you look across the recommendations from international groups, and national programs that have established their own regulations.

The first is that they're trying to balance these competing roles of the fact that you ought to provide long-term protection for future generations because of the long-term risks, if you're talking about long lived radioactive waste disposal.

The second consideration is a wide-spread recognition that there are scientific limits on the meaning that you can attach to quantitative assessments that are done over these extremely long time frames. So, in every case, there is a need to balance those sort of competing objectives.

By and large, the patterns that you see emerge as first, most of the nations established regulations cover ultimately a very long time frame that is on the order of about one to 10 million years. However, they have a subset of time within that overall time frame in which they choose to apply strict dose limits. It ranges from about a thousand years, some countries are a little bit looser with their terminology, they say several thousand years, but from about a thousand years to maybe 100,000 years. And, the dose limits that are applied during that time frame range from about 10 to 30 millirem a year. So, our 15 millirem is very much in line with those.

On that time period from a few thousand years out to about a million years, there's a number of different approaches that are taken, but they aim to recognize that quantitative assessment over that time ought to be given less weight in the consideration of the safety case. So, in some cases, there are no dose limits that are applied over that time frame, and there are sort of qualitative descriptions of how you ought to consider it, like no dramatic degradation in performance. There are some cases where the dose limits, the limits are retained, but not as strict limits. They're maintained as objectives with other arguments, like natural analogues, and such, being given more weight over the long time frames.

Also, during that time frame, there's often a change in the parameters of the performance assessment, so that you go to an expected case analysis and begin to eliminate consideration of more disruptive events. So, for us, that would be the equivalent, say, of saying that DOE ought to look at Yucca Mountain's performance without considering earthquakes or volcanoes beyond 10,000 years.

GARRICK: Mark Abkowitz.

ABKOWITZ: Abkowitz, Board.

I want to get some clarification on the way features, events and processes are being handles. I don't recall a specific bullet on a slide, but my recollection was that if the Department of Energy sets out, which I guess means the probabilities are low enough so they felt they didn't need to look at a risk over the first 10,000 year period, that these proposed regulations would allow them to set that same situation, or scenario, out for the 1 million year period. Could you clarify if my understanding is correct?

FORINASH: It's basically correct. Essentially, what we've done is that the FEP's analysis that was done for 10,000 years is sufficient for a million years, that you look at those FEPs, and you continue to assess that evolution of those FEPs, and the continued effect that they might have beyond 10,000 years, out to a million years. And, we did do, you know, sort of an assessment of different categories of events that we thought might be missed by that approach, and concluded that the effect of those things would be swamped by the effects of other FEPs that would already be included within 10,000 years, with the exception of a few key scenarios that we felt explicitly that DOE needed to sort of reconsider beyond 10,000 years. So, that's the four items that we specified, in addition, which are igneous, seismic, climate change--climate change being sort of the most obvious one that you often screen out the basic consequence, or, you know, probability during the first 10,000 years that you need to consider over the long time frame. And, then, the last one is corrosion.

ABKOWITZ: Abkowitz, Board.

So, in essence then, you said the EPA sat down with the DOE and went through this process and, in effect, reconducted the FEP analysis over a 1 million year period, or just took the 10,000 and extrapolated?

FORINASH: Well, we didn't sit down with DOE and talk about it. We did look at what in general the kinds of things that will be screened in or screened out of the analysis, based on the conditions that we had established for 10,000 year FEPs or--and we can see also from some of the existing performance assessments how that has been implemented in different cases.

So, as a group, we looked at the type of events that would be included based on our criteria, and, you know, those things that are important beyond 10,000 years, you know, many of them we think will continue to be important beyond that time. And, are there things that might be missed that would have important effects was the question that we were asking ourselves, and then, you know, there were these four major areas that we said we felt will be significant.

But, what we wanted to avoid was a reassessment of FEPs out to a million year time frame with an endless debate about whether one thing maybe should be in or should be out, especially if you're talking about, you know, one in 100 million over a million year time frame. If you have an event or a process that already accounts for the kinds of releases that would be expected from something that's at the margins, then we think it's more productive to focus on the performance assessment for those things rather than having a debate about what, you know, sort of endlessly about what could or couldn't be eliminated for a million years.

ABKOWITZ: Abkowitz, Board.

My concern is that the starting point in that process is looking at a 10,000 year horizon, and consequently, the scenarios that you think through and evaluate across that horizon tend to cast a net perhaps not as wide as the scenario net you would cast if you start off with a million years. And, so, by focusing back on those that were FEP'ed out initially at the 10,000 year period may not completely consider all the other things that perhaps could have reared their heads over a million years. And, sort of the other thing that concerns me is that the implication of taking something that you FEP at 10,000 years and then FEP at a million years is you're, in essence, saying that the likelihood that that event could take place from year 10,001 to year 1 million, basically, there's no chance it would happen. Or, that's one way to interpret that.

FORINASH: Well, what we said is what it would mean is that there's the chance that it would happen before 10,000 years is not less than the chance it would happen after 10,000 years. There is essentially only one thing--the fact that we felt fell in that category, and that's climate change, and we expect that we require them to consider climate change. And, we did look sort of at the categories to see whether there were other things that might be in that group, and were very hard pressed to identify anything beyond climate change that you'd actually see, you know, a change in the probability beyond 10,000 years.

So, I would say the other thing is that this was an area where we particularly look to the National Academy of Sciences report for guidance, and the igneous, seismic and climate change were the three areas that the National Academy of Sciences very explicitly said that they thought would be especially important to consider over the long time frame. The only other item that sort of clearly came out from the NAS report was that they clearly considered corrosion, and the effect of the longevity of the waste containers to be a very significant parameter in the behavior.

So, those were the four things that we pulled out. There were not other things that were highlighted explicitly by the NAS. And, so, we felt that that was a reasonable guide for us.

GARRICK: We have four more people, now five, that have asked to ask questions. We have five minutes to do it. So, you might think in terms of a minute a piece. So, our next one is Andy.

KADAK: Kadak.

Based on this last discussion, it sounded like the FEP question was a policy determination as opposed to technical determination. Is that correct?

FORINASH: We did not do an exhaustive technical analysis to see what FEPs would be included or not. That's the Department of Energy's job.

KADAK: Okay.

FORINASH: To do an exhaustive sub-analysis. Our job is to set the envelope and the basic parameters of what we think should or shouldn't be the kinds of events that should or should not be included in that.

KADAK: I'm just interested in what kinds of public comments you received to date about this new proposed standard.

FORINASH: To date, we have not received very detailed comments from any party. Those tend to comment more sort of closer to the end of the comment period. They're all over the board. I mean, we have people who said you're crazy, this is totally unprotective. We have people who say this is crazy, how can you possibly be arrogant enough to think that you would know what would happen in a million years.

GARRICK: Whatever it is, you're crazy. George, George Hornberger?

HORNBERGER: I had a followup question as well. Betsy, you indicated that the climate change was the single thing that you singled out as being an important FEP beyond 10,000 years. And, yet, my recollection is, and my recollection may be faulty, that the draft standard is pretty restrictive as to how the DOE mind considers climate change in the future. How did you come to grips with that kind of specification without a detailed technical analysis?

FORINASH: We're not very specific actually, I don't think, in how we describe the consideration of climate change. The NRC does have a draft proposed rule out to amend its licensing requirements to agree with our proposed standard, and they actually are quite a bit more specific on climate change.

What we did for the four scenarios that we required DOE to reassess after 10,000 years of igneous, seismic, corrosion and climate change was that we provided some direction that essentially said you can focus your analyses on the most important aspects of the scenario. So, for example, with earthquakes, you need to look at how it might affect the integrity of containers, or the integrity of the inside. You don't need to look at the potential effect of fracturing, because in our assessment, you know, it could even make it worse, or it could make it better, and there are other FEPs that you can do that would assess the effects of making it worse or better.

That's the same thing we did, in essence, for climate change, but we said you can limit your assessment of climate change to consideration of the effects of greater precipitation infiltrating the disposal system that could affect the dissolution and the transport of radionuclides out of the system. And, we said--we had quite a lot of discussion about how we might project climate change, and the difficulty of establishing the exact parameters of how climate might change. So, we said you need to account first for the effect of greater climate change, but you can do that assessment of greater precipitation by using a higher precipitation value than current exists, but at a constant rate, or some other simplification of that system.

GARRICK: Very quickly, David Duquette.

DUQUETTE: This will be a quick one--on the blackboard, the comment that you've used several times has been corrosion because you specifically said in the regulations general corrosion, and in my world, that means something very different than just corrosion. Is there some reason why general corrosion was focused on, and not localized corrosion, which has been a major concern of this Board?

FORINASH: Again, it's because of the time frames that we're looking at and trying to distinguish between things that might be important beyond 10,000 years, that might not have been considered important at shorter time frames. So, the progression of the general corrosion that you might be able to make a case perceived at such a slow rate that it would be inconsequential during the first 10,000 years, but we don't necessarily think that that's a legitimate claim to make in time period between 10,000 years and a million years. The arguments I think are not really in play with localized corrosion.

GARRICK: Our consultant, Bill Murphy?

MURPHY: Bill Murphy, consultant to the Board.

You said that there are no changes in the proposed rule to the reasonably maximally exposed individual, which I assume means that that person is still located at 18 kilometers away. And, you've also said that the DOE must consider how Yucca Mountain could behave under a wide variety of possible circumstances. Is one of those possible circumstances that the maximally exposed individual could be somewhere else, for example, at the discharge point of the groundwater flow system over a time frame of a million years?

FORINASH: Not under our regulations, it's not. Under the range of scenarios that you could envision, it's certainly possible that there could be significant changes in lifestyles, where people live, you know, how they get their food. But, I would say the very same thing is true within the 10,000 year time frame, and we made the decision, and I think it's very much in line internationally, that you look at present day conditions, and your reasonable predictions in the fairly near future about how things can evolve in establishing the RMEI. We have no better information beyond 10,000 years out to a million years that would lead us to reassess where people might live, and to add a more speculative aspect to the RMEI.

MURPHY: I was interested in where the people may be, but in where the radiation may be.

FORINASH: Well, the location of the RMEI is essentially the closest point at which we think people can reasonably access groundwater. And, so, being in a location either closer in, where they have less groundwater, or further away, where there would be more dilution of radionuclides, we think will lead to less conservative assessments than the use of our RMEI.

MURPHY: Thank you.

GARRICK: Thank you. We had one request for a question from staff. It appears that that's been taken care of as a result of the Board's rather complete series of questions.

All right, thank you very much, Betsy. That was an excellent presentation.

Our next speaker is Dr. Victor Gilinsky. Dr. Gilinsky is an independent consultant primarily on matters related to nuclear energy. I've known Dr. Gilinsky for a long time. He was a commissioner at the U.S. Nuclear Regulatory Commission. He is a physicist by training. I first ran across his work when he was at the Land Corporation and head of the Physical Scientist Department, and director there.

I also owe him a thanks that I've never given him. One of his most able technical assistants while he was a commissioner later became an employee of mine, and I wanted, Victor, to thank you for the excellent training you provided.

So, with that, carry on.

GILINSKY: Thank you very much. Good morning. It won't surprise you that I take a different view of the post-10,000 year period. But, I want to go beyond that and say some additional things about this new rule that I think haven't been widely discussed. What I'm presenting is really selected views on the subject. They don't cover the full range. Those will be presented in Nevada's submission to EPA.

The new rule is really very much the same as the old rule. Everything before 10,000 years is the same after 10,000 years. Whereas, we earlier had an infinite allowed dose, we now have a very high allowed dose. And, I want to make three points. One is that the post-10,000 year standard is even more permissive than it looks. The second is that the practical effect of this, and this is not so obvious, is to eliminate, or at least undermine defense-in-depth protection for the pre-10,000 year period.

In other words, the problems aren't just way off in the distance, and the EPA proposal fails some basic comparisons, including with its own stated goals. To understand what's happening, you have to know something about the history. Now, you may think that recalcitrance is a bit harsh, but you ought to read what the court says. And, you know, the judges were very, very smart. I attended the oral argument, and, they were very incisive. They understood it, I would say, better than any of the lawyers on either side.

And, often, much is made of the fact that they threw out, I don't know what it was, five out of six issues, they understood very well, that in granting the sixth, they were granting the functional equivalent of the ones they had thrown out because Congress said the action had made those moot. And, in particular, EPA was supposed to follow the recommendations of the NAS, and the court said the agency consciously and outrightly rejected the Academy's findings and recommendations. And, I think EPA was still in that mode, and make no mistake in under-rating the court.

The Court focused on the peak dose, which is what the National Academy focused on. And, the reason is the peak dose is terribly important because it measures the site's adequacy. Everything goes back to defense-in-depth, the fundamental principle of nuclear safety requires redundancy, which means among the various elements of your system. I just picked out one statement from an IAEA document, which was cited by EPA as one of their backup documents. And, they said, "The overall performance of the geological disposal system shall not be unduly dependent on a single barrier or function."

Now, the peak dose, of course, comes after the packages fail, which is a consequence of the packages failing. It measures the site's ability to retain radioactivity. A good site has a low peak, or no peak. A bad site has a high peak, which means the stuff gets out.

Now, EPA has a Yucca Mountain dose that it defends on health and safety grounds, and has gone through rule-making, and so on, it's the 15 millirems per year, and should be extending the obvious response to the court as to extend that limit to cover the peak dose. But, EPA shrank from doing this because, as we all know, if you go out further beyond 10,000 years, DOE has a big peak, and if you extend it to 15 millirems, you're going into that peak. Of course, the meaning of a high peak is that the site is not good, at least on the basis of those calculations.

But, what EPA did instead was draw a two-tiered standard that sort of comfortably is above the DOE calculations, and actually, in the EPA documents, they say that DOE doesn't have to do any more to satisfy this standard than it did before.

Now, EPA calls this standard 350 Millirem. The rationale for 350 is, it's a strange one, it says that Amargosa Valley is roughly at the 350 level, and natural background, including radon, but in any case, they conclude it's 350, They conclude that Colorado is 700, and, therefore, Amargosa Valley residents shouldn't fuss about getting another 350, which to me is a bizarre rationale.

But, the thing I want to focus on here is, some of you raised questions about it, is the fact that you're going to the median of the TSPA runs. And, in terms--incidentally, in doing this, EPA is completely ignoring NAS's specific recommendation, which I cite here, it says, "We recommend that the mean values of calculations be the basis for comparison with our recommended standards." It's on Page 123 of the NAS report. There is absolutely no mention of this cite of any of the EPA documents. So far as I can tell, they were not aware of it. Betsy can tell us different if that's wrong. But, it's in complete ignorance and defiance of this statement, which they're supposed to be in accord with.

But, in any case, if you look at the curves, which I'll show in a moment, the 350 is really in the more familiar terms of mean, is approximately 1,000, and it comes from the following. If you just look at the standard DOE curve for their basic case, and the red line is the mean, the blue is the median, and it's a--if you look at it, it's roughly a factor of three. Incidentally, it depends--it's only at that point that it's a factor of three. It depends on the calculation.

If you were to look at--sorry--if you'll look at in the region where it's rather steep, it can be an order of magnitude or more difference between the mean and the median. And, some of the other calculations like igneous events, and so on, the factors are quite large.

Incidentally, I just want to make one point here. These DOE calculations include drip shields. From our point of view, the drip shields should not be included in any of these calculations, and they should not be allowed to take credit for in any of the licensing cases, because whether or not a drip shield is going to go in in 100 years from now or 300 years from now, or whatever, is an entirely speculative thing. And, I think you will hear a little more about it tomorrow. But, I just didn't want to pass by without mentioning it.

Now, as they make clear and they're quite upfront about it, they use the median to get rid of the high doses, and this is a quote from the Federal Register statement, which to me doesn't make sense, but in any case, it basically says that the high values are sort of driving the mean to too high a level, which they've not happy with, and it's not, quote, representative of the overall distribution.

Now, which of these things you use of course depends on the question you ask. But, there are some instances and statistics where it's appropriate to use the mean, but is this one of them. And it's getting rid of the high case is valid. It's not like throwing out the kind of strange experimental result, which we've all done in the lab. I mean, it means if something went wrong or you didn't follow the rules or this is not part of the experiment and shouldn't be included. Here, we're talking about cases that are individual runs that reflect random parameter choices taken from parameter distributions selected by DOE and, therefore, all of these runs should have equal weight. And, there's no more reason to throw out high choices than to throw out low cases.

Also, despite some of the things that were said here a little earlier, I don't think the situation is that EPA needs to rein in DOE's inclination to be overly conservative. I mean, this is if DOE is trying not to get a license here. What it comes down to is the EPA has asserted the median in a way that basically gives DOE an extra factor of three. Now, you might say why worry about all this if we're talking about hundreds of thousands of years in the future. Well, the reason is it isn't necessarily hundreds of thousands of years in the future. I mean, that's one reason. It could come a lot earlier, and I'll show a slide about this, presenting my view on this.

The long times are a result of the assumptions in the DOE calculations, and the parameter choices. The assumptions about, in particular, the assumptions about corrosion, what kind of corrosion takes place. Do you get localized corrosion? Do you not get localized corrosion? What sort of elements are present in the mountain. What sort of minerals does it form?

The times that we're talking about are just simulation times. We don't really know when the peak is going to come. And, at this point, DOE's simulation results should not have a claim on our confidence. It's a scientific or technical brief of a litigant. I mean, they've worked on it. It may be valid, it may not be valid, but it hasn't seriously been tested. And, it's improper, in my view, for EPA and NRC to be relying on this information which is yet to be decided on in a hearing in forming rules about that these same calculations have to meet.

Here, by the way, what I've done is I've taken, on the right, is the picture you saw before. On the left, is the same thing with the case they present with no packages or drip shields. So, this is like no protection and optimistic protection. And, so, I try to expand things so they more or less match up. It's not perfect. But, to my mind, the real peak is somewhere in between, and we really don't know. I mean, we can have an educated guess about it, or a projection, but we really can't be sure, because we're talking about projections that go really far out, far beyond any sort of experience.

As I said, the peak could come early. But, then, you could say well, if the peak comes early, are we covered, we've got 15 millirem. What are we worried about? Well, not necessarily. Here again, you have to distinguish between simulation and reality. We're just talking here about a rule, which is basically a design standard that you're going to test prospectively on a one-time basis by using a computer simulation, which at this point, is based on relatively sparse geologic data, otherwise, we wouldn't have parameter spreads, and projections of package material performance far beyond our experience, as EPA itself makes clear in sections on uncertainty.

Now, if NRC accepts DOE's optimistic projections, the repository is basically designed to the permissive post-10,000 year standard. And, after closure, and probably sometime before that, as a practical matter, errors will be irretrievable. I mean, this is not like a speed limit where there's going to be a cop watching to see if you're going at 15 millirems, and stopping you if you aren't. I mean, if NRC and DOE turn out to be wrong in real life, and the packages fail earlier, this is the point where you need defense-in-depth, alternative protection. But the radioactive particles are not going to remember EPA's rule. They will follow nature's rules.

And, so, the practical effect of having a cutoff, and then relying on these ideas that you're going to have the peak come later, in both the old and the new rules, is to eliminate defense-in-depth protection for the pre-10,000 year period.

I want to say a word about how EPA got to all this. They have a certain sort of equation, you might say. They don't quite put it this way, but if you read the prose, it's many, many, many pages of prose, but basically, there's something they call reasonable expectation, which they rely on as being some kind of fundamental principle, in some way a weaker standard for judging adequacy, plus uncertainty about the future, and somehow these together are justification for having a looser standard.

Now, a reasonable expectation, this was given up by the federal government in the oral argument by the NRC specifically, and the judges refer to this in their decision. Yet, nowhere in any of the EPA documents on this rule is there any mention of this. And, they keep talking about reasonable expectation as being the principle that allows them to do--be kind of more casual about it all. And, to my mind, logically, their discussion of uncertainty is just backwards. I mean, uncertainty calls for tighter standards, not looser standards.

If we think the peak could come earlier, we--the major uncertainty is when this peak comes, when it's going forward or back in time. If we don't know where it's going to be, you want to cover the possibilities that it could be earlier. "Prediction of the performance behavior of engineered systems beyond a few hundred years is unprecedented based on current technology." That's a quote from EPA, or their principal contractor, actually.

And, if the uncertainty goes beyond some limit, and you don't know how the system behaves, that is not a reason to have a loose standard. At some point, you just say this is not the right place. We don't know how the system behaves.

I want to turn to some of these comparisons. Mention has been made of WIPP. One of EPA's objectives is to ensure that people living near Yucca Mountain are protected to the same level as those living near WIPP in Carlsbad. And superficially, it looks the same. One is 15 millirem, and the other is 15 millirem. One is 10,000, the other is 10,000 years. But, in WIPP, there is no water, and there's no migration of waste. And, the approval doesn't depend on the package. We're not worried about some package failing. And, EPA says itself that it doesn't expect anything to happen afterward.

So, the 10,000 year standard is basically an infinite standard. I mean, a lot of other countries have 10,000 year standards, but they'll say something like if nothing dramatic happens after that. So, it's okay, you know, 10,000 years, you may be pretty sure nothing happens after that. So, it's okay. To them, you have a specific standard for 10,000 years, and then you're pretty sure nothing happens after that. By contrast, Yucca Mountain, water is flowing through the mountain, and there's a kind of radioactive balloon payment for sometime in the future. So, this is not comparable, and to make it comparable, EPA would have to extend the 15 millirem beyond the 10,000 years.

Also, on the international standards, I took a look at this IA document that I list on the bottom, which is one of the principal documents that EPA lists as its reference documents for this rule. And, they have statements to say, it happens to be Safety Fundamentals, Principle 4, and they say, "Radioactive waste shall be managed in such a way that predicted impacts on the health of future generations will not be greater than relevant levels of impact that are acceptable today." In other words, you maintain the same standard. So, at least in terms of the IAEA, the two-tiered standard doesn't work. Also, I want to repeat this, about not being unduly dependent on single barrier function.

I also want to compare with reactors. You know, NRC's reasonable assurance has a sort of historical basis. EPA keeps pushing for a weaker reasonable expectation even though it was given up in the oral argument, and the court said it was virtually indistinguishable. In the reactors, there's multi-barriers. In Yucca Mountain, overwhelming reliance on the package. Separate standards for individual barriers, yes on reactors, no on Yucca Mountain.

Allowed dose, you know, the allowed dose is on the order of 10 millirem per year, and to an individual standing at the fence post. Yucca Mountain is 100 times higher. It's somewhere 18 kilometers away after all sorts of dilutions have taken place.

But, I think the most important thing is that when you're talking about reactors, you've got a system of inspection and enforcement. You can correct things. Here, after closure, it's irretrievable. That's a really important distinction.

I just put down a bunch of reasons for extending the 15 millirem standard. First of all, to provide defense-in-depth by ensuring an adequate site, and I quote the IAEA on this. To conform with the NAS safety recommendations, as required by law. They recommend that compliance assessment be conducted for the time when greatest risk occurs. Bob Frey in speaking to the ACNW again not too long ago, said they didn't recommend the alternative of a tiered approach. They didn't consider it.

To meet EPA's own stated objectives in relation to WIPP. To meet IAEA's Principle 4 about maintaining the relevant level of impact. And, finally, because 15 millirems is the only standard that has a firm basis in EPA rulemaking.

I want to close with I guess kind of a

philosophical point, which I learned, came to in part from reading something written by one of the members here. This isn't about having a perfect system. We know that engineered systems can't be perfect, and that isn't a reason for rejecting them. In typical engineering, some failure is to be expected, and, in fact, it's the process of developing things right. I mean, failure is one of the ingredients of success. You learn from failure to improve designs, and the fear of failure imposes a certain professional discipline on the designers.

You know, I give the example that nobody wants to be the one who overlooked the "O" ring in the space shuttle. But, for this to work, the feedback loop has got to have a certain reasonable time in relation to man's work, man's life, and so on. You can't be--it's got to have a certain finite quality to it. But, that's not the case here. The feedback loop is such that you have to measure in many, many, many generations, at best. And, so, the consequence of post-closure failures are going to come too late to fix the design.

And, they also come too late to affect the designers. And, without saying anything unkind about the designer, the fact is human nature being what it is, it means you don't have the same pressures for getting things right as you do, say, in the space program. Plus, right now, DOE is sort of single mindedly focused on getting an NRC license. But, what this says to me is that to compensate for this lack of feedback-induced professional discipline and correction, that we need much tougher regulatory standards, and a much tougher regulatory approach rather than a looser one.

Thank you.

GARRICK: Thank you. We have time for a few questions. Henry?

PETROSKI: Petroski, Board.

I wonder whether you are having a dialogue with EPA and DOE and other players, and, if so, what form that's taken?

GILINSKY: Well, Nevada has had some discussions with them. I personally attended one of the meetings with EPA, but, the dialogue is basically going to take place by virtue of Nevada submitting its comments in this rulemaking process.

GARRICK: Andy?

KADAK: Kadak, Board.

Dr. Gilinsky, I'm just trying to think about expectations. Do you think it's reasonable to have a standard that goes out to a million years that, say, is a 15 millirem standard? Do you think that's even plausible to be able to demonstrate?

GILINSKY: Well, you know, let me just turn to reactors. We have the ALARA program, we have very, very low allowed--

they're so low you can't measure them. They're calculated. I think that basically, you're setting a 15 millirems limit. You're saying we want to demonstrate that this site can retain radioactivity, and I think that's perfectly reasonable. I mean, you need it at WIPP. There's no question about meeting that standard. It doesn't matter how long you go out in time. I mean, I think this million years is kind of a red herring. The real thing to focus on is the peak, and whenever that peak comes, you can put it anywhere you want. But, wherever you put it, it ought to meet that sort of a standard.

KADAK: But, even you suggested that there was a large uncertainty. And if you look at some of your curves, they could go down from almost nothing to this large number, a couple hundred--

GILINSKY: The uncertainty, the principal uncertainty has to do with the failure of the package. And, that determines where that peak is going to be. So, at any one point, you may have large uncertainties, but the uncertainty is is the peak going to be there, or is the peak not going to be there. I don't think it's so important where the peak is, at the peak when you calculate it, you want it to be meeting a tight standard, because that is what demonstrates that you have a decent site.

GARRICK: Garrick. One question. How do you deal with the paradox that the better the containment, whether it be engineered barrier systems or the natural system, the further out in time goes the peak dose?

GILINSKY: Well, I think you want to have as good a containment as you can possibly get. There's no question about that. But given that we are projecting so far beyond any sort of experience that we have, we ought to apply defense-in-depth. We ought to have as good a containment as we can have it, and we ought to have as good a site as we can have.

GARRICK: Ron?

LATANISION: Latanision, Board.

You made the comment on a couple of occasions that the DOE is relying on simulations in the absence of hard data, something to that effect, and I'm wondering what you would envision as an alternative, given the time scale that we're talking about here. How would you, or could you be specific in terms of what concerns you about how this is being done? What's the alternative to simulations, given the time scale, and what would you do differently?

GILINSKY: I think we're projecting systems performance way out in time. We are driven to using simulations. But, we ought to apply a tough test to the simulations.

GARRICK: Andy wants to clarify.

KADAK: I just wanted to be sure I understand. You're not suggesting that WIPP be the site for Yucca Mountain, or the repository, are you?

GILINSKY: No, I'm not suggesting anything here. I'm making a comparison, and it's actually one that EPA itself makes, but I think the comparison is very different from the one they present.

GARRICK: Any more questions? Thure?

CERLING: Cerling, Board.

Earlier in his introductory remarks, Dr. Garrick made a comment about the conservatisms and reality based projections, and how would the calculations come out in favor or in not favor of the standard if more realistic comparisons were made in the TSPA model? Do you care to comment on that?

GILINSKY: Well, I guess I can't comment on the level of understanding of the various parameters and processes. My impression, looked at from Nevada's point of view, it doesn't look to us as if DOE is overly anxious to be conservative. Others have a different view here, as I have heard this morning. But, my impression is they are very much focused on getting to yes.

GARRICK: All right. We're pretty much to our schedule. I should ask if there's any comments from the staff at this point, or any questions from the staff.

(No response.)

GARRICK: Having none, I think we will adjourn for our scheduled break. Thank you. Thank you very much.

(Whereupon, a recess was taken.)

GARRICK: If we can take our seats, please?

Our next speaker will be John Arthur, who will talk about the OCRWM program and project overviews. John Arthur is the Deputy Director of the U.S. DOE's Office of Civilian Radioactive Waste Management, and leads the Office of Repository Development. John has been with the waste business for many, many years, and has some successful projects associated with his curriculum vitae, most notably WIPP. We're glad to have him here today.

So, John, please give us an update on what's going on with OCRWM.

ARTHUR: Thanks, Dr. Garrick. And, welcome, members of the Board.

I'm pleased to be here today to address this important meeting. We've had a lot to discuss since the last meeting, as mentioned earlier this morning, in February, and we have a good agenda with a number of topics to cover.

Paul Golan, who joined the Office of Civilian Radioactive Waste Management in May, and is currently the acting director, extends his regrets for not being able to join us today. As you're well aware, there's a number of challenges in Washington with some of the budget and other areas that Paul is dealing with.

My purpose today is to summarize the Department of Energy's Office of Civilian Radioactive Waste program and project, and in particular, I'm going to focus on about five or six areas.

First of all program priorities and our path forward as an organization. The FY 06 budget and some our organizational changes. Currently, where the project stands right now on review and comment on the proposed EPA and NRC standards. DOE's examination of the operating Yucca Mountain repository in a way that will minimize the handling of bare spent nuclear fuel. Impacts to and the status associated with the License Support Network and license application, and then, also, a status on some of the reviews of the high level associated with the original allegations on falsification of records that were brought to the Department of Energy's attention back in March of this year.

First of all, on some of the program priorities, I want to focus currently on a number of areas, improving, continued emphasis to improve, and our project management making discrete projects within a program like this. There's a large volume of work. We want to projectize as much of that as possible, and have strong discipline as we manage that, and continued improvement in our contract management, all the direction that's set for the program, working through the contract. There are multiple contractors.

Improving organizational qualities, I've addressed the Board many times in the past. We showed various performance measures, assessment of where we are on cultural areas, safety conscious work environment, and other key areas, and we want to keep moving that in the right direction as we move towards being an NRC license applicant.

Also, a focus on improving the safety, reliability and reducing complexity. And, that's what I'm going to talk a little bit later when I talk about the change in examination of canisters that was announced last week.

Also, under Paul's leadership, we've stepped up an emphasis on trust, but verify in a number of key areas. We're increasing independent oversight in various areas on reviews of ultimately the License Support Network and other key areas by funding universities and other independent experts.

A little bit now on the fiscal year '06 budget, and I noticed there was a small section in this morning's Las Vegas Journal on that. We are still currently today operating under what's called a continuing resolution. That will continue currently until November 18th. However, last evening, the Senate and House Conference Committees met and recommended funding the Yucca Mountain repository at $450 million for the fiscal year '06. That number is about--well, exactly $127 million below what we operated to in fiscal year

'05 at the $577 million level. And, it's also $201 million below the fiscal year '06 request.

Right now, since this was just announced yesterday, we're looking at the impacts of this in association with our program priorities, and I really won't have a bunch to say right now. It's going to take us several weeks, if not months, to get through all the analysis that's required.

Additionally, I know in the past, I talked sometimes about organization changes, and there's probably one or two key areas I'd like to bring to your attention. Previously, we had Joe Ziegler, who was Director for the Department of Energy, Office of License Application and Strategy. He announced his resignation approximately three months ago. Joe has addressed this Board before, has experience and leadership, has been very instrumental in the license application early development and strategy. He served in that position since August of 2003.

Yesterday, we announced Mark Williams was selected as the Director to replace Joe Ziegler. Mark joins OCRWM. He's currently within the Department of Energy in our Office of Environment, Safety and Health, where he has worked on oversight and safety of nuclear facilities within the Department for the last fifteen years. He has over twenty-five years of nuclear related experience, including the Navy propulsion programs. He served previously as a senior project manager in licensing and reactor regulation of NRC.

Although we don't have a reporting date yet for Mr. Williams, I anticipate and expect him to be on board within the next month or so. So, you will get a chance to meet him in upcoming meetings. Until then, many of you knew Claudia Newbury. She's our Acting Director for this important office.

I'd also today like to ask Ted Feigenbaum, Ted, if you'd stand up? Ted took over as Bechtel SAIC's President and General Manager on August 15th of this year. I'm very pleased to have Ted's experience on the project. He's an experienced nuclear executive with 33 years of engineering, design, construction, start-up, and various licensing experience in the field of commercial nuclear power. His most recent position was Chief Nuclear Officer at the Maine Yankee Atomic Power Company. Again, we look forward to Ted's experience in this program as we proceed ahead.

A little bit now on the EPA and NRC standards. We are completing our review currently of the proposed EPA and NRC regulations. We intend to submit our comments to EPA on or before the comment period expires on November 21st of 2005. And, we also intend to submit our comments to NRC on or before December 7, 2005, when their comment period expires.

In parallel currently, and for the last several months, we have been continuing to review the potential changes that would be required in our licensing case, based on the draft language in the EPA and NRC regulations, and will make the necessary adjustments once those regulations become final.

Now, I want to transition a little bit. Most of the discussion today will be on examination of operating a repository at Yucca Mountain in a way that's going to allow us to minimize the handling of bare spent nuclear fuel and allow it to operate primarily as a clean facility.

When Secretary of Energy Bodman came on board, he asked us to look at all aspects, take a total look at the Yucca Mountain program. As part of this process, we looked at ways to add a simpler approach to our operations, and that's what drove us to something I'm going to talk a little bit later about a Critical Decision process that we announced last week.

On October 25th, DOE announced our plans to examine the operation of Yucca Mountain repository as primarily a clean facility, clean operations. This change in direction and design, and that that exam will occur, means that spent nuclear fuel would be sent to the repository in standardized canisters that itself could be disposed in the repository and would not require repetitive handling of the fuel.

If you recall, when I had Paul Harrington and some of our other DOE managers briefing the Board in the past, we've talked evaluations of over 225,000 spent fuel individual assemblies that would be required to be handled. This approach greatly reduces that, if not eliminates it.

Examination that we're going to talk about here in a minute will review the impacts on the safety, the operation and long-term performance of Yucca Mountain, fuel handling and construction of the repository, including any complexities of Yucca Mountain's post-construction operations, the numbers and types of facilities. If you recall, we explained the layouts before, some of the surface operations, what will actually occur as a result of this implementation of a canister process, and also the overall NRC licensing process.

I thought it was important because sometimes in the Department of Energy, we use terms that are a little bit different from that in industry or even other agencies. But, we have a very important order in the Department of Energy, it's a 413, which is all project management. That order drives any new projects, the missions changes, whether spent on important national security missions, whether it's spent on our environmental clean-up in the science program, and also the Office of Civilian Radioactive Waste Management.

Currently, for the program that we previously presented to the Board, currently baseline as we call it, we had what's called a Critical Decision approved in June of 2004. To explain that, again, we will now have to do an evaluation of the implementation of canisters, and provide this to our Secretary of Energy Acquisition Executive to decide if the project should continue on its current path, or proceed into the next phase.

Now, this is what you do on any new project. Where we are here is we have, and I'm just going to explain what the Critical Decisions would be, and it's no different than probably what industry would pursue in the new capital of their projects.

The CD-0 process is when you approve the need, permission to start our project definition, new project coming on line to say you have to proceed to go to the next level.

CD-1, for which we had approved back in June of 2004, is approval to select development alternative, and also start preliminary design.

CD-2 is approval of a final design and your performance baseline and construction costs.

CD-3 is your approval to actually start construction.

And, CD-4 is approval to start operations.

So, the one I'm talking about is the Critical Decision-1, is what we will be re-evaluating.

Some of the documentation that's required on this effort is first of all, conceptual design. So, what we want to do is build on the existing conceptual design we have, and look at the canister process, do all the necessary reviews that are required. It will require Safety Hazards report, long-term management plan. A lot of these documents were already prepared so, again, in this process we're going through, while we're going to take a fresh look, we're going to build on work that's been already done.

On October 25th, as I mentioned, we issued a letter, the Department of Energy, to BSC, Bechtel/SAIC, which tests for development of a conceptual design, or the CD-1 package. And, some of the key elements that we had in that letter were, one, canister operations. Canisters arriving at the repository would be disposable after being placed in a waste package. We want to develop minimum bare fuel handling capability that would also be used for any off-normal operations. Even when you look at primarily canister, we're still going to need to have some bare fuel minimal handling capability for any off-normal, or some areas where you just can't put the material in a canister.

We want to utilize aspects of the current design to the maximum extent practicable. Maintain still a phased construction approach. If you recall before, we talked about a phased approach, because if you look at budget allocations and just what you can construct, you've got to learn to proceed in a phased approach.

We directed the inclusion still of both truck and rail delivery, or receipt at the repository. We want this to still be consistent with our Office of Civilian Radioactive Waste Requirements Document for acceptance rates of spent nuclear fuel. We don't want to deviate off those receipt rates at the repository and for emplacement.

Two other areas I might mention. We want to minimize the impact on initial conditions for the post-closure safety case. And, then, also at the same time as we've been reviewing various sections of the license application, to look and recommend other system optimizations that can occur at that same time. So, we want to make controlled changes, but also principally focus on the canister approach.

In the letter, we requested from Bechtel/SAIC a response on the feasibility and a preliminary report by November 25th, which is 30 days from receipt of the letter.

Now, I want to talk about some other critical aspects of the project. I'll try to answer the questions a little later on the Critical Decision-1 process. A little bit on the License Support Network. As of late September, DOE had substantially completed processing of all the documents that were required for the certification of the

LSN, other than the special issues like ongoing privileged review.

On September 22, 2005, the NRC's Pre-Licensing Application Presiding Official Board issued a decision holding that a July 2004 draft of the LA was a circulated draft which should be also placed in the LSN, License Support Network.

On October 3, 2005, the Department of Energy appealed that order, and it's under review, or will be under review by the NRC Commissioners. In the meantime, while we're looking at this Critical Decision-1 process, the License Support Network will continue to be loaded, will continue to maintain its status, but obviously, no certification could be made until such time as we get the final decision from the NRC, and also look at the necessary documentations required as a part of this Critical Decision-1 process.

A little bit also, I had some questions earlier about the license application. We put a temporary hold on the development of the LA until the potential changes in the design, also the finalization of the EPA standard and changes that will be required, and also a resolution of our ongoing evaluation of the potential falsification of records issues can be addressed.

So, right now, we do have some revisions occurring to the documents, but overall, we put a stringent configuration control until we have a chance to look at our path forward and make the necessary alignments.

We have put the draft sections under configuration control, and again, as I said, we're going to proceed in a disciplined and deliberate manner to produce a high quality license application.

Now, the area that work is underway, and I think some of that may be talked about in some of the later sessions of this meeting, work required replacing moisture infiltration modeling and technical analysis as a result of the earlier allegation of falsification of records. There's a considerable amount of work underway right now on that, and that work can proceed. It wouldn't be impacted with any of the decisions I talked a little bit earlier about.

Also, modeling and technical analysis to support compliance with the long-term promulgated EPA standard. We're continuing the review and evaluation in that particular area. And, then, as I mentioned earlier, any other sections of the LA, license application, that would not be affected by the canister approach implementation.

We're not going to be constrained by any artificial schedules or completion dates, and the LA will be submitted when our program, and the Secretary of Energy has determined that it's ready to be submitted to the Nuclear Regulatory

Commission.

One other area, and then I will summarize, I want to talk a little bit about, it was back in March actually, after the February meeting, that it was brought to the Department of Energy's attention about the potential falsification of records associated with this program, principally in the moisture infiltration area.

When the Secretary of Energy announced the discovery of the USGS e-mails of concern in March 2005, he stated that OCRWM would evaluate both the technical and programmatic impacts of that issue, and also the Department's Inspector General would conduct a misconduct investigation.

With regard to assessing the technical and programmatic impacts, we've had a lot of work in that area since March. We worked for several months to address all of these issues, and the results of this internal evaluation of the technical impacts are now being reviewed by a number of external experts. This review, part of, as I said earlier, trust, verify, independent verification policy, will provide a rigorous check of the work and give us the benefit of these independent perspectives.

The external review of the technical impact USGS e-mails was initially at mid October. The reviewers requested additional information on November 1, and that review is still continuing. So, I cannot predict when these reports, critical reports, will be released. It is in some of the final independent reviews right now, so we hope that that will be out in a reasonable time frame, but we want to make sure it's absolutely accurate and the right review before it's released.

In summary, the announcement we made last week is the right one. There's been a lot of discussion through the years about a canister approach, and given a full evaluation at this time, this is the right thing to do. This Critical Decision-1 process, we're trying to do it in an expedient manner, but again, it's important, as you're well aware at this time, that we move smartly. First, a lot of our focus right now is working to set the necessary requirements before we proceed into the review by Bechtel/SAIC. That process will take months to complete. And, then, with that and through that process, when the necessary approvals are given, only then would we align the license application schedule and design and path forward.

So, that's where the program is at this time, and I'll be glad to try to answer any questions. Thank you.

GARRICK: Thank you. Howard?

ARNOLD: Arnold, Board.

Your decision to specify these multi-purpose canisters, or whatever you call them, changes the point at which the fuel is packaged into the canister from Yucca

Mountain to a number of sites, the various utility sites. And, there are a lot of implications of that, including even on some sites, the feasibility of doing that. And, I'm just wondering if you've explored site by site out there where the fuel is, what all your decision requires them to do, and whether it's feasible site by site, or maybe you have to go somewhere else and do it, some third party site that isn't in the program now?

ARTHUR: Very good comment, and let me just address it the best I can. That's why right now is an important time, when the Department is not just doing this on our own. I mean, we will have a number of meetings with utilities and others as we move forward along this, to make sure that the requirements are set right on the beginning here, so we do the right analysis.

We do have good information. I think Chris Kouts will be addressing the Board, I believe it's tomorrow morning, could talk better than I, but on information that each facility has now, as far as crane capabilities, various areas that are controlling at each of the utility sites, and, so, as we move forward, and again, that's why I said in the announcement last week, principally canister. We know fully that we may not ever be able to get 100 per cent. We recognize clearly there's some sites that have various controlling factors now. So, right now, we're just moving smartly through the planning phase before we take the next step, and there will be a lot of integration with others outside the Department of Energy.

DUQUETTE: Duquette, Board.

A couple questions. One very general. It sounds like you're taking about a 20 per cent or 25 per cent cut in the program for the coming fiscal year?

ARTHUR: I believe that number to be about 25 per cent.

DUQUETTE: How is that going to be applied?

ARTHUR: I'm not sure, and I'll tell you we've been doing a lot of review over the last month. In fact, with our offices, most of my federal staff have been put aside to actually try to independently review all the Bechtel/SAIC and other budget numbers, and recommend some priorities. Clearly, this Critical Decision-1 that I just talked about, getting this alignment done and done right is a high priority. And, then, we're going to have to take reductions, because we moved into this year, we were, while we had a pretty large amount of what we call uncosted carry-over that could be applied to this year, it's still going to require some impacts and reductions off the base program. So, that's what I meant earlier. It's going to take us weeks, if not another month, to get all those decisions made.

DUQUETTE: Duquette, Board.

The second question I have is a little more on the technical side. This decision to move over to encapsulation, if you want to call it that, at the utilities, rather than all of it at Yucca Mountain, do you see that as a hindrance to a timely submission of your LA? That is, will you have to complete that study before the LA goes in?

ARTHUR: I guess I would say, you know, what you have to do on this, is always have a predictability as much as you can and minimize risk as you move into that licensing environment, meaning after it's submitted. As we talked before, and I know many of your comments said that there was considerable increased risk when you have, and, you know, you can go over to France and watch at LaHague, they have successfully moved I think a very large volume of spent nuclear fuel in a dry environment, and still have increased probability and safety risk with that. And, so, I think in this particular area, it minimizes that risk, and it's the right thing to do.

But, again, as I said earlier, that's what we're going to do over the next several months, is make sure all the analysis is done, not just on DOE, but to the utilities and others, so we make the decision and go through the final approval, we will have a range of estimates, and we can move into it smartly. And, we will also be looking at what it takes as far as our license application schedule, and other requirements. So, that's what's really going to worked on hard for the next several months.

DUQUETTE: So, you don't know if you have to complete the study before LA or if you can do some of it having submitted LA and make changes to the LA afterwards?

ARTHUR: I believe, you know, this evaluation will be done before we would submit a license. It's a critical decision. It would have to be completed before we'd submit a license application.

GARRICK: I have Mark, Henry, Andy and Ron. Mark?

ABKOWITZ: Abkowitz, Board.

John, I also wanted to follow up on your comments about this new DOE initiative. And, as you were explaining the work that's being undertaken, two things kind of immediately jumped to my mind. One is the regimes that the surface facility and then transportation at the waste acceptance sites have been somewhat independently studied, and the need for the integration of that is going to be significant, and particularly, should be part of the canisterized container study.

And, so, I guess one of the things I want to ask is the extent to which you'll be using the Total System Model to try to understand those distinctions.

The second thing is that the other thing that came to mind was the issue of thermal management. And, you didn't mention anything about that. I was wondering if you could comment as to whether part and parcel with this study, is also a study of perhaps revising the Department's thermal management strategy.

ARTHUR: Let me first address the Total Systems Model. First of all, that's going to be the model that drives a lot of this review. In fact, Chris Kouts, who will be briefing I believe it's tomorrow morning on that, I have a meeting with him later today to make sure, you know, we're looking at this from a system impact, so as you change one variable, you know what it does from the front end of the waste, or generator, or transportation system, across the board.

In fact, one of the areas we want to look at is the current wattage limits on the various fuel. You know, we have a requirement about 11.8, and, so, that's one of the guidelines we gave BSC, is still stay within that as you go. But, at the same time, much of that fuel, especially your new fuel, a lot of fuel is a lot higher than that. So, we're trying to look at this from a system, what would be required on the aging capability, from the transportation on the front end to make this work.

At this time, you know, we're still looking at the 11.8 number to drive this, so we haven't varied, to go back to at this time, back to a cold repository.

GARRICK: Henry?

PETROSKI: Petroski, Board.

I was interested in the Critical Decision process that you discussed. As I understood it, you follow this process at DOE for significant projects. And, now, you're revisiting this CD-1 phase for the Yucca project. Is this common to revisit that phase of the Critical Decision path, and if not, why is it being done in this case?

ARTHUR: First of all, on a project of this magnitude, and again, if you're looking at Yucca Mountain, it's a much higher life cycle cost when you look at the tens of billions of dollars than a lot of our other capital projects out there, even some of the system projects that have been approved. And prior to going to get a necessary baseline approved at the Secretary's ESAP, again, we had original plan and current baseline was to have a license application in in December of '04, and we're all past that now by well over a year, and, so, these changes will be required to come up, and in our decision process, we felt it was best to do this through the Critical Decision because it affects our long-term acquisition strategies. We need to get a new baseline cost. You know, while I could sit here and speculate, and again, we're trying to be real careful now not to tell Bechtel here's a design solution, too, do away with this facility. We're trying to focus on the quality and requirements that's required.

But, I could speculate on this. I you looked at some of the earlier designs we've presented to the Board that showed some of the larger dry transfer facilities, there may not be a need for those anymore. I could do away with those and focus on canister, handling facility, and so with some of the large cost changes that could occur here, we're going to have to go back into a rigorous review of this. This isn't just normally you need an extra year for some project. There's a bigger ramification, and we want to do the right analysis.

GARRICK: Andy?

KADAK: Kadak, Board.

I think one of the important things that you need to think about when you start doing this review is the thermal management program, because if you clearly limit your canister loading to the 11.8 and you don't have a good foundation for what the thermal management strategy is in the repository, you could be optimizing to a system that isn't the system that you ultimately use. So, I would start there as a place where you're going to validate and where you are, where you think you are.

The other thing you didn't mention was on the Total System Performance Assessment. What is the status of that, given what you're using as a standard, and what kind of work have you been doing to go out to this over the peak dose point?

ARTHUR: I'll cover the high level, and then I'm going to let I think Bob Andrews and Abe are going to talk tomorrow, and I'll let them get into more specifics. But as we mentioned before, we are doing an analysis against the standard to look at what it requires from a potential new section, the license application, the necessary TSPA runs.

Now, as regards to the canister, I mean, there is a little bit of a trade-off here in some of the metals and areas, so as the canister specification is developed, we want to take a look at what, if any, impacts that has on the Total System Performance model. So, right now, as part of this Critical Decision, I'm going to have our TSPA and other folks right at the right hand of us doing that analysis, so when we look at this, we know all the work that's going to be required.

KADAK: But, it all starts with thermal management.

ARTHUR: Correct.

KADAK: The other thing, if you're looking for priorities about where to focus your money, your remaining money, I would suggest you read John's opening remarks.

ARTHUR: All right.

GARRICK: John, given the questions, and I know you summarized this a little bit in your comments, but it's focusing on the license application. Can you say once again what you consider to be the one or two or three principal technical issues you have to address in order to feel you're in a position to go forward with the license application?

ARTHUR: Okay. In addition to the--

GARRICK: And, the reason I ask that is we seem to be talking in two reference points. One is the general reference of the project, and the other is what you are planning to do to file a license application. And, I just want to kind of get a distinction there.

ARTHUR: First of all, there's a lot of sections of that license application. We've briefed you on some before. They're in very good shape. If you ask me right now, again, I'm saying I have to go through the review of this canister and make the modification to the license application. one of the first technical issues, I'd say, right now is one that we plan to have a lot of discussion and technical exchanges with NRC as we move into the New Year, and that's on the level of detail for the pre-closure safety analysis that's going to be required.

Now, we've had a number of meetings at a higher level, and it's time now to get down to level specificity. And, again, as I mentioned, what we want to do is define and minimize licensing risks, so when that document is submitted and all the supporting documents, we minimize undue delays due to other issues. So, that's one. It's a level of detail in the pre-closure safety analysis.

The other areas, one that you've mentioned, if I think back where we were about a year or so ago, it's the conservatisms and some of the areas, and I think our folks, as they'll talk tomorrow, have done a good job to try to define what is conservatisms versus what is realistic. And, you know, if you recall last year, back I guess it was late '04, we directed some new work in some of the neptunium solubility and a couple other areas to try to go in a more realistic value versus some of the higher ones. And, this regulatory environment is getting to the right balance before we submit, to make sure you have a certain amount of conservatisms, I think you will agree are going to be required, but what is the balance. So, we're taking a look at some areas there, and some of our folks can talk a little bit tomorrow on that. And, you asked me two, and I'll just--but there is a lot of analysis, very sound and well done.

GARRICK: Thank you. George?

HORNBERGER: John, it seemed to me that you mentioned specifically the falsification of records as ongoing. And, perhaps, am I inferring something incorrectly that it's taking a little longer than you thought? Because this past summer, when John Arthur was quoted as saying that the net infiltration work was technically defensible, and was a matter of redoing or remediating to get to the point where it would stand up to scrutiny. Is that taking longer, or is it still technically defensible?

ARTHUR: There's several aspects to the review I mentioned, and there's several prongs, as we said, and we still believe there's a solid technical basis to the LA and the site recommendation. But, getting the right rigor in all the analysis and getting it presentable before it's issued is where we have some of the independent reviews underway.

Additionally, there was a second prong that we called expanded condition. You know, it was always kind of attitudes and behaviors exhibited. This was done in the e-mails. And, so, there is a lot, when you look at a program of this magnitude, I think you will all agree, I mean, it's first, it's a precedence, and I'm not aware of any federal, private sector or other agencies putting all of its records into something like a License Support Network. But, to do all the necessary searches to make sure we run that to ground to the right level is what's taking us the time. And, it will come out soon, but we want to make sure it's absolutely correct before it's issued.

GARRICK: Ron?

LATANISION: Latanision. John, you may have answered this, and I may have missed it, but with the license application, do you have a sense of when you're likely to be ready to submit?

ARTHUR: No, not at this time. And, I believe that once we get through this Critical Decision, that will set the basis by which we file a license, complete the necessary work, make the decisions for the canister approach and go forward. Right now, there is not a schedule.

LATANISION: Could it be years or months? No real sense?

ARTHUR: It's going to be a sense of time. I mean, to do this Critical Decision-1 and do it, and get it right, it's going to be at least, you know, three to six months. So, it's taken us time. It's important to do this right because this will reset the program.

GARRICK: David?

DUQUETTE: Duquette, Board.

Over the last few months, there's been some discussion about reopening the concept of reprocessing again. Is the project looking at that at all in terms of its future, in terms of the LA, or in terms of anything else?

ARTHUR: From our perspective, I mean, the project is not, I mean, I'm not involved in that, the policy and reviews underway. Our focus is on design, the license application, the necessary work. As you're well aware, there's been a lot of discussions on even some of the policy decisions in the budget yesterday associated with looking at the potential for reprocessing. A number of other areas you've watched, there's some evaluations earlier on interim storage. There's a lot of those areas, but I haven't heard any that say give up on a repository. Even if you went into some of that, there would still be a need for a repository.

DUQUETTE: Thank you.

GARRICK: John, I have a couple of questions from the audience. And, one you just touched on. Do you anticipate the need for an interim storage site at Yucca Mountain?

ARTHUR: I really can't speculate. We, right now, if you look at the current baseline we have, if the license application were in today, we had up to 21,000 metric tons capability for what we called aging at the time. If you recall, we were going to bring in spent fuel, handle the bare capability. So, we're still going to maintain a certain amount, even under canister. It may require cooling for a certain period of time. But, that's the main area of the repository our side is looking at right now. It's tied into what's required for the operations.

GARRICK: And, my second question from the audience is do you think DOE's canister change will have any impact on the private fuel storage?

ARTHUR: I can't speculate. What we're trying to do is take a total look at it, and find out the best one to comment. They have a license application proceeding, and we're focused on this separately from private fuel storage.

GARRICK: Thank you. This is the opportunity for the

Staff now to ask a few questions. Carl?

DI BELLA: Carl DiBella, Board Staff.

For the multi-purpose canister, I take it the canister and its storage and transportation would have to be licensed by NRC through their spent fuel project office under 10 CFR 71 and 72. Assuming you had designs for both the other--and the canister, how long does that licensing process take? What's sort of range would you expect? And, I take it that we don't have the design now. You haven't finalized the specifications, if I understand correctly.

ARTHUR: First of all, we'll be looking at some of that through our fatal decision process. You're well aware we would like to get the right canister design and develop the first time. It would require the necessary approvals by NRC, but as we mentioned in our announcement, we're looking at principally a commercialized approach to pursue that. But, right now, our focus, at least from the federal government's side, is to set the requirements and performance specifications for that.

Timing to do that, you know, is a multiple year process just to get the design, and then into the license application, or the licensing that's required, and then into the actual development and manufacturing.

Another point I might add, though, we are working with our partners from the Naval Nuclear Propulsion programs.

I've had some of my design engineers up in Idaho, I mean they have a very effective operation currently for canisters, the transport, loading, shielded type transfer operations. So, we're looking at a number of options to allow us to pursue this in the most cost effective manner. So, it's not just one area. We're looking at a multiple right now.

GARRICK: Dan?

METLAY: Dan Metlay, Board Staff.

My understanding is that in the last 15 months, you've been loading more of the documents onto the LSN, but, that increment of documents is not yet publicly available. Do you anticipate it won't be made available until you go for certification?

ARTHUR: Good question, Dan. I mean, currently, the plan is they wouldn't be made available until the certification. But, that's where we currently are. That could change. Right now, I forgot to mention that in my remarks. We have about 3.3 million documents out there, and, you know, one of the areas I should have mentioned, we've applied all the lessons learned from the denial or the rejection for certification last year. We've had a lot of independent and internal quality assurance reviews to make sure it's very traceable, what we have out there.

GARRICK: I have another question from Board Member Kadak.

KADAK: Kadak, just a couple of quickies.

On the LSN, when do you think you'll be ready to submit to NRC your claim of readiness?

ARTHUR: Well, that's going to probably happen tied into our schedule with the license application, because right now, if we pursue the current directions in the Critical Decision-1, it's clear that under those requirements, we would have to consider all the new documentation that's created as a result of that. So, that's one area I'm currently looking at.

KADAK: But, I thought that the certification is that the system is in place and running well, not that everything will ever be on the system that you would put in in the future. So, when is it that the NRC thinks you're at least caught up to what they expect to see in such a system?

ARTHUR: Let me, if I can, just clarify one point. I don't have the actual regulations for LSN, but it also says that all relevant documentation is available at that time. And, relevant would mean that if we're pursuing this Critical Decision-1 on canisters, then I'd have to have the necessary documents. And, so, that doesn't mean everything created, because technically, it would be about six months prior to filing of a license application.

KADAK: Okay. The other question is, and you didn't really discuss it too much, was the status of the rail line, and what your expectations are relative to if Yucca Mountain opens in the near term, how much of it will be by truck and how much of it will be by rail. And, are you looking at alternatives to the current rail line?

ARTHUR: First of all, on the rail, one of our priorities, and continues to be a high priority in the program, is to keep trying to advance the rail. It's clearly known that some of the weight of the packages and other areas, that rail is clearly going to be an optimization. We're keeping obviously the option also open for truck. But, in light of the 450, we're going to have to sit back and look at the balances of where we are for this year. But going into this, rail is a high priority to keep pursuing that as a transport mode.

KADAK: So, where do you stand in the Nevada extension?

ARTHUR: Right now, we're continuing our work on all the necessary environmental documentation, and continuing to do the necessary work that will be required for design of that rail mode.

KADAK: And, you're not looking at alternative lines that still might be in the running, given the other information?

ARTHUR: Not right now. I mean, we're always looking at options. But, right now, we're trying to focus if it's completed.

GARRICK: Our consultant, Bill Murphy?

MURPHY: Bill Murphy, Consultant to the Board.

Do you perceive that there are new technical issues for sets of different priorities for technical issues, because of the lengthening time scale from 10,000 years to a million years?

ARTHUR: There's some other technical experts that could answer that better than I. But, clearly, we're trying to move along in the direction. I found interesting the discussion on the FEPs this morning, about what's included or excluded. But, we've tried to make judgments, you know, at the 10,000 years based on criteria that what would be included or excluded, and we move that forward, there's probably clearly some areas in FEPs that could be excluded. So, there's a trade-off in trying to get to the necessary level of precision, but we believe right now internally we're building the best analysis we can against the draft standard, and we'll obviously watch what happens as EPA finalizes that.

GARRICK: I have one more question from the public. Since there is no prototype canister, if you had one, how many years would it have to be tested on site?

ARTHUR: I believe that's along the question of what Carl asked me a little earlier about how long does it take, and if I haven't, whoever asked that, I can talk to at the break. But, we want to develop right now, and Chris Kouts I believe has programmed to work on the performance based spec for the canister, the necessary requirements, and then there's a timeline to get that designed, get it through the approval process and into manufacturing. So, I don't know how much testing actually would be done at the site.

I also might remind that canisters are in use right now. It's nothing completely new. What we're trying to do is standardize this and look at it from a systems approach, as we talked earlier.

GARRICK: All right, any more questions from Staff? From the Board?

(No response.)

GARRICK: From anybody else? Everybody wants to have a reasonable time to get lunch, I can tell.

DEVLIN: Thank you very much.

My name is Sally Devlin. I'm from Pahrump, Nye County, where Yucca Mountain is. It's so nice to see you, John. Of course, I asked what on the canister. You have nothing on the canister, and I read your report, all thousand papers, and that is not acceptable with all the disclaimers. And, the one thing that you say is you tested the alloy 22, and my bugs, and for the new members of the Board, I introduced microbic invasion, and the colloidal movement of water besides lightening, and that is my bugs ate it. So, you have no canister at this time. The canisters that they are currently using, remember your standards, they have to live 20 years, 30 years, 100 years, 3000 years, and a million years. So, you really have no canister, and this is the lynch pin of the whole project, as far as I'm concerned.

I wish you'd give a better timeline, because these things do not happen, and the stainless steel and all the rest of the crap is not acceptable. So, please be honest with the public, and I'm the public. Thank you.

ARTHUR: I should have mentioned, Sally, you reminded me of it. The actual canister we're talking about would be sleeved in the waste package currently designed. It wouldn't be in lieu of that. I'm glad to talk to you more on some of the other areas.

MS. DEVLIN: Thank you.

GARRICK: All right, if there are no further questions, we will adjourn until 1:00 p.m. Thank you very much.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

GARRICK: Could we take our seats, please?

We'll now get underway with our afternoon session. I want to first indicate that for the follow-on papers after Mark Peters' paper, or discussion on science update, the two presentations, one on drip shield design, installation and degradation, and the one on localized corrosion of the waste package, I'm going to ask Board members Ron Latanision and David Duquette to lead those discussions, including introducing the speakers.

For now, we're going to hear from Mark Peters on science update. Mark is currently the Senior Technical Advisor in the Chemical Engineering Division of Argonne National Laboratory, and his expertise is in earth sciences, and he's involved in managing, testing and research and development efforts associated with the permanent disposal of spent nuclear fuel. And, we're delighted to hear from him and get an update on what's going on in the science side.

Mark?

PETERS: I don't like to be chained to the podium, so if it's okay, I'm going to stand here, unless that gets too close to you all.

It's great to be back. Thanks for inviting me back to give a science update. I've got 45 slides in 55 minutes, so I think that's better than I used to do actually. I used to jam--oh, given 55 minutes, I've had at least 70 slides. So, this should be nice and leisurely.

So, I'm going to walk through, it's really in the same kind of format that I've provided in the past, walk through the data collection program. These are ongoing activities.

Again, it's a status report, so we're going to walk through the data collection and testing program, starting with the unsaturated zone, work in the unsaturated zone, then the saturated zone. I'll update on work in rock properties, and there, I primarily mean thermal properties, and then shear wave, shear wave velocity measurements, then an update on igneous processes, both probability and consequences, and then finally an update on what we're doing in the source term area.

A couple caveats. As you see here at the bottom. This is preliminary, draft in nature. Again, we want to give you all a feel for what's going on in the ongoing program. And, ultimately a lot of this work will fit into the performance confirmation program for support of future license activities.

So, starting with the unsaturated zone. I've used this slide many times in the past, but I don't think necessarily with some of the current Board members. This is a map view of at least part of the Exploratory Studies Facility, as well as the detail of the ECRB cross-drift. Again, north in this direction, Solitario Canyon Fault here, Ghost Dance Fault here. I just want to point out a couple of the areas that I'm going to talk about today, in terms of some of the ongoing data collection.

I'll talk a little bit about the drift scale test. We're closing in on the end of the cooling phase for that test. Alcove 5. I'll also talk about where we're at with the bulkhead studies, meaning you will recall that the back half of the cross-drift has had a series of four bulkheads installed for three or four years, where we've isolated that whole back half from ventilation, and done periodic reentries into that area, and we've provided some insights on not only seepage in the unventilated areas, but I think also more importantly, what sort of processes might take place inside of an open drift, in-drift conduction and condensation processes.

Also, not on this map, but down here in the South Ramp, as you all well know, I think, there was observed seepage in the South Ramp last winter, and I'll give you a brief status on where we're at with that. And, then, move into the saturated zone, and then come back, talking about thermal properties and how we've used underground testing to get a better handle on thermal conductivity and thermal properties of the rock.

Starting with the drift scale test, I think I don't probably need to dwell on this, but again, we've been heating, we heated the drift for four years, and we're now closing in on the end of the cooling phase. The cooling phase will probably be called good and over by December, January time frame, and then there will be a likely an unventilated reentry into the heated drift area to do some inspection, picture taking, collecting of samples that are sensitive to ventilation.

I will remind you that we installed coupons in there, various alloy coupons. We put microbial samples back in there with the intent of when the test was over, we would go back in and retrieve those and subject them to laboratory analysis. And, then, after that, there will be a post-test characterization drilling program that's currently in the baseline plan for fiscal year '07.

Again, we heat the drift with nine canister heaters, and then 50 wing heaters, 25 on each side, and the boreholes, as you see, throughout both above and below the heated drift drilled from the observation drift, as well as in the heated drift itself where we measure the thermal mechanical, collect water, for water and gas analyses, chemical analyses, as well as geophysics and the hydrological holes to look at the movement of moisture.

Just a status. This happens to be the temperature, the power history for the heaters, as well as the temperature history for one thermocouple that happens to be sitting in the crown of the drift about halfway down the heated drift. And, it just shows that the temperature is around 60 degrees Celsius. And, again, we're a month or so away here from calling the end of the cooling phase.

Next slide. One of the things that we've done recently is--I'm trying to think of a joke, but I can't come up with one. One of the things that we have done recently in the drift scale test is we felt that we had to have additional humidity and temperature control inside the drift, so we've actually installed some new humidity and temperature sensors inside the opening itself, just, again, to get better temperature and relative humidity coverage inside the opening itself.

No surprises in these bullets. The temperature variation within the drift shows that it's coolest near the bulkhead towards the front. You recall that we are blowing air onto that bulkhead, so there is some boundary effects on the test that we have to account for. And, the hottest area is about halfway down. Again, no terribly great surprise.

The humidity is generally consistent, and follows the systematics that we see in the temperature.

In the next slide is some of the temperature history. Since March, up until close to present, the temperatures are shown here along the top, with the scale on the right, just showing the temperature history at different sections of the heated drift, and also some relative humidity sensors at various parts of the drift. You can see some anomalies in the--I call them anomalies--but, some changes in the humidity within the drift. A lot of these can be attributed to changes in the ventilation outside of the bulkhead, actually. So, again, it underscores the point that we are seeing, and this is something that we've continued to account for in our models in terms of how we explain the observations in the drift scale test. We have to account for those boundary effects with the bulkhead.

Another observation that we thought important to share is some of the lessons learned from some of the sensors that we've installed in the test. Very long-term test, eight year long test, we expected failures of the instruments, and, so, we installed redundant sensors, we put in more sensors than we felt we probably needed, for the short-term test.

This is a fairly complicated way of looking at the sensor performance. This particular set of sensors is the resistance temperature devices that we used in the test to measure temperature primarily in holes that were drilled from inside the heated tunnel itself. And, what's plotted here shows the total number of sensors are plotted relative to the axis on the right, shows the total number of sensors, which is up around probably just over 1800 sensors, and, then, the cumulative number of failures.

The important point about this is the nature of the failures. Really, during the heating phase, the nature of the failures was due to basically failures as we expected from just some instruments due to poor wiring, or otherwise. We'd get some level of failures. So, sort of a low baseline level of failures that we expected.

The interesting thing is is that just after we turned off the heaters, and it seems to be coincident with the boiling zone, or just outside the boiling zone, we saw a significant jump in the number of failures of the sensors, and it's likely due to probably actually corrosion of the alloy 600 sheath that protects the nickel wires. That's the working hypothesis. But, it seems to be concentrated, again, just in front of the boiling front, and it seems to have--we saw a lot of failures over the course of on the order of a year, and it's since levelled off, and as you can see by the cumulative curve, we've levelled off.

All of the data that's come out of these suspect sensors is marked according to our QA program. But, I think it's relevant in that, you know, it's important to understand why these sensors failed. We'll try to go after that question when we do the over-coring for the post-test characterization. But, also, I think it's important when we start talking about monitoring things in the repository for very long times, one of the things you've got to think about, which may not be always--that always comes to mind, is how some of these sensors will perform over long times. It's not always straightforward, so, for example, make these instruments retrievable.

The bulkhead investigations. Again, I'll remind you of the map that I showed you back nearly a thousand meters of the cross-drift, which cuts across the repository block, and exposes the majority of the repository horizon rocks. It's been isolated from ventilation for quite a few years now.

Originally, the test was conceived to, I'll use the word challenge, our seepage model. We're going out across the block. It also encounters a part of the block where we expect relatively high infiltration rates underneath the crest of the mountain. So, we were doing a lot of testing in the front part of the cross-drift, and not a whole lot in the back at the time, so, we thought we would bulkhead it off, isolate it from ventilation, and effectively look for seepage.

One of the interesting things that came out of it is is that there was heat sources back there, like the TBM still had power running to it. We still had various transformers back there, and they were providing heat sources that actually were driving some interesting moisture re-distribution processes back there. And, so, it's provided us some very valuable insights on what goes on inside of an opening with heat sources. So, it's been very valuable from that perspective.

This test is winding down. The last entry is going to be in December, and that's really why I wanted to bring it up, is that we are going for the last entry in December, and then we'll be taking the sensors out, doing a lot of post-test tabulations, then wrapping up the test over the course of the fiscal year.

Next slide. Just a representative, what I'm plotting here is the four zones, there's four bulkheads, and, so, these are simply stations in the cross-drift. So, this is 1763 meters from the opening of the cross-drift, to 2200 meters. And, so, we've got four sets of bulkheads, so, four zones. This happens to be a representative thermocouple in the center of each zone.

Just to give you an idea, as we finally started to get at the problem of isolating heat sources and turning off power to some of those heat sources, we started to drive the temperatures from well above ambient in the 32, 33 degrees Celsius range, down to much more like we would expect at ambient temperatures.

The interesting thing is is that there isn't uniform temperatures in all four zones. If you look at the systematics, Zone 1 is actually closest to the first bulkhead, it's hotter than the other three, but it isn't systematically going from 1, 2, 3, 4, and decreasing temperature. You get reversals. And, we're still working to understand, analyze the data and understand why you get those differences in temperature gradients between the zones. Clearly, there's effects of the ventilation. You still get a ventilation effect in Zone 1, almost certainly.

The other thing I'd point out is there's a gap here. This is where we lost power to some of our data collection equipment behind there.

Next. Also, relative humidity. Again, an important point here is that as soon as you close off these bulkheads, isolate the heat sources, you can see that the ambient relative humidity in an unventilated section of tunnel in the repository horizon is close to 100 percent, 99 percent relative humidity, and that's what you would expect, given our hydrologic conditions in the unsaturated zone.

Again, in the back zones, away from the

ventilation, you get close to this 100 percent relative humidity, much more variation and lower relative humidities in the zone closest to the bulkheads. It's also the hottest areas, so you would expect it to be relatively low RH. A lot of noise in the RH here, and I think that probably is at least partly attributed to again this effect of the ventilation blowing on that first bulkhead.

But, an important point about the back zones, you very rapidly get up to those ambient relative humidities, so the rocks, the wet rocks effectively communicating with the drift.

I've got a bullet here. This is data that we've talked about in several Board meetings in the past. When we do these entries, we go in, again, we're not ventilating, we go into these areas, we don't ventilate and we go in dressed up in suits, which some of the Board members did in previous trips, and go in and observe the moisture distribution. Where you see puddles, where you see the moisture accumulating, and have also collected water samples and done chemical analysis. And, all those lines of evidence continue to suggest to us that really what's going on is it isn't seepage into the drift, it's this in-drift condensation, conduction condensation phenomena that's driving the moisture re-distribution inside the opening.

South Ramp. The South Ramp in the ESF over about a couple hundred meters of the South Ramp where the Tiva Canyon--remember, the Tiva Canyon is the cap rock on top of the sequence at Yucca Mountain. So, you have the Tiva Canyon welded tuff, then the Paintbrush non-welded, and then the repository horizon.

In the Tiva Canyon, the cap rock along the South Ramp, late February into March, there was over 200 meters, several areas where there was wet areas observed in the tunnel, and also some areas where there was actually dripping, absorbed dripping. This happened to be in a year, this plot shows a six month average rainfalls for the wetter part of the season at Yucca for ten years, from '95 to 2000. This happened to be in an above average year in terms of the amount of rainfall. Again, the seep started. We first observed them, one of the locomotives driving down the South Ramp actually doing a daily inspection actually noticed the wet areas on February 28th.

The next slide shows a sequence of pictures starting in early March through to late March, just showing the nature of some of the seepage.

We've done chemical analysis of the water. They've mapped all of the observations of wet spots relative to the geology in the area. And, also, Berkely has taken their seepage model and done some analyses to test whether the ambient seepage model as used in the draft LA is consistent with the observation, not to try to predict the occurrence, because we're not doing site specific rock properties, but just to try to convince ourselves that it's consistent with the observation that we've seen in the South Ramp. General conclusion is it's consistent, but that's still preliminary work, and that's being finalized in a report as we speak.

Drift shadow, something that, I'll use the word, isn't in our current basis. The notion that you might--not only will the drift provide a barrier to seepage as water flows through the unsaturated zone, and perhaps provide a capillary barrier, but also the notion that the presence of the drift itself may provide a relatively lower saturation zone under the drift, such that perhaps releases that might come from the drift into the rock would actually be controlled by diffusion rather than invection.

We've got a set of projects being done, actually, this is part of the Science and Technology Program, to test that concept. So, again, we're testing the concept. So, this is longer looking in terms of perhaps if we can prove to ourselves this concept may exist, and that would be something that could be explored and included in our basis downstream in licensing.

Two things that I want to illustrate here. One is we've been spending some time, Berkeley and the USGS in particular have been spending some time trying to think about what are good analogues for this kind of process. This particular site is a sandstone unit in a mine, actually in Northern California, where they found a mine that has two levels of drifts where we're going to drill a series of boreholes, do some geophysics, and compare that to the model predictions that we've put forward using the rock properties at the site itself. This simply shows a prediction of what one might expect for this mine. Again, two layers of drifts. This is just a color scheme showing the liquid saturations, where you would expect the drift shadow to be relatively low in liquid saturations below the drift. So, we're going to go do the work in the field, compare that to the predictions. That's one component of it.

The other component of it is we're looking in the underground at Yucca, we're actually looking at lithophysal cavities as potential small holes that may produce this shadow effect. And, again, this is, again, work that the USGS and Berkeley are conducting collaboratively.

This is just some preliminary examples. What we're using is we're using uranium series isotopes as a signature for flow. So, to put it somewhat simply, these data, these are preliminary data. They're actually plotted, it's basically per mil deviation from secular equilibrium for the U-series isotopes.

So, put simply, if this number is close to zero, you would expect it to be relatively low flow. If it's significantly different than zero, less than zero, you'd expect it to be high flow because it's relatively young water flowing through and resetting the clock. If you've got secular equilibrium, that means you haven't had a lot of water flowing through with uranium isotopes in it, uranium and thorium isotopes to reset the clock.

So, this particular lithophysal cavity kind of bears out the pattern that one would expect if there's a drift shadow in place. I'm not showing some of the data from some of the other cavities. I would say we're still--it isn't obvious that it's occurring like this in all the lithophysal cavities. And, again, this is work in progress.

Next slide. Also, part of the Science and Technology Program, I know the Board is familiar with the work that we've been doing recently at Pena Blanca. This is just a list of some of the highlights of our work at Pena Blanca. Again, we've set up to now collect seepage in some of the adits.

We're also doing some predictive modelling of the UZ and SZ transport at the site, and comparing that to some of the observations from some of the wells that we've drilled, and also some of the wells that Chihuahua has drilled. And, our wells are actually drilled in cooperation with the University of Chihuahua. So, this work continues this fiscal year, and all the tasks, we've delivered the science plan to the Board I believe when it was first developed, and all those tasks are ongoing. So, we hope to continue to collect important data there, and present that to the Board as we collect more data.

We're also working hard to try to get the message out on some of the work that we're doing there. We had a session in 2005 GSA that Ardeth Simmons chaired, and that was well attended.

Saturated zone. Again, the majority of our work in the saturated zone is I'll use the word cooperatively. Nye County has an independent Early Warning Drilling Program that you all heard a lot about from Nye County, Bill Hammermeister and others, over the years. We work from a scientific perspective cooperatively with Nye County. We collect data from the holes that they drill, and it's actually an outstanding scientific cooperation, and we're using a lot of that data in support of our saturated zone flow and transport models. Some of the sorts of things. Basic litho and hydrogeologic framework for particularly the areas down gradient from the repository. A better definition of the alluvial aquifer down gradient of the repository.

Some single hole and cross-hole hydrologic and tracer testing, and I'm going to talk a little bit about some of the recent tracer testing that we've done in one complex.

We've taken some samples and done laboratory sorption measurements for some of the key radionuclides. Eh/pH data to try to get at this notion of what's the oxidation reduction conditions in the saturated zone.

This plot, again, shows U.S. 95, Yucca Mountain would be up here, just off the map. I guess that's the southern edge right there. Again, this just shows the location of the Nye County boreholes as they've completed the three phases of drilling.

Next. A more detailed slide of the same hole locations. We've talked in the past about our cooperative work at the 19-D complex, where we had originally sited our alluvial tracer complex, which is yet to be implemented, but Nye County has done a lot of work there. We've done some work there.

Recently, Nye County conducted a single hole and cross-hole tracer test at the 22-S, 22-PA, 22-PB complex, and that's what I'm going to present, a couple slides on some preliminary results from that testing.

Next. First, this is a very detailed slide that shows the tracer test interval in 22. For those of you all who understand the saturated zone in more detail, if you look at the stratigraphy of the alluvium in 19 versus 22, there's no significant differences, no significant surprises there. It's what we would have expected to see in 22 based on what we learned at 19 and some of the other holes.

So, again, Nye County has set up a three hole complex. You can see the scale here. It is on the order of tens of meters, and they've done both, again, push pull single hole tests, as well as cross-hole tests.

Next slide. First, a little bit--a few results from one of the single well tests. Here, you inject the tracer, you shut the hole up, and you wait for either three or 30 days, and you go back in and you pump it back out.

This just shows the results for two tracers--actually, three tracers, iodide, as well as fluorobenzoic, various sorts of fluorobenzoic acid. The iodide is effectively a conservative tracer. The fluorobenzoic is probably moderately reactive. But, you can use the shape of the curves, normalized concentration versus pump time to back out important hydrologic parameters for the alluvium as located at 22-S.

And, you can see some of the bullets below here. The representation of specific discharge, which is effectively the conductivity as a function of gradient for looking at both peak and mean arrival times, basically looking at the shape of the curve, and backing out these hydrologic parameters. These specific discharge estimates are not terribly different than what we got from some of the testing at some of the other complexes, and is consistent with the bases in the TSPA, as in the draft LA.

The seepage velocity, actual groundwater velocities are actually consistent with what our current basis is. So, again, the single well tests, you look at different tracers of different retardation capacities, and you can back out these important parameters.

Cross-hole is next. Cross-hole tests, here, you inject in one and pump in another. And, again, this is just two ways of looking at it. One is a log log scale, one is a linear linear scale. Here, we've got a wider variety of tracers. You've again got the fluorobenzoic acids. You've got a conservative bromide tracer. You've also got polystyrene microspheres that are meant to represent colloids in the system, and also lithium, which is moderately sorbing in the system.

And, if you look at the details of some of these curves, you can see some actual interesting results in terms of lithium. No surprise in this system. There is significant retardation of the lithium ion. There's some early arrival, but it does bear out that it is moderately sorbing in the alluvium in the Yucca Mountain area, and underscores the importance of the alluvium in terms of retardation in the saturated zone. And, also, some interesting systematics, at least attenuation of the microspheres, and it's telling us something important about how colloids transport through the system. This is preliminary information. It's being evaluated as we speak to be potentially incorporated into the AMRs in the future.

Looking to rock properties, first, let's talk about thermal conductivity. This is something that we've discussed several times in the past. We've done a series of five tests in the underground: in the Topopah Spring, test in the lower lithophysal unit, which makes up the majority of the repository horizon, as well as the other units that make up smaller proportions, like the middle non-lithophysal and the upper lithophysal.

We've completed five tests. They're really single hole heaters, with anywhere from one to three or four temperature boreholes. And, we heat the rock for a period of time below boiling, and then turn the heater up and drive the rock above boiling, and use the temperature measurements and the conduction only model to basically calculate the thermal conductivity of the rock at that specific location.

Next. I've talked about tests 1, 2 and 3 before. We're going to show a complete dataset for all five tests in the next slide. Test 4, again, was in the lower lithophysal, here, down closer to the South Ramp. Test 5 was in the upper lithophysal at the actual turn from the main South Ramp. They both ran for about a year, and, so, they've been off for over a year, and now we're analyzing the data.

Next slide. Just representative temperature plots for the two tests. Excuse me, this is actually for Test 5. This is just showing the crossover. Again, the boreholes are, the heater and the temperature boreholes are perpendicular to each other. So, this is simply the distance from where they cross over. This is the time series. This is for the below boiling portion, below 100 degree C. portion of the test, and the above 100 degree C. portion of the test.

When we analyzed the test results, we also have used the thermohydrologic model to see if there's any significant convective effects. And, the thermal parameters that we back out are actually similar for the conduction only model and the thermohydrologic model.

Next slide. Table, I don't know if you really want to--I won't go through this in gory detail. But, it shows the results of the field measurements, and the thermal conductivities that we calculate using that conduction only model, as compared to the model report where we have calculated thermal conductivity for the different units within the stratigraphy. And, also, down here along the bottom, shows the range of thermal conductivities that we used for the lower lithophysal from wet conditions to dry conditions, as used in the multi-scale thermohydrologic model. So, in general, observation is consistent with the model.

Next slide. We have also spent a lot of effort over the course of the last year and a half or so on geotechnical testing. That's been focused on both surface based drilling around the surface handling facilities, as well as the aging pad area. I'm not going to talk about that today, but that has been ongoing.

We have also done a lot of work using techniques, this is work that's been conducted by Professor Ken Stokey and his students. He's at the University of Texas at Austin. He's done a series of field data collection exercises, both at the surface and in the underground, and that's what I'm going to talk about, a couple slides on that today. And, then, he's also done a series of laboratory analyses as well at UT.

What does this support? You're effectively calculating geotechnical properties, in the case of spectral analysis of surface waves, you're getting shear wave velocities in the rock. And, that's clearly supporting our evaluation of the preclosure and post-closure ground motions.

Again, spectral analysis of surface waves, you either vibrate the ground with a truck, or you hit it with a hammer in the underground, and you have an array of geotherms. You measure arrivals of the surface waves. You analyze those arrival times, and the responses, and you can determine shear wave velocities for the rock.

Next slide. They've done a series of surveys at the surface. You can see those. The most recent surveys done over the last year or so are shown in the kind of gold lines. Again, this is with the vibrasize truck, you go along, set up a geotherm array, go along, thump the ground, and collect your data. The next slide actually shows some of those results. This is for, again, all the 2004, 2005 data. Again, this is preliminary data showing the shear wave velocities as a function of depth again for this more recent data. We're in the process of finishing the analysis of this data, and comparing it to the previously collected data, and determine how that might be incorporated into the seismic design basis.

Next slide. We have also done underground, and here, we used the sources, typically somebody hitting the rock with a hammer. They have done these data collection activities at a variety of sites in the underground. You can see they're all shown here in green. So, throughout the underground, they have done this, focused primarily on the repository horizon, as you can see by more of the green in the cross-drift, again, heavy emphasis on the lower lithophysal since it's the majority of the repository. Go back one second. Also, concentrating on integration of their efforts with what the USGS has done in the underground, looking at detailed fracture densities, the nature of fractures, fracture orientations, et cetera.

Next slide. Results of some of the underground data. This happens to be for the lower lithophysal. Again, the scale is different. You're using a hammer as a source here. This shows the shear wave velocities as a function of depth from the source. Again, these are for all the lower lithophysal samples, again, the more recent data.

The same thing with the surface data. We're in the process of finalizing the analysis of this data and comparing it to our existing design basis.

Switching gears from the UZ/SZ, rock properties, and what's going on in the underground at Yucca Mountain, now, I want to talk briefly about what's going on in the igneous processes. I want to start with probability.

As you all are aware, there are six quaternary basaltic volcanoes within the Yucca Mountain region, within 20 kilometers of the repository site. You recall back in 1996, we finalized the Probabilistic Volcanic Hazard Assessment that is effectively the basis for our current disruptive events models, at least in terms of volcanism.

There were eight buried anomalies at the time of that initial PVHA that we knew about from previous aeromag surveys. The USGS and Nye County a couple years ago finalized an aeromag survey of the area, and found additional anomalies. We have since gone in and done some more detailed aeromag surveys, and identified up to 15 additional anomalies in the Crater Flat area. So, the next step was to, okay, let's now pick the right anomalies that would be relevant in terms of an update to our PVHA, and let's go drill those anomalies and see what we see. If we see basalt, age dates those basalts. So, that work is ongoing as we speak.

Next slide. This is just the results of our aeromag survey. Again, the repository here at Crater Flat, let me get this right, Black Cone, Red Cone, Little Cone, Lathrop Wells down here. There's the repository. U.S. 95 runs right about there. This just shows the anomalies. We chose to name them by letters of the alphabet. And, again, we're in the process of drilling some of those anomalies. We just finished the third drill hole last week.

Next slide. A little bit more detail on the three anomalies we've drilled so far. We've drilled Q, A and O, which is just off the map here, again, Black Cone, Red Cone, Little Cone. Yucca is just up here. We're moving to Anomaly I next, I believe, which is actually off the map. It's actually way off the map. It's down by Lathrop Wells and U.S. 95. Some of the anomalies we actually modelled one way and saw something different, and in other cases, we were actually right on with what we expected to see.

Anomaly Q was a new anomaly we had modelled as a tuff. We actually saw basalt. It's similar in character to the 11.3 million year old flows that we had dated previously. This is currently in the laboratory for age dating.

Anomaly A previously recognized as a buried basalt, we found the basalt and it was actually close to within the target of where we thought we would see it. When I say modelled as, that's from the geophysics.

Anomaly O, the one we just completed last week, we modelled as a tuff, and we saw tuff actually right in the middle of where we expected to see it.

So, again, it provides the confirmation of buried basalts, or actually either confirming or revising what we thought the interpretations were from the geophysics. And, these are all being age dated. So, that's being completed as we speak. That will all be rolled up and provided to the expert panel as we update the PVHA probably in fiscal year 2007.

Consequences. I talked about probability, the update of the PVHA that's waiting on the result from this age dating. There's also been a significant effort on looking at the consequences of an igneous type disruptive dike, perhaps intersecting the repository and perhaps disrupting packages within the drift. So, we've done a lot of work on developing the fluid dynamics code that we use to represent the pyroclastic flow if a dike intersects a drift, the pyroclastic flow that propagates down that drift, what's the nature of that multiphase flow. Also, can more than one drift be affected by a dike that intersects, say, one drift.

With the influence of the backfill. Recall our current design. We have backfill in the main drifts. We've also got a cap at front of each drift to try to help mitigate, using engineering to mitigate the effects of igneous disruption of a dike intersecting the repository.

We've got some preliminary results that confirm our assumptions in terms of how fast the flow might freeze if it travels down a drift, and we're in the process of starting to think about how the drip shield and packages would play into that analysis.

And, then, finally also thinking about starting to do an analysis on the influence of structures, faults, and how that might control dike propagation in the area.

Next slide. Okay, now, moving completely out of the natural system, and I should have said at the beginning, I'm not talking about corrosion. I think you're going to hear way more about corrosion--well, maybe some of you want to hear a lot about corrosion--but, you're going to hear a lot about corrosion on the drip shield, and localized corrosion of alloy 22 from other people, so I left that out on purpose, thinking you're probably going to hear plenty about it later.

But, I do want to talk about the source term a little bit. Source term, again, now we're talking about what's going on inside of the waste package prior and after one would breach. What are some of the processes that you have to think about when you think about the source term?

Well, clearly, you've got to think about how the waste form corrodes, in equilibrium sense and also kinetics. The waste form meaning a high-level waste glass or spent fuel assembly. As it corrodes, it forms secondary alteration phases. What role do they play in release of radionuclides? You've got a package under the current design that's got a large stainless steel--a large amount of stainless steel on the inside. As that degrades if you breach a package, how does that affect the chemistry inside that package?

What do the alteration products, how do they play in terms of sorption, potential sorption? And, then, finally, the formation of colloids, say, from the degradation products, for example, from the degradation of the waste form, how do they play in?

Just a picture. This happens to be from a paper by Bob Finch and Rod Ewing. This is actually just a photo of corrosion of uraninite. But, effectively, when you talk about spent fuel corrosion, you're talking about UO_2 in an oxidizing environment. So, that's why Pena Blanca is so interesting, for example. It's uraninite deposits in an oxidizing environment.

The pirogenesis, when we go and do spent fuel dissolution experiments, corrosion experiments, the pirogenesis that you see in the laboratory, the sequence of alteration phases that you see forming is actually pretty similar to what you see in, say, a Pena Blanca setting.

Next slide. One of the important questions that we've been asking ourselves really for a long time, so, we've started some focused experiments over the last year or so, is the fate of neptunium.

As I think you know, and when you look at our performance assessment results, really, as long as I've been on the program, VA, site recommendation, SSPA, up to now, neptunium is a significant contributor to dose. The actinides are significant contributors to dose. And, so, we've been spending a lot of effort trying to understand what controls the solubility of those actinides as you're corroding spent fuel.

One of the fundamental questions is is what's the role of the alteration phases. As the fuel alters, does it have any control on controlling the solubility of the actinides, particularly we're going after neptunium specifically are pretty heavy right now. And, then, that said, what's the controlling--even if you think about neptunium solubility control as a pure phase control, what's the important oxidation state in the pure phase? Is it neptunium-5, is it neptunium-4?

If you look at the results, the PA results, when you look at control by Np2 05, or neptunium-5, versus Np02, neptunium-4, versus control by alteration phases, it has a pretty dramatic effect. That's the solubility models, the assumptions have a pretty dramatic effect on the dose. So, we've been spending a lot of effort trying to understand what's the right model to use.

So, this is just some results. These happen to be experiments that are being done at Argonne, where we've actually spent a lot of time carefully taking samples of some of these spent fuel corrosion samples that actually develop a microcooling technique, where we've taken systematic core samples, small core samples across the spent fuel corrosion alteration product interface, and we're doing detailed measurements using advanced proton source to look at the neptunium, uranium, plutonium concentrations, and the variation in redox state.

And, the preliminary results suggest that to boil this down to the fundamental, we hope what will be the fundamental conclusion is that neptunium-4, Np02, looks as if it could be in fact the controlling, pure phase controlling solubility of neptunium. That's an important observation.

Another question is, okay, if you have neptunium in solution, one of the arguments that's been made over the years about why Np02 or Np4+ in the oxide state would not be necessarily controlling phase for neptunium solubility is there may be kinetic--it may be kinetically inhibited from forming from solutions.

So, we're also doing a series of experiments to look at the kinetics of Np02 formation in these solutions, and we're effectively taking the technique that was shown by Roberts, et al. at a relatively high temperature, and we're extending that to both higher and lower temperatures. And, ultimately, we're also starting to include uranium into the system to look at that redox couple between uranium and neptunium. Again, ongoing work to look at our basis for neptunium solubility.

Finally, I mentioned alteration phases, the previous experiments were focused almost exclusively on looking at this issue of Np4+ versus Np5+, Np02 versus Np205. We're also, in the Science and Technology Program, starting to probe what is the influence of alteration phases.

This particular plot is the results from Peter Burns' work at Notre Dame. This happens to be a paper in 2004. We didn't actually--Yucca Mountain didn't actually pay for this work. But, I put it up here because it's published and also because we are in fact funding Peter and his grad students to do a lot of the follow-on work related to this.

But, what's shown here is, again, the neptunium content of crystals in the presence of a neptunium bearing solution. There's probably--the only person who understands the suite of uranium minerals in this whole--it's a pretty obscure group of minerals. But, you've got a lot of names that even the everyday mineralogist doesn't recognize. But, they actually form very fascinating structures, and there's tens of these structures, and Peter Burns in particular has spent his career understanding these structures.

But, what's shown here is they tend to form either framework structures or sheath structures. Uranophane and the compreignacite are actually, I'll call them framework structures, or sheath structures, where you get either sodium or calcium in the interlayers. Whereas, the meta schoepite and the beta uranium hydroxide--uranium oxyhydroxide, they tend to form these sheaths, without any cations in the interlayers. And, what Peter is saying is actually when you start to look at--actually, there doesn't appear to be a lot of neptunium incorporation into these structures, like schoepite and meta schoepite, but if you look at some of the structures where you have cations in the framework or in the sheath between, you probably get a charge--that allows you to update neptunium into the structure.

Now, again, if we can demonstrate that the alteration phases and the right alteration phases are in fact controlling neptunium solubility, that provides--that's probably a reasonable model and it probably also provides significant retardation of neptunium from the repository.

Next slide. So, I hope I've given you at least an idea of some of the ongoing work. Science continues. The data is being used to support the models that are in the draft TSPA, that will clearly be used for performance confirmation and any future licensing activities as we move ahead.

And, I'll be happy to entertain questions.

GARRICK: Are we live here yet? I think one of the things that's very encouraging about your summary is, of course, the work that's trying to get a better handle on radionuclide behavior, and, in particular, the source term work. But, I'm still wondering why we don't see more effort with respect to the unsaturated zone and the saturated zone with respect to specific radionuclides in terms of retardation and retention. And, I'm thinking of the information you presented with respect to the analogue, where you indicated that the isotopic studies that were done, the migration rates of the isotopes were orders of magnitude less than the groundwater flow rates. So, suggesting that groundwater flow is not a very good surrogate for radionuclide transport.

Obviously, it's a good carrier. There's no question about it. And, it provides you with some information. But, I'm surprised to not see more effort in trying to better understand the radionuclides of interest and how they behave in the geologic media, in particular, the unsaturated zone and the saturated zone.

Source term is critical, but I don't think we're still giving ourselves as good an indication of the performance of geology with respect to the transport and behavior of specific radionuclides maybe as much as we should. Why don't we see more radionuclide specific R&D in transport and retardation and retention in those zones.

PETERS: Well, I agree with you, first of all. It's important, and I'm not sure I'm going to convince you that we've got it, but I probably didn't do myself any favors, because there is work that, for example, is going on in the Science and Technology Program in the saturated zone and the unsaturated zone that I didn't present, that in both programs that probably might have under-represented what we're really going after there. I won't sit here and tell you everything that we need to do.

GARRICK: We've heard many times that, of course, in the unsaturated zone, and this is not my field, as you can tell, but we've heard many times that in the unsaturated zone, that fractures in faults are the primary avenues by which the flow takes place. But, on the other hand, we're also led to believe in some presentations that the contribution from matrix diffusion in the unsaturated zone could be of a much more significant contribution than we're seeing. And, all in the spirit of realism and realistic assessments, and not hearing a great deal in the Science Program about that specifically is the basis for the question.

PETERS: Okay. Well, in the case of matrix diffusion, we do have on the order of three or four projects in the Science and Technology Program looking at that exact issue. I guess my first suggestion would be that maybe we need to provide you a more complete picture of what's in that program before, so at least you're informed.

GARRICK: But, the way it seems to be discounted is that the partitioning between the fractures and faults and the matrix is so much in favor of fractures and faults that you get the sense that there's not a great deal of interest in trying to quantify--

PETERS: It's a difficult problem.

GARRICK: --the contribution of the matrix diffusion. That was my comment.

PETERS: Okay.

GARRICK: Okay. Thure?

CERLING: Cerling, Board.

Just two questions related to your discussion of EH and sort of conditions, and one I guess at 43, just two slides back. In these last couple of slides here, and the couple before this, you talk quite a bit about the coupling between uranium and neptunium, and I was just wondering what else is going on with the other sorts of things that are on the inside of the canisters that could contribute to secondary mineral phases, iron, and other minerals.

PETERS: There's a stainless steel basket degrading in the iron oxyhydroxide, so that's--

CERLING: So, I was just wondering what sort of--is there any research going on in that, and how that will affect neptunium and migration of other products.

PETERS: And, the answer is yes. This bullet is vague. For example, adding uranium and iron to the system to look at that exact--at those kinds of questions.

CERLING: Okay.

PETERS: And, then, also the parts of the Science and Technology Program that I didn't discuss were looking at those alteration products, how they affect the redox commissions in the package, even in the breach package, could it be a reducing environment inside there, questions I think that are pretty obvious to you. And, those have direct relevance to, say, the oxidation state of technetium, for example, and how it might transport. So, those are the sorts of questions that we're also asking in the slides that I didn't show here today.

CERLING: Cerling, Board.

I hope we hear about those in the future. And, then, on slide 18, you mentioned in your Number 6, Eh and pH, and I was just wondering what's going on with respect to that. That's in the saturated zone conditions.

PETERS: Right. You don't need to come up, but if I'm wrong, you guys just started the project at UNLV to look at the Eh, effect of the redox commission in the saturated zone. So, that's the project we've just started really in the summertime. And, then, I know the project's also been thinking, we're looking at some of the Eh data and rethinking whether or not we do in fact have some evidence maybe for some reducing conditions. So, we kind of going at it from two fronts. So, I would say that's a work in progress we need to keep you informed on.

GARRICK: Ali?

MOSLEH: Mosleh, Board.

My question is a little bit more programmatic than technical specific. By one account, like 2000 that go into the TSPA type calculations, I was wondering if you could comment on the criteria for selecting the types of issues or parameters that are subject in the S&T Program?

PETERS: No, I understand the question. I'm not sure I'm totally--I'll give my answer, and then Bob Andrews can probably give maybe a better answer.

Now, let me talk about the testing program that we've been doing on the project for all these years. It's been focused, first of all, the thousands of, however many parameters there are, there's, what, 200-something uncertain parameters, and, so, clearly, you want to think hard about the uncertain parameters, but I'd say really our program has been informed, risk informed with what are the areas where we need to demonstrate--that provide significant performance, and that's been where we focused our testing program. I mean, we've looked at all the barriers, but I'd say really it's in the last three or four years it's been informed by the risk. TSPA has played a significant influence on that.

Now, the Science and Technology Program, we've kind of taken a step back and said okay, you know, that's all good and fine, but now let's think about--the TSPA is an important consideration, but let's think about where we can demonstrate understanding, and where, like John was driving towards, for example, in the unsaturated zone, matrix diffusion. If we can demonstrate that there's significant matrix diffusion in the unsaturated zone, that could provide significant performance benefit.

If we see something that surprises us in the opposite direction, that's science. So, I would say the short answer is TSPA has been a heavy influence. Really, since I've been involved in the last four or five years.

GARRICK: George?

HORNBERGER: I think that we really do appreciate the science results that you've been describing, and I can certainly see that TSPA has informed some of the things you've gone after. I'm not so sure that we're convinced of how the results that you're showing us play back in to an analysis, and I don't know if you were here this morning, but John Garrick, in his introduction, had indicated the Board at some level would certainly appreciate seeing some kind of parallel effort, where some of these new results that you're getting get fed back into a more realistic assessment so that we could have some better quantitative understanding of what's going on. That's a comment. But, I do have a question.

You mentioned that you were doing some work on consequences of an igneous event, and that you have some preliminary results. Your slide 38 says that preliminary results confirm assumptions that magma moving through a 30 centimeter gap will freeze in approximately 500 meters. And, I wonder if you have--is this just computations? Because, for example, my friend, Bruce Marsh at Johns Hopkins, has lots of evidence from the geological record. His magmas, or his defaults in Antarctica, things freeze pretty quickly. This stuff does not come up, you know, super-heated. It's right around--it's pretty easy to freeze. So, one aspect is are you looking at sort of geological analogues to help you understand whether you're right on this.

The second thing is, and I know part of the whole TSPA modelling assumes that the canisters in interacting with magma are somehow going to disintegrate and be carried up to the surface, and Bruce Marsh tells me that there's no way in hell that that will happen. Those canisters are cold, and they will very rapidly get solid rock coating around them, and we have so much evidence that that will happen. Are you looking at freezing magma that way as well?

PETERS: I'm going to probably ask someone to stand up and help me with the analogue part. I know they look at analogues to address various aspects of the problems, specifically about how long it takes to freeze, how fast. But, I do know they use them to some extent.

On the second one, let me start by saying to my knowledge, we're not doing any experiments on, for example, the durability of the package inside--your point is well taken. I mean, EPRI stood up in front of you and said basically the same thing. I don't know what else to say about that.

But, Mike, do you or Greg want to say anything more about analogues with consequences? Is there anything with that?

COIN: I'm Mike Coin. I'm the manager of the Igneous Activity Group for BSC.

With respect to the magma freezing, we are looking--we are using analogue information as input to our analyses. I think it's a 3-D analysis done by ITASCA. It looks at different temperatures of magma coming in. It looks at the viscosity of the magma, and various aspects associated with that.

They're also going to go back and look at the effects of the waste package as the magma moves down through the drift, the chilling effect of the waste package, we'll be looking at that as well.

GARRICK: Andy?

KADAK: Kadak. You have a lot of activity going on quite apparently, and I guess what I was looking for is what have you learned from all this activity? And, I don't know whether it's appropriate now to go item by item, but I'd sure like to know what you learned from the block test. I'd like to know what you learned from the bulkhead test. I'd like to find out what you know, or what you're doing, rather, as to water flow in the repository, given the heat conditions.

So, could you tell us a little bit about those things?

PETERS: This will be Mark Peters version of the implications of the testing program. So, I'll do it on the fly. Is that okay?

KADAK: That's fine.

PETERS: Drift scale test. Temperature, conduction dominant.

KADAK: I'm sorry?

PETERS: Conduction dominant, mode of heat transfer is conduction in the drift scale test. Surprise, Surprise. We tried to pond water above the drift. We actually drove water. We set the heaters up so that we tried to do that. It's freely draining.

KADAK: Everywhere?

PETERS: Well, in that heated--in that drift scale test, and there's no reason to believe that it wouldn't behave similarly in other places. But, in that specific test, we actually set it up and configured it such that we were trying to see if, in fact, there would be a significant storage of water above the drift, and we were unable to do so.

KADAK: You couldn't get water up?

PETERS: Well, it went up, but it drained inside.

Mechanically, the rock behaved as we expected. We moved a lot of water around. No surprise. We actually learned quite a bit about water chemistry and gas chemistry from the heater tests. Actually, as an aside, it was somewhat fortuitous. We didn't have chemistry sampling holes in our smaller single heater test, and we were gathering water in one of our holes, so we quickly configured our holes for the big test to sample water, and we learned a tremendous amount about water chemistry in the thermally altered area. So, I'd say that's probably the biggest.

KADAK: And, how does that compare to what you've assumed?

PETERS: Well, we've actually evolved what we assumed based on those test results. I would say when we started collecting that water, we were technically assuming it was J-13 that was contacting the back, J-13 water, so we've learned a tremendous amount about how water evolves in the thermal setting from that test.

Let's see, bulkhead experiment? We didn't see any seepage, but it sure told us there were a lot of things going on. Again, this is probably not a surprise to you, but just bear with me. We saw a lot of problems. You've got a heat source inside an open tunnel in an unsaturated zone. You've got a lot of convection and condensation processes in there. That required us to go and think about features, events and processes, for example, condensation in cold spots in the drift, and things like that. So, that's probably the biggest thing there.

KADAK: As I remember, that was part of the Science and Technology Program, as well; right?

PETERS: No, that's part of the--well, there's an analysis just confirmed by the Science and Technology Program, so we're looking at some of those same processes.

But, that was speaking in specific with the task.

KADAK: But, this condensation and vapor transport I think is really important in the modelling of the repository.

PETERS: Actually, it was.

KADAK: So, what is it that you think is happening now?

PETERS: There's are uncertain processes that we're getting convection and redistribution of moisture and condensation in cold spots in an uncertain manner. It's a complex process.

KADAK: Okay.

PETERS: And, so, we've had to really carefully think about how we incorporate those processes into the model, and if we don't, have good justification for why they won't occur, or they have no consequence.

Let's see, where am I at? Do you want me to go through one by one?

KADAK: If you could. I mean, it would be instructive.

PETERS: Thermal conductivity, we were using lab scale measurements for a lot of years. Is this okay? We were using primarily lab scale measurements, and you can think about the whole host of scaling issues that you would have with that when you talk about a fracture. So, the field tests I think have really actually, I would argue, confirmed our understanding of how certain thermal properties are with the rock.

I think we can say the same thing, we've done a lot of scaling from the laboratory scale, the field scale with the rock properties in the last three or four years, and I think we've learned a lot. It's confirmed really what we were assuming.

Igneous, I mean, you go through a more detailed aeromagnetic survey, you find more buried anomalies. The key is how old are they. We're going to have to go find that out.

Oh, I missed the saturated zone. The importance of the alluvium in terms of retardation I'd say is an important observation. You would expect the alluvium to have a lot of sorption in the alluvium, but we didn't have field evidence for that because we hadn't done the tests. We benefitted tremendously from the Nye County on there, and the cooperative work.

KADAK: How about that South Ramp?

PETERS: Yes, I would say--we never saw water there. I guess when we were mining through parts of that, particularly at the base of the Tiva Canyon, we saw--never free water. So, the fact that it was still being ventilated to some extent, I don't think anybody expected to go down there and find water. But, when you look at it and you go do the analysis and look at where our seepage model, what you would expect to see, you could expect to see some seepage given the precipitation rate. You don't have the amount of water between there. You're talking about a fracture drop, and that's to the surface. It's only 40 meters down, the kind of precipitation we saw.

KADAK: You didn't see any deep further down?

PETERS: It was only in the part of the tunnel that was close to the surface, so you only had like 40 or 50 meters of rock over your head. We didn't see any deep down where you were under PTM.

Source term. We've done a lot of corrosion experiments on rocks and have learned a lot about how it might alter. But, the source term models have always tended to, for good reasons, we had assumed pure phase solubility control. So, how you could do a solubility control for the actinides, for example, is controlled by their pure phases. What the role of alteration phases might be, I think if you got experts up here, they would tell you that it's likely that alteration phases play a role. But, we don't have the basis for--it's a difficult problem. I think we're on the path of being able to perhaps have done that.

So, I'm sorry if I didn't give you a good setting for how this might be feeding into the models. That's good feedback I think the next time we stand up. Whoever does this needs to show you, you know, an arrow that points you to the right part of the model, and how it affected the analysis. That's good feedback.

KADAK: Just a quick scheduling question. Suppose this reprocessing program takes hold and the waste forms would considerably be different than, say, spent fuel, or perhaps even in the new fast reactor system, you'll have different waste forms. How much more additional analysis or modelling or experiments would you have to do to get to the same state that you're in now, and relative to understanding how spent fuel behaves? Are we talking a five year program?

PETERS: I mean, this is now my estimate. This isn't a Yucca Mountain issue, but I've thought about it some when I spend my time in Argonne thinking about reprocessing. To qualify a waste form by different--you're talking five to ten years to qualify that waste form.

KADAK: And, then, understanding how it might--

PETERS: Right. Right. And, the difference is it becomes potentially--you know the details.

KADAK: Thank you.

GARRICK: David Duquette?

DUQUETTE: On slide 9, if I heard you correctly, I think you said some of these, or many of these areas were due to corrosion of the sheath on the--

PETERS: That's the hypothesis.

DUQUETTE: And, that was alloy 600?

PETERS: Correct.

DUQUETTE: And, that occurred in how long?

PETERS: In how long?

DUQUETTE: Yes.

PETERS: Well, we started this, we saw the failures, as you can see, over about a 12 month period.

DUQUETTE: They were in play for about 12 months?

PETERS: Yes.

DUQUETTE: Total?

PETERS: No, they were in play for five years.

DUQUETTE: For roughly five years?

PETERS: For five years.

DUQUETTE: So, you're getting corrosion to the point of failure in sheaths of alloy 600 in a five year period.

PETERS: That's the hypothesis.

DUQUETTE: And, no one has done any analysis on that?

PETERS: We can't. They're in the hole. We're going to go in and do the characterization after the test, try to over-core a hole a pull them out and see if that's, in fact, the case. I was just trying to tell you what the working hypothesis was.

DUQUETTE: No, I understand what you're trying to say. I just was curious, because if you're getting failure of alloy 600 in the somewhat benign environment that you think you have in the vault--

PETERS: I'd be careful about saying it's a somewhat benign environment. You've got Teflon in there, you've got grout in there. It's not clear to me that it's even--it could be an environment that's not terribly relevant to our corrosion problems. So, I think you've got to let us drill the holes, go in and see what really happened.

DUQUETTE: I thought you've already extracted some.

PETERS: No, I'm sorry, no, that's coming.

DUQUETTE: Okay.

GARRICK: Thure, then Bill.

CERLING: Slide 13. Cerling, Board.

I was just reminded by Andy's nice comment about feedback, and I was just wondering on the issue of the seep that showed up last year, because we've heard so much about capillary barrier, and I was just wondering how the lessons from this seep are feeding back into our understanding of capillary barriers.

PETERS: I tried to get at that a little bit with the discussion on the preliminary modelling analysis. A lot of the character was wetness along the walls as opposed to drips. The analysis was set on principally--did at Berkeley against preliminary, it wasn't to predict the observation, it was just to see if it was consistent, but, in fact, suggests that you could see this kind of precipitation rates with broad assumptions about the rock properties. They didn't do site specific rock properties here. You would expect to see some percentages of drip that would be seeps, so it's not inconsistent with our modelling.

MURPHY: Bill Murphy. I have a couple questions. First of all, I'd like to say that I'm very pleased to see the work on secondary phases. That's an issue that's been of interest to me for a long time. However, some of these data are new to me, of course, so perhaps you can confirm my sense of an indication I seem to see here on Page 42. You showed the neptunium to uranium ratio is across the boundary between spent fuel and an alteration phase, I guess B is some oxidized uranial phase. And, actually, this looks rather discouraging to me, because you have up to about a part per thousand neptunium in the fuel, and almost none in the secondary phase. It would suggest very strong exclusions of neptunium in the secondary phase.

And, in slide 44, we see the exchange experiments with neptunium which shows they can correspond into the secondary phases, that in fact these look like enormous aqueous neptunium concentrations. These are .2 milligrams per milliliter. That's 200 parts per million neptunium. And, extrapolation of these data, any realistic neptunium concentrations, seems a little discouraging to me, in terms of sequestration of neptunium in secondary phases.

PETERS: Okay. Well, that's disappointing that it's discouraging to you. Go back to 42. I'm not sure what to say, but other than we're doing the work. We'll see what comes out. But, on this one, that's been kind of the problem, is we haven't been able to find neptunium in the alteration. This is from the drip test.

MURPHY: Yes.

PETERS: We haven't been able to find it in the alteration phase. And, so, that's why you go--then, you've got Peter Burns and others looking at it from the perspective. They're doing hydrothermal bomb experiments. You probably know this. But, in very high concentrations.

One of the questions that you have to ask of Peter's work is neptunium is associated with the phases, but is it actually incorporated into the phases. That's a fundamental question as well. So, he's going to the APS to try to get low concentration. Part of that is driven by the analytical technique, as you know. So, he's also going to go to lower concentrations, maybe go to the APS to get higher sensitivity--lower detection limits, excuse me, and try to go after that problem. But, I mean, you're right.

And, then, the other problem is it's not in the schoepite, in the uranophane, and then we've got to deal with the issue of when do these phases form, when is the neptunium leaving the fuel, stuff that you understand. It's a complex picture.

MURPHY: Thank you.

GARRICK: Any other comments from the Board? The Staff? Yes, David?

DIODATO: Diodato, Staff.

Mark, thanks again for your presentation this afternoon. I had a number of questions, but I'll limit them. First, on slide 2, you mentioned performance confirmation is kind of a driving factor here in these analyses. To what extent do performance confirmation plans exist that would help to guide your studies here?

PETERS: Well, Debbie presented--she presented the plan as of--that was last meeting wasn't it? In February, she presented the current thinking of the Department on performance confirmation. I mean, I think we've got a plan that's fairly mature for the state we're at.

DIODATO: So, that performance confirmation plan is pretty well together for--

PETERS: I mean, for the state we're at in the program, yes. I mean, there's a lot more to do. They're just now starting to think about how to go about doing detailed planning for those activities. There's a plan in existence. Debbie's presentation I think is generally probably consistent still with where we're at.

DIODATO: Thanks. On slide 15, the drift shadow analogue ideas is interesting and kind of exciting, the Hazel Mine. And, that sandstone, is that fractured rock?

PETERS: Yes. They're not close enough that I can see them. Yes, they're fractured. They don't look to be totally open from the picture. Bo could probably speak to the detail of the fractures more than I could.

DIODATO: Yes. I mean, I usually think of sandstone as mostly a porous medium type.

PETERS: You can see fairly large spaced fracture networks.

DIODATO: Yes. So, it's a compelling analogue in that sense. And, then, the question is in the modelling the program uses for simulating the dryout, it's mostly the fractures themselves that dry out. And, so, the question is we don't really have any existing method known to the science to measure fracture saturation, but you've got predictions there, and I don't know if your prediction is gross saturation in porous media and fractures, or if it's just fracture saturation. How would you confirm the fracture saturation dryout if we don't really have a mechanism for that measurement?

PETERS: Well, it's a good question. Based on looking at the scale, this is probably matrix saturations, first of all. You know, being able to measure saturation, I can't answer your question about how you measure saturation fractures, you know, we're going to do the geophysics and the conceptual models are going to have to assume a certain coupling between the fractures and the matrix. But, you're going to have to try to use the geophysics to match the overall saturation and try to make some sense out of it.

DIODATO: Okay, thanks. The other issue is something actually you didn't talk about, and the Chairman brought up the idea that the Board is interested in fundamental understanding in the opening remarks. One of the things that's been out there is the Chlorine 36 problem, and I wondered if you wanted to tell the Board what the status of that investigation is at this point. It's been ongoing for a number of years now.

PETERS: I thought I would be able to stand up here one time without having to talk about that.

The project's work, the USGS and Los Alamos, is in the final--they've got a draft report that's in the review process to document what we did over all those two or three years on the problem. The UNLV, I'll call it independent study that we started is ongoing. I don't know the details on what they've done and results. They've taken extensive samples. I don't know if you've heard much about how they took the samples, but they took them from locations, Ghost Dance and the Drillhole Wash, they mined in and took out large samples. They've also taken hand samples. So, they're in the process of doing the lab work on those.

DIODATO: Have they told you when they expect to have analysis of these samples?

PETERS: No. I don't know if anybody has heard anything from them. I mean, it was originally conceived as an 18 month study. There were delays in getting them in the underground because we had other issues with doing work underground. So, they just took the samples here in the last three, to four, to five months. So, you're probably looking at, I would think, you know, a good year or so.

DIODATO: Can I just ask a clarifying question? What are you trying to show with these samples?

PETERS: You're familiar with the Chlorine 36 issue at all, or do you want me to start from scratch?

DIODATO: Just--

PETERS: Okay. Well, we started--what we started to do probably four or five years ago was go in and validate the occurrence of bomb pulse Chlorine 36 in the underground. And, when we went in to do that, we so far have been--found it difficult to reproduce the data. And, so, we were dealing with a lot of comparing between two laboratories, between the USGS and Los Alamos, and, so, finally, we decided, the Department decided to document what they have done in the validation study, and actually have UNLV do an independent evaluation, collect additional data samples, and do the work.

DIODATO: So, if it isn't the bomb test Chlorine, it's something else?

PETERS: What the validation study found is they couldn't reproduce the bomb pulse, and the bomb pulse would suggest presence of fast pathways. The UZ model accounts for fast pathways. In fact, it doesn't--our models still conservatively represent the closest. However, it clearly calls into question how well we understand, and, so, that's why we have continued to pursue it. UNLV, the people involved, some of whom are involved in the fluid inclusion work, look at the other hypothesis. And, so, they've done a good job with these tests.

DIODATO: Thank you, Mark.

GARRICK: Any other questions from Staff?

(No response.)

GARRICK: Or second questions from the Board?

(No response.)

GARRICK: All right. Well, Mark, thank you very much.

We're now going to turn the discussion leadership over to David Duquette and Ron, I guess, on the next speaker. So, go ahead, David.

DUQUETTE: There are going to be three presentations. I'll introduce the speakers just before they come up to the podium. But, I'm going to ask that all three speakers speak before the question and answer period, because each one can only be 15 minutes long, and I think it will break up things too much, and they're all tied together anyway. So, I'll ask the three speakers to come up, one at a time, and the first speaker is Michael Anderson, who is a Bechtel employee. His expertise includes thermal hydraulic and structural analyses of systems and components, nuclear criticality, fuel cycle and reactor performance analysis, and other nuclear reactivity issues. Currently, he's responsible for the design of the waste package ancillary components, such as the emplacement pallet and the drip shield, and a wide variety of design and thermal analyses.

Dr. Anderson?

ANDERSON: Thank you.

I'd like to point out that this presentation is divided into two parts. One is about the configuration of the drip shield. The other is the emplacement devices. In the interest of having three presenters rather than four, I'll be presenting information on the emplacement devices that Robert Thornly, who's an engineer for those devices, is in the audience and able to answer any detailed questions about it.

Let's go on to the next slide, please.

The drip shield, to start out with, looking at its requirements, it's fundamentally a post-closure device. It's installed just before closure of the repository. And, so, it doesn't have any important safety functions. Rather, it merely must facilitate installation and not preclude the retrieval of the waste packages, if that should become necessary.

It is, though, an important device in terms of isolation. And, from a design perspective, we want it to prevent seepage from entering the drift from water dripping onto the, you know, prevent it from water dripping under the waste packages, and also prevent, or protect the waste packages from direct impingement of rocks that might fall, for that matter, be ejected from the sidewalls.

You can see the drip shield, parts of it, basically, the sheaths or plates are made out of Titanium grade 7, and Dr. Gordon, whose presentation will be giving you more information on the motivation for the selection of particular materials. The connectors, the bulkheads, the beams, the stiffeners, are all made out of Titanium grade 24, which is a stronger material, and provides the structural members for the waste package.

I should point out that there's three stiffeners that run down the axis of the drip shield here. You can't see it in this particular illustration, but they provide additional resistance against rockfall.

There's these bases and stabilization pins, which are Alloy 22. The intent there is to protect the Titanium from the rusting metal that's in the invert in the cross pieces.

Drip shield dimensions, it's a fairly large device. It's 9 1/2 feet tall, 19 feet long, and weighs about 5 1/2 short tons. So, that is a very large, heavy structure. By comparison, the waste package weighs about 50 metric tons on the average, so, you've got 5 metric tons versus 50 metric tons, about 10 percent of the waste packages.

Let me hold on this for just a minute. I'd particularly point out here this lifting feature. This will become important when you talk about the emplacement devices. That's the device feature which is used to maneuver the waste package down the drift and place is over the waste packages in place.

Next, please. Of special interest to the Board in the past has been the amount of clearance between the various waste packages and the drip shield. So, you can see the largest diameter waste package, which is 5 DHLW inside of the waste, DOE co-disposal waste package; the smallest, the 12 PWR waste package, you can see the large amount of differences in the free volume. This distance here has been set based on rockfall evaluations, so that the largest credible rock and highest energy rock will not result in the deformation where the drip shield will actually contact the waste package there.

I would also point out to you the slight strain of the legs of the drip shield. That's used to help lock the adjacent drip shields in place. And, if you go onto the next slide, there's an illustration where the actual connection takes place. You can see a drip shield that's already in place. You have a drip shield that's being brought down the drift to be lowered over the top of the adjacent one. You see this breakage here, just in interest of keeping it all on the same page, we put things closer together. This is actually 40 inches.

So, if the waste package is 9 feet tall, that's a little over a third of the total height of the waste package. It's necessary to have that much clearance in order to fit the one waste package and the emplaced waste package. So it's a very strong joint when things are lowered down and engaged. You've got a lot of overlap. You've got these sidewall connector pieces that are holding things in place.

As far as the margin here, we've got about four--a little over 4 1/2 inches where these can slide relative to each other, and facilitate the placement of those drip shields.

I might point out that the current requirement of the waste package placement is 10 centimeters, or almost 4 inches. So, we actually have a little more here than we would have for the waste packages.

Now, this shows all of the different emplacement devices that are associated with the drip shields. You see the transport locomotive, which is no different than the one that's used for putting the waste packages, bringing the waste packages down into the mountain, the drip shield transporter, or drip shield on its transporter, drip shield emplacement gantry. Here, you see some emplaced drip shields, and a waste package there.

And, what's important here is this hand-off location here where we transfer things from this drip shield transporter to the drip shield emplacement gantry.

Drip shield emplacement gantry. It's very similar to the waste package emplacement gantry. It uses many of the same structures and features. It's rail based. You can see down here it runs on the existing rails in the drift. It's self propelled, probably from a third rail electrical system in the underground. It does have a backup battery system that will permit the drip shield to be lowered, and then the emplacement gantry will return to that interface point so that it can be returned to the surface for servicing if that becomes necessary. It's remotely operated. You can see here these two radio communication. The actual operation would be, in some sense, manual, but would be done remotely by operators in the surface facility.

These little features here are high resolution cameras and associated high intensity lights. When the gantry is loaded with the drip shield and it goes down the drift, the wheel turns are indexed so that we can at least in a coarse fashion get the drip shield and gantry very close to the emplacement of that adjacent drip shield that we want to connect against. And, then, the details are all done by an operator with the cameras and the high intensity lights to do the final positioning of the drip shield over the already emplaced adjacent drip shield.

You can see those lifting features, which engage with the drip shield lifting features that I talked about earlier. You can see the whole interface.

The drip shield gantry transporter, of course, we've got to have a way to get the gantry down into the drifts, and also transfer it from drift to drift for emplacement purposes. It's based on a commercial type rail car. You see there's some location features to get it all lined up so that it's in the right position for its transport.

The drip shield transporter, again, based on a commercial railroad flat bed car. You can see there's a couple location features on there to get everything lined up for the surface facility, so there's predictability of when it reaches the underground remote systems there.

The transport locomotive, you probably heard about this before with regard to the waste package emplacement, and, so, I won't dwell on that.

Go on to the next slide, please. There's a couple slides here that talk about the steps which have to occur in order to emplace a drip shield, and I'd like to go ahead and skip over these two slides and go directly to an animation that we have here. Skip ahead a couple to the animation.

So, here, we can see that the first thing is to transport the drip shield to the transfer dock. This is done by the locomotive. Then, the gantry moves on and straddles the drip shield, picks it up by the lifting features. Then, it moves it down the emplacement drift, positions the drip shield properly, lowers it into position, and then goes on, in this case, to be taken to the next drift. But, otherwise, to catch another drip shield to be emplaced.

Finally, there were a couple, or at least one question that the Board posed to us with regard to the drip shield and the emplacement devices about what's going to be available in the 2006 to 2008 time frame.

For the drip shields, we do have configuration and assembly drawings and calculations to support the demonstration of the safety functions. And, those are all, have been done for some time. Under the current schedule, support for prototype procurement will begin in 2008. For the drip shield gantry, there is a conceptual design, but, again, under the current schedule, there's no plans before 2008 to advance the design or produce a prototype.

Thank you.

DUQUETTE: The next presentation is by Jerry Gordon, Framatome. He has over 30 years of experience as a manager or principal scientist associated with various research and development efforts in materials science and corrosion and related engineering, both at GE and the Yucca Mountain Project. Currently, he's a Senior Staff Scientist on the Waste Package Modeling and Testing Team at the Yucca Mountain Project Site. He's going to talk about the drip shield creep and environmental degradation.

GORDON: Thank you.

What I intend to cover in the next 20 minutes, or so, is to describe the selection basis for the drip shield materials, and to review some of the issues the Board asked in their questions, the first being environmentally induced cracking susceptibility of the drip shield titanium materials, the effect of creep on drip shield performance, and the various corrosion degradation modes relative to the drip shield, general, localized and galvanic corrosion.

There was a methodical process involved in the selection of the drip shield. There were a series of materials selection meetings, peer reviews, and so forth, and I've referenced some of them here. They resulted in 1999 in a selection of Alloy 22 for the waste package corrosion resistant material, and either Titanium Grade 7 or, as a lower cost potential alternate, Grade 16.

These are very corrosion resistant materials, but one of these principal objectives was to provide defense-in-depth by selecting corrosion resistant materials from two different alloy systems, the titanium palladium alloys, and the nickel chrome alloy system.

The Grade 24 was selected somewhat later, a couple years later, for the structural support. I'll talk a little more about these materials. And, I think it's important to recognize that titanium alloys were considered, and in several cases, are still under consideration for waste package applications in various international programs, including the Canadian, Japanese and the German programs. And, I'll describe some of the data that were generated that are relevant from those programs, as well as the project data.

The shell, plate and welds on the drip shield, and Mike Anderson sort of gave you an overview of that, are titanium Grade 7. It is a variant of commercial titanium Grade 2 that contains a very small palladium addition on the order of point .17 percent, which provides a great enhancement in corrosion resistance, as we'll see.

The highest strength structural materials, the design basis alloy is titanium Grade 24, which is a variant of the aerospace alloy, titanium 6 aluminum, titanium Grade

5, that also contains small palladium additions.

We're also looking at lower cost potential alternates for the structural material, particularly titanium Grade 29, which is a Ruthenium analog of titanium Grade 5, rather than palladium. It's an extra low interstitial version, has lower iron, nickel and oxygen, which gives it enhanced toughness.

Let's talk about stress corrosion crack and hydrogen induced cracking. Let me start off by--these are the two principal modes of environmental induces cracking that titanium alloys are potentially susceptible to. In the German repository program, they looked at, and are still looking at titanium. They've done a lot of stress corrosion tests, brine and slow strain rate tests, in which the specimen is slowly strained in an autoclave brine environment until it fails. And, the ductility parameters and time to failure are measures of the degree of environmental effects on the failure motor, the extent of environmental cracking.

Their tests included high temperature, 170 Centigrade, chloride brine, and they included welds and base metal, and based on their total suite of tests, they concluded that Grade 7 was not sensitive to stress corrosion cracking in these brine environments.

NRC funded work at Southwest Research Institute. They ran a series of slow strain rate tests. These were not temple samples, which makes them potentially more susceptible to hydrogen induced cracking. They ran in a deaerated, and that's sort of important, sodium chloride brine, and they ran with and without 10th molar sodium fluoride. And fluoride has the potential to be a bad actor under some cases with titanium.

They found no stress corrosion or hydrogen induced cracking in these notched specimens in the sodium chloride brine without the fluoride. With the 10th molar of fluoride, they did see a decrease in ductility, some evidence of brittle fracture on the fracture surfaces, and they concluded that it likely was due to hydrogen induced cracking. And, the titanium Grade 5, the highest strength material, was more susceptible, as one might expect, than the Grade 5.

It's important to contrast that with the aerated repository conditions, and the extended period of dry oxidation following emplacement and closure, and the temperature goes way up, 150 to 200 Centigrade, the humidity is low, and basically, the titanium alloys get pretty strong, with a fairly protective film. So, in the literature, that indicates the protectiveness of that type of film.

These are some project data, slow strain rate. These are cathodically polarized to very low negative potentials, where one can force hydrogen into the surface, and accelerate the propensity for hydrogen induced cracking in both titanium Grade 7 and an early potential alternate that's been dropped from consideration, that is a higher strength nickel molybdenum containing alloy.

I should point out titanium Grade 7 is an alpha titanium. That is a hexagonal close path alloy. The Grade 12 is an alpha beta alloy. It contains some body center cubic phase. These tests were done in a chloride brine, to a pH 2.7. And, if you look at the parameters, time to failure and reduction in the area, the function of negative potential below the corrosion potential, the tests were run as low as 1500 millivolts below the corrosion potential, very aggressive in terms of hydrogen induced cracking potential. The Grade 7 basically was unchanged over the range of applied potential. The Grade 12 did start to suffer hydrogen induced cracking at the more negative potentials.

There have been, and still undertook a large number of U-bend samples at the Lawrence Livermore Long-Term Corrosion Test Facility. This is an example of an exposed to five years, and then removed, and examined at high magnification. These U-bends include Grade 7, Grade 16 and Grade 12, and they've been exposed and removed at various periods from a half year, up to five and a half years.

The brines cover a range of pH from 2 to 10. They're primarily concentrated versions of J-13, on the order of generally a thousand times concentrated, and they contain from zero to .1 mill of fluoride. And, in no case, did the stress corrosion observed in these U-bends out to the five year exposure period for the Grade 7 or Grade 16.

In contrast, there were a few of the Grade 12 welded samples that showed stress corrosion cracking. And, that was in the 10th molar so-called simulated concentrated water, SCW.

These are also project funded data GE Global Research. These are stress corrosion initiation tests that involve constant load temple samples, loaded in an autoclave with the brine. The autoclave pressure on one side of a atmospheric--applies a load and maintains it on the sample. And, in the first test campaign, and this is in a concentrated basic saturated water, pH about 12 at room temperature, about 10 at 125 to 105 Centigrade. So, it's a basic concentrated J-13 type brine.

They ran both annealed and cold worked, 20 percent cold worked. The annealed samples failed in very short times. These are pretty high stresses. The yield is about 36 ksi. These are thousands of pounds per square inch. So, this is significantly above yield. Surprisingly, the cold worked samples, which normally are more susceptible, didn't fail, and these tests alter about 1900 hours.

In the second campaign, GE covered a range of stresses from 40 to 50 ksi, and they got a time dependency on the failure. But, the failures were still, at least we expected them to run out to long times, and they failed in from a few hours to several hundred hours.

Because of the short failure times, it seemed prudent to run some air control tests at the same range of stresses. These are two examples. And, we found the material did creep at, in this case, 105 degrees Centigrade, and it was classic type of creep, primary, steady state, and ultimately transitions to tertiary creep, where the rate increases with time. This is displacement or strain versus time. Consequently, we're getting failure times similar in air that we saw in the bring.

I plotted here a creep rupture curve for titanium Grade 7 and Grade 2, which have the same mechanical properties. The Grade 7 data from GE and the Grade 2 data from Atomic Energy of Canada. And, you can see at the higher stresses, this creep curve basically intersects the diamonds, which are the brine, failures. Pretty good evidence that we're seeing creep rupture rather than stress corrosion, at least at these high stresses. And, we have now started testing samples at lower stresses.

I've also plotted on here our current stress corrosion initiation stress criterion that's used in our model. It's 50 percent of the air temperature yield, and you can see, although these aren't stress corrosion failures, they're creep rupture, we didn't see stress corrosion, in these stresses and these times, so we do have significant margin in our initiation criterion.

I plotted here the stresses remaining on the U-bend samples that were removed from six months up to five years from the various brine environments at Lawrence Livermore. These stresses were measured by x-ray diffraction on the samples after they were taken out, and they represent the range of maximum stress as they vary depending on where on the sample you took the measurements. So, this is an envelope of maximum stresses as a function of time, and they do drop off with creep relaxation.

Stress corrosion is normally divided into initiation and crack growth. And, crack growth is a consideration, for example, with weld plots that are surface breaking or other defects, where one might have a stress intensity factor at the plot tip. And, under those conditions, it's possible, if you have a susceptible material, to initiate stress corrosion, and then to propagate it.

These are some data also obtained at GE Global Research. These are compact tension fracture mechanic samples. They were fatigue pre-cracked before putting them into the autoclave environment. They're put under tensile load initially, they're cycled, and after some time, they're held at constant load, or stress intensity factor. And, the crack extension can be monitored with reversing DC potential drop. It's a very sensitive technique to measure online crack growth.

These are titanium Grade 7 material. They're saturated, saturated water, very concentrated, basic brine of 110 Centigrade. And, after about 12,000 hours, the load was transitioned to constant load, and the sample continued to grow at a very stable rate of about 1.25 times 10^{-8} millimeters per second, a very low rate, but nonetheless, a sustained growth rate.

In light of the creep rupture failures of the constant load samples, we decided to run a companion titanium Grade 7 compact tension sample in air. That's the second set, and we did get sustained load, crack load, and after about 800 hours, the rate dropped to somewhat below the rate in the environment, but very close to the rate in the environment. So, it's very likely that we're getting a very significant, if not complete, creep component to the crack growth.

These are some more recent data on titanium Grade 29, which is the analog to Grade 24 that contains Ruthenium. It's a higher strength material, as I mentioned. These tests were done in the simulated concentrated brine.

I should point out that both this brine and the basic saturated water brine do contain about a 10th molar of fluoride.

Again, the sample was cycled, and then it was held at constant, low stress intensity factor, and we observed a fairly stable crack growth rate, only about a factor of 40 higher than the Grade 7 material. And, we know that this material is orders of magnitude more creep resistant than the Grade 7. We're seeing a higher growth rate, so very likely, we're seeing stress corrosion crack growth in this higher strength material. We do plan to run an air control test similar to what we did on the Grade 7.

Since we're seeing creep at repository kind of temperatures, which is unusual, considering the melting form of titanium, usually creep doesn't occur at that very low temperatures compared to the melting point. So, we had a task to do calculations on the effect of creep on time dependent deformation of the drip shield.

As in place, the drip shield is in a fully stressed relief annealed condition, so they're very low residual stresses. And, there are two significant loading scenarios in which stresses on the order of 50 percent of yield or higher can be generated. One is the rock rubble loading in the lithophysal zone. This can happen during long-term drift degradation or seismic degradation. It's basically a very coarse gravel pile on top of and along the sides of the drip shield that applies a sustained load over time, and it could lead to drip shield creep rupture or collapse potentially.

The second scenario is seismic induced rockfall in the non-lithophysal zones where you have the faults in the rock that under seismic loads can result in rocks falling and impacting onto the top surfaces of the drip shield. And, that can lead to denting and potentially to lock in residual stresses that can be high enough to initiate stress corrosion. They're not sustained stresses. They tend to relax with time.

Both of these scenarios were analyzed. Reference to calculation here, it's fairly detailed.

For the rock level loading, the maximum calculated creep strain, and this is after 10,000 years after emplacement, is less than 5 percent. We set a conservative acceptance limit of 10 percent. That's the lowest strain at which one sees the transition from the steady state secondary stress to the tertiary time increasing deprivation that could lead to creep rupture. So, we're a factor of two below that limit, looking at different realizations or load applications along the perimeter of the drip shield. And, Mark Board in the next presentation will go into more detail on these types of loading.

For the rockfall impact case, which he will also describe in much more detail, I'm just describing the creep component of that, the rock can impact on the corner or the edge. It's likely to crumble or fall to the side, but it will leave behind a dented area that can have locked in residual stresses. The creep/stress relaxation calculation, however, indicates very similar to what we saw with the U-bends that were also fixed deflection residual stresses. They relax fairly quickly on a repository time scale to on the order of 50 percent of yield at room temperature. And, at higher temperatures, in ten years, down to maybe 65 percent of yield. They continue to relax over time.

Switching from creep, which is a form of degradation, to general, localized and galvanic corrosion. With respect to general corrosion, titanium alloys are known to be very resistant. Project data, these are data generated again at Lawrence Livermore in the Long-Term Corrosion Test Facility, in a range of brine covering acidic to basic pH, out to about five years. These are maximum measured rates plotted here for both the Grade 16, which went in earlier, and then the Grade 7 material. And, the rate does drop off with time, as one might expect. These are crevice samples at the upper line, and more exposed samples at lower rates. These are the 100 percent rates.

Actually, the median rate at two and a half years for titanium Grade 7 is less than 10 nanometers per year. It's extremely low. But, Grade 24 we're only now starting to collect data, but there are data in the literature that indicated somewhat less corrosion resistance than the Grade 7, which you might expect, since it's alloyed with 6 aluminum--which effectively dilutes the titanium. But, although it's a factor of 4 to 5 higher than Grades 24 and 29, to Grade 7, compared to non-palladium containing alloys, if you will, of these materials, they're orders of magnitude lower corrosion rates, and still extremely low, even a factor of 4 to 5 is very low.

Next slide. The project data are pretty well confirmed by--these are data from European Repository Program, the German Program, evolved into a European Repository Program a few years ago, and they're out to about four years of exposure. They actually ran in a range of brines covering a range of pH's. These particular data are their longest term data, in so-called Q-Brine, which is a sodium, potassium chloride, magnesium chloride, a very aggressive brine, pH of 4.9. And, they also see the rates drop with time, and at four years, they're down around .07 microns per year, still a very low rate. It's very comparable to the rates that we measure on the project, even though this is a more aggressive brine. And, very important, they find that the rates are temperature independent up to about 200 C. at least.

These are some crevice data. We're now switching to localized corrosion. These are critical crevice temperature data from peer reviewed literature. The red bars here contain, among other alloy, titanium Grade 7 and Grade 16, in a range of primarily acidified pretty aggressive aerated, deaerated, and even ferric chloride brine.

And, on the right, the cross-hatch bars contain data from Grades 24 and 29. They cover a range of brines, also some of them fairly aggressive. And, in all cases, the critical crevice temperature is over 200 degrees Centigrade. So, we build a very resistant material to localized corrosion.

These are some cyclic polarization data. The upper two curves are project generated data on uncreviced titanium Grade 7, and the lower curve is Southwest Research Institute generated data on creviced titanium Grade 7.

These are cyclic polarization, in this case, in a deliquescent type, very high nitrate brine at 120 Centigrade. And, in the forward scan, at about 800 millivolts, we start to see oxygen evolution. But, it goes up to 2 1/2 millivolts, and then is a reverse scan, and there's no evidence of localized corrosion, such as pitting on this specimen.

Similarly, on a 150 C. chloride brine, with and without a small nitrate addition, we see very high margins between the repassivation potential, where the forward and reverse scans intersect, and the repassivation potential is about 500 millivolts higher, with a small nitrate addition than without it.

These data in 1 molar sodium chloride are interesting because they go up as high as 165 Centigrade. They include welded and rock material, and they're tightly creviced, and these are the repassivation potentials, again, measured by cyclic polarization.

They drop from about 4 volts at 90 C., down to about 1.3 volts at 165 Centigrade. On the right, is a corrosion potential measurement in aerated on titanium Grade 7, and an aerated 1 molar sodium chloride, and the potential levels out after several hundred hours at about minus 170 millivolts, which is well below the repassivation potentials, indicating a very large margin for localized corrosion.

The Board asked about galvanic corrosion of titanium Grade 7. There is that potential with Alloy 22 in the base of the drip shield, where the Alloy 22 base contacts the titanium. There is also that potential between the titanium Grade 7 shell and the carbon steel in the invert, assuming one has a continuous film running along the side of the drip shield contacting from the Grade 7 over the Alloy 22 to the carbon steel. In that case, you could galvanically protectively couple the carbon steel to titanium.

These are project sponsored data generated at

Atomic Energy of Canada, White Shell Laboratories. We ran a series of galvanic couples of dissimilar metals, and these are examples for Alloy 22 against titanium Grade 7. This is a non-deaerated simulated concentrated water, concentrated brine, with some 10th molar chloride. These are the corrosion potentials as a function of time. We ran two types of crevice tightness. One was very tight. The other was a tapered, open at one end crevice, and the potential was a little bit higher on the open crevice, as one might expect, than the tight crevice, where we can get a higher driving force, because of the oxygen depletion within the crevice.

Most important, there was no evidence of galvanic or crevice corrosion on these samples when they were pulled apart, cleaned and examined at high magnification.

The European Program ran very tight contact couples. These are bolted, insulated bolts, titanium 7 against carbon steel, exposed at 150 C. to a concentrated chloride brine, pH 6 1/2. The triangles on the X axis may be hard to see, basically very, very low weight gain compared to the carbon steel, as one would expect. After a very long exposure time, the rate was still extremely low, and was basically the same rate as the uncoupled titanium, indicating essentially no effect of the carbon steel on the corrosion of the titanium.

Similarly, the carbon steel corroded at about the same rate as without the titanium couple. They did metallography and they saw no evidence of corrosion attack.

So, to conclude, I've covered a lot of territory fairly quickly, of necessity. The drip shield alloys are highly resistant to stress corrosion and hydrogen induced cracking under repository relevant conditions. They can be made to crack under non-relevant aggressive conditions.

In terms of the general corrosion rates, they are extremely low and temperature independent to at least 200 Centigrade. As we saw, there are wide margins for localized corrosion under repository type conditions, fairly aggressive in temperatures and the environment.

We didn't see accelerated galvanic corrosion for couples between Alloy 22 and titanium or carbon steel and titanium. And, finally, the creep deformation can occur under rock rubble loading conditions. Calculation shows the resulting strains are acceptable.

Thank you.

DUQUETTE: Thank you, Jerry.

The third part of this presentation will be presented by Mark Board. He's a geological and geotechnical engineer with ITASCA Consulting Group, with 25 years of experience as a practicing engineer. He was manager of the Seismic Studies Group and Project Engineer of Subsurface Design at the Yucca Mountain Project from 2001 to 2005, and he's currently a consultant to Bechtel.

One last comment. At the end of your presentation, Mark, I'd like to ask both of the other presenters to come to the podium to take questions.

Thank you.

BOARD: Today, I'm going to talk about the mechanical degradation studies we've done for the drip shield. In particular, these are the objectives of the talk.

First of all, I'd like to describe the various sources of mechanical degradation of the drip shield. In doing so, I'm going to briefly review the emplacement drift degradation under both vibratory motion and long-term rock mass strength degradation. The reason I'm going to talk about this is because rockfall is the primary loading mechanism for, mechanical loading mechanism, for the drip shield.

I'm going to review the drip shield structural response calculation to quasi-static loading from the rubble generated by drift's collapse. I'm also going to review the drip shield structural response to impact loading that Jerry alluded to earlier when he talked about the creep calculations and the denting. And, then, I'm going to talk about drip shield structural response to vibratory motion.

This flow chart shows the basic sources of mechanical degradation of the drip shield. Basically, the map is subjected to three different types of loading during the history of the post-closure, thermomechanical stresses, seismic loading, and the general rock, in situ rock mass stresses that exist.

These then drive the potential for drift degradation in the rock mass, which in turn results--can result in rockfall and the two different rock types that are typical to the repository horizon, which are the lithophysal and non-lithophysal units, which I'll explain in a second. These then can result in these three different mechanisms I mentioned in the objectives, quasi-static loading. The reason for the term quasi-static is that over time, as the rock mass tends to fail around the tunnel, you get this slow build-up level over time. So, it's not exactly a static situation, although you could think of it that way. The other, impact loading and potential denting in the non-lithophysal rocks, which can produce large blocks, and then, finally, vibratory motion and potential for separation of the drip shield.

As Jerry talked about earlier, these two aspects, the quasi-static loading and the impact denting, those residual stresses in the drip shield are then fed off to creep calculations that Jerry discussed earlier.

The only area that is also separate that I'm not really going to talk about today in any detail, but just to mention, is that seismic events can give rise to fault displacement, and fault displacement is taken into account in the TSPA model in the same manner that seismic loading is, and damage to drip shields can occur from fault displacement, as well as mechanical loading.

Just to briefly mention, I know you've heard it before, and that's that the Topopah Spring, the repository host--is generally divided into two rock types. One is termed the non-lithophysal rock, and the other, lithophysal rock. The matrix material of both of these rock types is virtually the same. It's hard, brittle, high quartz content material. What differentiates these two rocks is the geologic structure that occurs within them.

In the non-lithophysal rock, the structure is primary cooling related fractures that can have fracture links on the order of a few meters, and scapings that can be on the order of a meter or less. The type of failure that one expects in this type of rock is the production of wedges of material, or irregular shaped blocks that can be removed or dislodged from the tunnel surface.

In the lithophysal rock, it's characterized by a percentage of voids in the rock that are cooling related gas pockets essentially is what they are, and on top of that are a series of short, rough, ubiquitous fractures, or like a fracture fabric of discontinuous short link fractures. The porosity in this rock can be as high as about 30 percent in lithophysal voids that averages about 18 percent, or so, and these lithophysal voids can be--they average about 10 centimeters in dimension, but they can be as large as a meter, is what we've seen, and they can also be highly irregular in shape.

This photograph is a little bit difficult to see, because the lighting isn't very good, but this is a lithophysal void here.

The main difference between these two rock types is this rock, the rock mass is structurally weaker in this case, because of the presence of these voids within the rock. Also, this ubiquitous fracture fabric that exists, when a tail rock tends to create small blocks of material, the block size, we feel, and this is not what I've showed here outlined in yellow, it's about 10 centimeters, or so, the irregular joint structures here tend to produce larger blocks, as I'll show in a minute.

We analyzed drift degradation under dynamic loading, and also long-term strength degradation modes. In the dynamic analyses that we did, we conducted sensitivity studies with ground motion time histories that were generated from the seismic side of the house, based on the probabilistic seismic hazard assessment work that was done.

We used 15 different potential ground motion time histories that represented each annual exceedent frequency level. Instead of terming it in terms of annual exceedent frequency, like maybe you've seen in the past, 10^{-4}, ^{5}, ^{6}, and ^{7}, it's more accurate, we feel, to represent that in terms of peak ground velocity level. So, the .384, 1.05, 2.44, and 5.35 meters per second represent the peak ground velocity levels for these different 15 time histories that we examined at each one of these peak ground velocity levels.

The sensitivity studies would get included also, examining a range of rock properties and in situ fracture geometries, and for the non-lithophysal rock, which I told you is, we feel, in the failure mechanism is based on pre-existing fracture planes in the rock, we conducted three dimensional analyses that had stochastic variation of joint structure in them that was derived from field mapping studies that we did. And, here, we used up to about 50 different stochastically defined fracture patterns in combination with these different time histories and rock properties to conduct sensitivity studies.

The idea of this was we were trying to produce stochastic distributions of rock volume and rock sizes and dimensions that would be representative of the broad spectrum of what we might see in the repository in non-lithophysal material.

I forgot to mention on the previous slide this non-lithophysal rock mass represents about 15 percent of the total emplacement drift area. The lithophysal material, which is the material with pores in it that we feel will break, it was just weaker and will break into much smaller particle sizes, represents about 85 percent of the emplacement area. So, this is the typical condition that one would see.

I mentioned here that although we examined response at very high peak ground velocity levels, up in excess of 5 meters per second, I believe John H. gave you a talk some-- within the last, I'm not sure exactly, within the last year, I believe, that shows that the project has come to the conclusion that we can represent a bounded peak ground velocity level of around 4 meters per second, and we feel that that's quite conservative in itself, and that's based on the fact that we don't see any geologic observation of failure in situ of the rock mass. And, this type of peak ground velocity level in the last 13 million years would have resulted in observable failures underground of continuous fracturing between the lithophysae that we feel would be very easy to recognize, and we don't see anything of the sort.

And, so, we've done what we think is a conservative bounding at 4 meters per second. So, although I'm going to be talking about calculations at this level, we feel that we are somewhat under that level.

From these different methodologies, we use a two dimensional approach, which I'll show in a second, in the lithophysal rock to calculate rock fault. We developed distribution to the volume, shape, mass, velocity, and then from mass and velocity, we calculate the kinetic energy, and the locations of impact of these blocks on the drip shield.

The total drift collapse, we simply said we don't exactly know how long this drift collapse is going to require to occur, but what we're going to represent by simply reducing the strength of the lithophysal rock mass and forcing it to completely collapse around the drip shield and use that as a means to calculate the ultimate loads to the drip shield.

This shows some of the bulk of the non-lithophysal rock calculation under seismic loading, and these are histograms here show that the block masses that we calculated from the many analyses that we did, show a roughly negative exponential distribution of rock mass occurring, and that's what we would have expected from the fracture patterns that we have.

The medium block size is the same for all ground motion levels. It's about .15 metric ton, which the density of this rock is about a foot to a foot and a half on a side. So, these are the larger block sizes that we have. The maximum block size, block energy that we had was a 28 metric ton block, it's the largest block that we saw dislodge during these analyses. But, as you can see, the distribution increases the number of blocks dislodged as a function of peak ground velocity, and also somewhat larger blocks can be dislodged, but certainly the 28 metric ton, which is the largest, is quite an outlier on this plot.

For the lithophysal rock, which I mentioned, is significantly weaker because of the presence of these large pores and porosity. We also did seismic calculations to predict when collapse or partial collapse would occur.

What we found is that for these rocks, rock types, we did a sensitivity study where we varied the porosity level, strengths, things like that. We came to the conclusion that for--actually, for peak ground velocity levels greater than about a meter per second, significant collapse begins to occur in the tunnels, and by the time you hit a peak ground velocity around 2 meters per second, we expect that the drift would be largely collapsed.

And, on the right-hand side, I show a series of pictures from the ground motion that causes the greatest amount of damage and greatest amount of disruption from the 2.44 meter per second ground velocity level. And, this is typical of the runs that we would see.

The rock mass failure response, response to stress in the rock, is proportional to the peak ground velocity, and that means that at the arrival of the strong ground motion, which occurs within a second or two of the arrival of the earthquake wave, we did collapse occurring in the tunnel. And, as you can see, these are seconds after the initial state after the ground motion begins to arrive.

And, the important thing from this that I want to point out to you is that in a lithophysal material and in the non-lithophysal materials as well, we get partial collapse at ground motion levels that are below 2 meters per second very early on after the arrival of the ground motion. And, in fact, we get pinning of the drip shield very early on from the rock mass that settles around it prior to the point, which I'll point out later, we get significant motion of the drip shield structure itself.

So, when analyzing the vibratory motion of the drip shield, the actual true case, the most realistic case that we should be analyzing, which is what we did, is to look at a drip shield that's actually surrounded, or partially surrounded by broken material.

The quasi-static loading on the drip shield. We examined six different cases where we forced collapse of the drip shield using this discontinuum model, which was similar to one that I just showed you on the previous page.

As you can see, we did this with a discontinuum model because it's very important to predict non-uniform loading to the drip shield from broken rock rubble.

What we did is we subdivided the drip shield into 30 segments around the exterior, and it's the same 30 segments you see along this axis here. And, for right now, the absolute level of the pressure--and what I wanted to show you is that the pressure applied around the drip shield can be highly non-uniform in nature. You can get high loads at one point, and then low loads directly adjacent to it. And, the reason for this is because you're having point contact of blocks up against the drip shield, and it's important, I think, to account for this non-uniformity in loading rather than making some empirical assumptions about what the weight of that rock would be around the outside.

We used this loading from exactly as you see it in the LS-DYNA structural calculation, and applied it to the drip shield to determine its stability under this quasi-static load. And, what we found out was that it's stable under all these load combinations that we calculated. And, what we did to try and get some estimate of what type of safety margin--I don't want to use the term a factor of safety here, because it doesn't really apply to this non-uniform loading situation. But, what we did is we took each one of these cases, and we increased the density of the level around the outside to preserve the non-uniformity of the loads on the outside, and continued to increase the density and the weight of the material until we got enough deformation where it actually deflected the point where it touched the waste package.

And, what we found out is that this safety margin, if you want to term it that way, is approximately three times over what the worst cast loading is that we have that we think from the quasi-static drift collapse.

The response to seismic effects. Again, these are the impacts of large rock blocks, potential drip shield separation, and the third one, which I'm not going to talk about anymore than this, is to simply say that for seismic events of annual frequencies lower than 2 times 10^{-7} per year, and this was derived from the Probabilistic Seismic Hazard Assessment, we can produce fault displacements that are large enough to actually contact the drip shield. In that case, the drip shield is assumed to lose its functionality, and that in fact in the TSPA model, close to 60 drip shields are actually lost due to this fault movement.

I just wanted to point out that there is another mechanism for drip shield loss of functionality that you might not be aware of.

Jerry talked about this briefly, mechanical impact from rockfall. We feel that the only areas that will produce large enough blocks to actually dent the drip shield is in the non-lithophysal material, which as I pointed out, has a median block size of about .15 metric tons that can produce, in the highly unusual case, as you saw from the histogram, blocks that are large enough to be in excess of 10 metric tons.

What we did is for the 2.44 and 5.35 meter per second PGV levels, we took the 50th to 95th percentile of the largest block energies, and we applied these blocks, we essentially accelerated them into the drip shield at the top corner and the sides to examine the denting that you get from that.

This shows the largest rock block that we had, which was 28 metric tons, which we impacted on the crown of the drip shield in this particular case. The red area that you see around here are those areas that exceed--in which the residual tensile stress exceeds 50 percent of the yield strength of titanium Grade 7, which was what the criteria was being used for surface area damage, and we found out that you can get as much as about 15 percent damage to the surface area with this 28 metric ton block, but it's generally less than a percent for the large majority of the blocks that we had. Under any of the rock blocks that we had, even the largest, the structure itself remained stable when it's impacted.

And, I wanted to point out that, as Mike Anderson did, that this drip shield is a rather robust structure. The surface plates are almost an inch thick, and it has very substantial structural members beneath. So, these dents that can result are really not all that large, we found out. For example, for the 28 metric ton block, you can get up to about a tenth of a meter total deflection of the roof, which is still less than what's required to contact the waste package.

But, when you look at the area that can actually pool water, you find out that by connecting essentially a surface contour, what can actually pool water, that depth is around 5 millimeters, or less than two ounces of water can be pooled.

So, what we found out is even with these very large dents, that a significant volume of water does not pool in them, and as you know, the static head is required to drive that through. So, we're talking about not a very large significant volume of water from denting.

Jerry also mentioned that creep calculations were then done. We took the stresses, the residual locked in stresses that we felt were from these dents, and then fed them off to the creep calculation that Jerry mentioned earlier.

Drip shield separation is the final area that I wanted to touch on. Mike Anderson went over the way that drip shields are interlocked with one another, and they can be separated by two basic modes. One is if you displace in a shear displacement fashion one drip shield relative to another by about 40 inches, or close to a meter. The interlock locking bars and the mechanisms prevent complete separation of the drip shield unless you lift it up past those bars.

Axial force can be generated by differential normal movement of the drip shields, which can apply stresses to the welds and the interlocking bar members as well, and those can be broken and cause separation.

I wanted to mention something about the incoming wave motion that's applied to these drip shields that I think is very important to recognize. We've got a chain, essentially a chain of these drip shields that are interlocked end to end that's up to about 600 meters long in an emplacement drift. The incoming seismic waves are almost vertically incident. They have at most 10 to 15 degrees incident angle. So, they're vertically travelling waves, and their dominant frequencies of the wave form of the incoming seismic waves have very long wave lengths, in excess of a kilometer.

So, it's important to recognize that we've got drip shields that are essentially 5 meters in length, and will have a wave that has a wave length of in excess of a kilometer that's vertically incident to the tunnel. So, essentially, I'm taking this entire tunnel and lifting it up and down like this, as well as taking it, of course, horizontally and vertically. But, there is not a significant difference in motion from one drip shield to the next, and they behave more or less in a synchronous fashion, unless certain things tend to break up that synchronicity, which I'll point out in a second.

So, if conditions are uniform along the drift, and by uniform conditions, I mean frictional resistance along the base of the invert of the drip shield and between drip shields themselves, these drip shields will move synchronously.

How can you get drip shield separation? Well, you can potentially get it in an open tunnel under these uniform conditions that we have if the drip shields can rotate and contact the drip--there's enough potential separation where you could actually get them to separate from one another. And, it is possible to get this type of rotational motion, because the seismic wave passing up has a vertical and a horizontal component, and since the motion is applied at the invert, you can develop a moment in the drip shield so it actually rotate. So, it is possible to get a rotation that could cause up to about 2 meters of potential differential movement. So, it is possible.

The other way it is possible is that if these drip shields are moving normal to one another, axially along the drift, it is possible that you could rip the connector bar welds that connect them, if you develop enough force in that way.

As Mike pointed out, there's about four inches, I believe, of potential movement in there in that interlocking mechanism. So, that's one other way.

To investigate this, we did two different types of analyses. The first thing that we started a number of years ago was a full three dimensional analysis of several drip shields that included all the exact geometry of the connector bars and everything that would interlock. It's a very computationally difficult problem, as you can imagine, to look at the full detailed geometry of this, and the seismic waves that have a strong motion that may be in excess of 20 seconds long. These simulations are computationally intensive.

So, these three analyses that were done were for three drip shields interlocked, and rigid end boundaries were put in place to represent completely pinned end conditions.

What we found out from that was that drip shield separation didn't occur until the 5.35 meter per second level of ground motion, and you may have read that in one of those studies that were originally done. What we found out is what actually caused the drip shield separation in that case was that at that 5.35 meter per second level, we actually got enough displacement where it struck the end in the model, struck the end rigid plate, and then caused chaotic motion of the drip shields internal, and they rode up on top of one another.

Well, so, the boundary conditions were unrealistic from that analysis. But, what it did show is that you have to have ground motion in excess of the 2.44 meter per second level to get drip shield separation, even in an empty, open drift.

Now, to investigate--we think it's very important in this case that we have many drip shields interlocked with one another--and, so, to investigate that, we did some much simpler two dimensional kinematic type models, in which we analyzed all 15 different ground motions for a wide range of peak ground velocity levels, with very frictional conditions, metal to metal, metal to rock, all drip shields in a line, and we analyzed it for partial and complete drip collapse. We applied all these ground motions to these long chains of drip shields, in which we had a normal and a shear contact between drip shields, and we examined under what conditions would we actually produce separation.

What we found out in actual fact that we could not produce drip shield separation from these ground motion levels. With the ground motions that we have, the drip shields tend to move synchronously with one another, and it's difficult to get them to separate.

A very important thing I think that I want to leave you with is that why don't we feel that separation is possible? And, that is that the normal case is that for these high ground motions, is that the emplacement drifts will be either partially or completely collapsed. The collapse occurs in all cases before we generate enough motion to actually separate the drip shields in an empty tunnel. So, what happens, as soon as you surround the drip shield, even partially, with rubble, if the frictional restraint on the sides of the drip shield cause a high degree of damping of motion, and if you have complete collapse, the mass of the weight sitting on top makes these go in complete synchronous motion with the tunnel itself.

So, the conclusion that we came away from this was is it's very difficult to get drip shield separation under the conditions that we would expect for these large ground motions. For the ground motions that don't cause collapse of the tunnel, they're too low to cause separation of the drip shields in any case.

The conclusions we have then is that the rock mass loading to the drip shield we feel uses realistic non-uniform load distributions. We went to great lengths to try and attempt to calculate what block masses in sizes and blocks would be dislodged under earthquake loading, and we applied those non-uniform loading conditions to the drip shield to calculate structural stability.

Impact loading from the largest and most highly energetic blocks were examined, and the drip shields are structurally stable under those loads, and we estimated the dent depths that were determined for use by others within the program.

Drip shield separation was examined for a full range of seismic shaking, and rubble from the drift degradation prevents drip shield separation through frictional restraint, or just simply from the mass applied to the interlocked drip shield chain.

And, then, finally, a small number of drip shield failures from fault displacement resulting from seismic events is included in the TSPA.

DUQUETTE: Thank you, Mark. Could I ask the presenters to step up to the podium, please? And, we are running about 20 minutes late, and, so, I don't want to take the reputation away from one of my colleagues on the Board, who always runs his meetings late, so I'm going to try to hold questions to those that are very critical.

And, this is one of those presentations where I think we could go all afternoon with questions, because we got a whole bunch of pieces of data, and let me ask if the Board has any questions first. And, I'm sure Dr. Latanision does.

LATANISION: Well, I first want to say that this was a very comprehensive response to questions that the Board has brought up over the past several months. And, I want to applaud that. We learned about the potential concern regarding low temperature creep back in March, and I think what you've done in the interim, or what you presented here in the interim is a reflection of a very good response, and I appreciate that.

I do have a couple of questions, and I can pursue those and consider some of the implications of the questions. So, Jerry, these are mostly directed towards your presentation. If we could turn to Slide 8, the data on the left side, the graph, indicating that the annealed material was more susceptible than the cold worked material. Two questions. Is the fractography in this failure in these annealed samples the same as the fractography in samples that were exposed to air?

GORDON: Yes, it is--with some voids in the fracture surface.

LATANISION: In both cases?

GORDON: Air and creep.

LATANISION: Okay. In air and in this environment?

GORDON: In the brine environment and the creep failures look very similar.

LATANISION: The dry environment is the creep failure; right?

GORDON: Well, the air creep and the brine, fractography is ductal--

LATANISION: Okay, that's an important starting point.

Then, secondly, the implication of this data, namely that the cold worked samples are more resistant, would suggest if we now turn to Slide 16, that the as-emplaced drip shields should not be stress relief annealed. Am I reading, is that the correct implication?

GORDON: Well, it has weld residual stresses in it, presumably at yield, and the stress relief anneal drops those to very low levels.

LATANISION: So, that's intended to remove welding stresses?

GORDON: Correct.

LATANISION: Then, maybe the follow-up would be is it worthwhile considering shot peening as a means of inducing cold work into the drip shield as an approach to minimizing susceptibility, the creep, based on that data?

GORDON: It's only a surface shallow phenomenon. So, I'm not sure it would--you're not cold working the whole cross-section.

LATANISION: All right.

GORDON: So, I'm not clear necessarily it would improve the creep resistance or the stress corrosion.

LATANISION: Let me turn to the last figure, I guess that was number--not the conclusion--but, 23. My concern with the, and we've talked about this a couple of times, the fact that there are Alloy 22 feet on the bottom of the drip shield, which then are in contact with the carbon steel rails; right?

GORDON: Correct.

LATANISION: I wouldn't be so concerned about crevice corrosion in that situation as I would be that the carbon steel would be cathodically polarizing the titanium, and given the area differences, you may not see any evidence of corrosion of the steel, there's such a large area perhaps that you may not see it. But, the net result is that you would expect the carbon steel to cathodically polarize the titanium alloys and you have shown evidence that these alloys are susceptible to hydrogen induced cracking. So, is that an implication of concern?

GORDON: Well, I did so Grade 7 was cathodically polarized to extremely negative potentials, and exhibited no evidence of ductility loss. It's extremely resistant. It has a critical hydrogen content greater than 1000 ppm, compared to 400 or 500 for commercial titanium.

LATANISION: Yes, but we're dealing with a pretty long time period here, so, you know, if it's a question of how much hydrogen can you introduce in materials over a short period, I would agree. But, we're dealing with something that's got a much longer life.

GORDON: That was analyzed. There isn't a model for hydrogen induced cracking of the drip shield. I've got some backup slides, but that will take some time to go into, but we assumed 50 pounds of carbon steel contacted the titanium Grade 7 over a fairly small area. And, hydrogen distribution after 10,000 years was calculated based on diffusivities, and it turned out that the maximum hydrogen was about 400 ppm over a small area. As you go away from the contact area because of diffusion, it drops off quickly to very low values. It never exceeds, never comes close to the 1000 ppm.

LATANISION: So, you're not concerned--you're concerned neither about hydrogen induced cracking, nor stress corrosion cracking?

GORDON: Correct.

LATANISION: One last question, and this relates to the rock drops. Once again, based on the observation of the increased resistance of cold worked material, the implication would be that if you have rock drops that lead to rocks which hit the surface and bounce off and leave the surface in a cold worked condition, as opposed to sustained load, that that might not be all that bad. That would actually not induce problems of the kind we were initially concerned about; is that a correct implication?

GORDON: That's true for two reasons. They cold work the surface, and the rocks and residual stresses, but they relax from creep.

LATANISION: So, rock drops aren't necessarily a big concern with these drip shields?

GORDON: It concerns the stress corrosion, that's probably true.

LATANISION: Okay. Interesting implication.

DUQUETTE: Howard, you had a question?

ARNOLD: Yes, Arnold, Board.

My question comes from ignorance of the state of the manufacture of infrastructure with regard to titanium. Is it reasonable to assume that you could in fact buy these things either now or 20 or 30 years from now?

GORDON: We did review the design with titanium vendors, including Timet, which is in Las Vegas, is their headquarters, and they see no problem with being able to fabricate them. They're all large titanium structures fabricated.

DUQUETTE: George?

HORNBERGER: Hornberger. I have a couple questions for Mark.

Mark, you mentioned that 2 meters per second, you had collapse. Could you translate 2 meters per second to the--interval for me?

BOARD: Yeah, that corresponds to somewhat less than 10^{-6}, 2.44 meters per second was the 10^{-6} ground motion level.

HORNBERGER: So, it's fairly low probability. I mean, if you think about the waste packages being there with no drip shield for on the order of 100 years, you have some probability then that you could have rockfall directly on the waste packages.

BOARD: Probability is not my field. I'm not exactly sure how that translates to 100 years.

HORNBERGER: That's still fairly small.

The other question I had then was what's your assessment of the tunnel stability in the absence of seismic shaking?

BOARD: These tunnels are quite stable. My training is in that area, is in tunnel stability and drift stability in the area, and actually, this material is very good material for construction, and for support. Those tunnels out there right now, I know that I've taken some of the members of this group out there before, and they're in very good condition. The ground support that's in there right now is really more just from a standard of safety purpose that you have to put ground support in, obviously. But, it's very light support, that's widely spaced rock bolts, and there's no, in general, there's no wire mesh, or any other kind of support there.

The tunnels have been in excellent condition since they were constructed, which is about seven years now.

So, as far as the loading conditions from in situ stresses alone that we have, we still have a very substantial margin for design safety over and above that in situ stress loading, and also the thermal mechanical stresses as well. It's when you apply these very large seismic loads, as you know, 2 meters a second is a very large load, and in fact we know from personal experience in seismic reactive mines that the calculations that we are doing are realistic from the standpoint that they are producing groundfall, rockfall estimates that are very similar to what we actually see in reality in deep mines that have experienced those kinds of ground velocity.

But, in conclusion, it's good construction material, easily supported, no stability issues that we have, and realize, too, that the tunnels that we have are spaced apart wider than what they will interact with one another on. So, they essentially act as isolated tunnels, with no interaction, and because of that, there isn't any reinforcing, stress concentration or loading effects from other structures.

HORNBERGER: So, your anticipation if this were to remain open for even a few hundred years, in the absence of a strong earthquake, you wouldn't anticipate huge problems with collapse?

BOARD: No, I would not. As you know, we've put in some rather substantial ground support in there to make certain that we don't have issues with retrievability, and things like that. But, in general, I wouldn't have any particular issues with having these in the condition they're at standing for a long period of time.

DUQUETTE: Dr. Kadak?

KADAK: I'd like to ask this gentleman, did your analysis assume any of that structural support, what they were planning, I think stainless steel?

BOARD: No. We assumed in these post-closure analyses that the ground support that was in place, even if it's stainless steel, which are relatively thin plate structures, will have corroded and be gone at that point in time, so the tunnels are unsupported in all the work we have done.

KADAK: Those things that they're proposing to put in, you're saying they will be corroded as well?

BOARD: Given a long enough period of time, realize that the calculations we are doing here are for post-closure time frames, which can be thousands of years, and, so, our assumption from a conservative standpoint was that the ground support, even if they're made out of stainless steel, would not be existing at the time. So, we felt that was a conservative approach.

KADAK: Okay. So, the existing support structures, you're saying are adequate until closure?

BOARD: Existing meaning the ones that are in right now?

I think the question that Dr. Hornberger asked was would I feel personally that those tunnels out there would remain stable for a long period of time. And, I do personally. But, however, because of the requirements that we have of being able to retrieve the material and everything, it's more than prudent, I think, to put in ground support, so we reduce any kind of level of risk to low levels, and that's the reason that you have the ground support that's installed in there.

KADAK: Have you looked at other types of ground support for longer periods, such as some kind of a grouting system? I recall reading something about Oak Ridge has technology that they claim might be chemically okay.

BOARD: Yes, but you know, I'm not up to date on what--I know what you're talking about. First of all, cement grouts and things for tunnel linings were removed early on because of the potential for enhancing corrosion of the waste packages. And, so, we have been relying on non-organic and non-cement based ground support systems, which are the typical things to use in tunnelling operations, are cement or shotcrete lining.

Now, I understand, but maybe Mark Peters knows some more about it, but I understand that some work at Oak Ridge is being done on cement, that a low pH cement, I don't know much about that.

KADAK: Another question on impacts of a 28 metric ton boulder. Did you look at the puncture issues?

BOARD: Yes, we did.

KADAK: And, no punctures?

BOARD: No, the strain levels are actually too small. We actually did look at the potential for tensile tearing of the plates, and the strains are below that level.

I think the think, again, to realize is that these plates are very thick in nature. They're almost an inch thick, and internally supported structure with both longitudinal stiffening units, and also these lateral braces that are quite closely spaced. And, so, we don't have wide areas that are available and open to be punctured with large strain levels.

KADAK: Can I ask Mr. Gordon a question?

Mr. Gordon, your presentation kind of left me a little bit confused. I was wondering about what is it that you're finally picking as the titanium number? You had 7, 12, 16. I'm not going to say 32, but there's a lot of them. What is the design choice, and can you tell us the temperatures to which you tested these things? Are they the temperatures that the repository will be at? And, the environmental conditions, are those the environmental conditions that the repository will be at?

GORDON: Right. First of all, the design basis is titanium Grade 7 for the plate material and the welded metal. And, Grade 24 for the bulkheads and the other structural supports. Also, looking at lower cost, either lower palladium or Ruthenium placed on palladium, because the petro-chemical industry is using a lot of the Ruthenium analogues for the Grade 29, and it costs less. So, we're carrying it along, and it has very similar properties.

KADAK: At what temperature?

GORDON: Peak drip shield temperature, I'm probably not the right person to say that, I think it's certainly less than 200 C, or less, unless the drifts collapse very early.

KADAK: Okay. Well, as I recall, the numbers you were testing at was 120--

GORDON: 150 Centigrade.

KADAK: I didn't see anything as high as 200.

GORDON: In the German program, they did run to 200 degrees Centigrade, in very aggressive brine. We've tested in a range of brines, mostly at lower temperatures. But, there are data out to 200 Centigrade. If fact, there's data out to 350 Centigrade, and I show it, and in no case did crevice corrosion occur below 200 Centigrade.

DUQUETTE: This will be more of a comment than a question, although you can certainly respond to it. You mentioned the Canadian program, and Shoesmith is working with you at the present time, quite clearly showed stress corrosion cracking in virtually all of these alloys in some of the environments that they've tested in Canada. The German program did not, but the Canadian program certainly did. So, they're not immune to stress corrosion cracking or hydrogen embrittlement.

GORDON: The project assumed stress corrosion occurs if stress is greater than 50 percent of the yield.

DUQUETTE: That's right. The other comment I'd like to make, which is something that's happened to this Board more than once, and I'd like to take a quick look, a very quick look, at Pages 9 and 13 in that order.

If you take a look at those two, that's 40 ksi and 50.7 ksi. Let's take a look at 13. And, now, you're showing data at 30 ksi. And, so, you're somewhat--

GORDON: No, this is--

DUQUETTE: Let me finish my comment, please. We're looking a little bit at apples and oranges, because if you look at the data on the left-hand side in that data, that's between it looks like 4000 hours and 6000 hours, and you've got steady state something happening.

If I take a look at the data on the right-hand side, it goes up to only about 800 hours, and you stop conveniently at the same rate as you're getting at 4000 hours later, with the assumption that you've now reached steady state in the creep curve.

We'll be interested, I think, to see the total report on this to see how these compare out to those kinds of times for the creep curves to show that data way out at the far end, because I'm not at all convinced that you reached steady state in that creep curve where you've stopped the data at about the same rate as you have on the left-hand side.

GORDON: Right. I agree. I think I may have said the rate is decreasing with time. But, they are different. These are stress intensity factors, which are megapascal root meters. And, the others are stress, which is megapascal.

DUQUETTE: Right.

LATANISION: This is data that was derived from Peter Andresen? Is Peter equally convinced that this is now a creep problem?

GORDON: Failures that we've seen are creep. We're not saying that titanium Grade 7 absolutely is not susceptible to stress corrosion. We've put samples on tests, they're just starting, at lower stresses where creep is not going to be an issue for 100,000 hours or so.

LATANISION: The question that Andy Kadak raised in my mind, the issue that there are--I mean, titanium, and these grades are used in chemical process systems at temperatures in this range without any evidence that I know of of low temperature creep. Is low temperature creep something new?

GORDON: No, it's been around since the Fifties. It's in the early versions of the Metals Handbook, along with some creep curves at room temperature, for both 6 aluminum 4 vanadium and for Grade 2.

LATANISION: But, as low as 150 and 200 degrees Centigrade?

GORDON: As long as room temperature, yes.

LATANISION: Okay.

DUQUETTE: Are there questions from the Staff?

(No response.

DUQUETTE: There are three, and I hope we have very quick answers to questions from the audience, and I think it affects all three of you. As long as you have the podium, Jerry, the first one was performance of the drip shield will also depend on many dissimilar metal welds in the titanium itself, Ti Grade 7 to Ti Grade 24. Does DOE plan on examining dissimilar metal welds expected in the fabrication of the drip shield with respect to the types of corrosion reactions you might get, general corrosion, stress corrosion, hydrogen embrittlement?

GORDON: The weld metal is Grade 7, so there's a dissimilar metal weld between the weld metal and the Grade

24. We haven't yet tested that. We do hope to test that.

DUQUETTE: This one says drip shields interlock. Has heat transfer been evaluated from one spent fuel assembly to another with the drip shields in place? And, Mike, that might be your--we've left you off the hook so far, so let's drag you back in.

ANDERSON: Could you repeat that again, please?

DUQUETTE: Sure. It says that drip shields interlock. Has heat transfer been evaluated from one spent fuel assembly to another with the drip shields in place? In other words, I guess another way of putting it has a thermal analysis been done for transferring heat from one waste container through the drip shield combination to another container?

ANDERSON: Yes. And, I'll speak first to design. We've done multi-package simulations with the drip shield in place, as many as 12 different waste packages of different configurations and different heat generation rates in the K curves, with the drip shields in place, to the rock walls, and assume conduction only in the rock. And, I'm sure the folks in the science side have done many more than we have.

DUQUETTE: Thank you. And, Mark, I think this last one is for you. I think you've already answered it, but I'll let you give a very quick repeat of your answer. What happens if a rockfall occurs before the drip shield is installed? How do you get the drip shield in?

BOARD: Well, we specifically designed the ground support system that went in there to prevent that from happening. And, this relates to the familiar question. We were concerned about the potential that you could have very small rocks that rest on the track, and things like that, that would have to be cleaned to get the drip shields in place. And, so, we put in there some very robust ground support system that involves putting in plates, stainless steel that are bolted into the rock, they're stainless steel, to ensure that they would be around for a hundred years. And, they let ventilation air through, but have a small enough opening of grid work in it to be able to prevent any rocks from falling through, and it goes from invert to invert around the entire outside.

DUQUETTE: Thank you very much, all of you. And, we'll take a 10 minute break instead of a 15 minute break. Let's get back to somewhat on time.

(Whereupon, a recess was taken.)

LATANISION: We're going to continue with our discussion of corrosion if everyone will take their seats. We're going to continue the conversation regarding localized corrosion. But, before we do that, Michael Anderson has a footnote to his response to one of the questions in the preceding session.

Michael?

ANDERSON: It's actually in addition to the response that Mark made about preclosure rockfall, about what would happen. And, if memory serves properly, the ground support is not important to safety. It can't be credited. And, so, if we should have a rockfall in the preclosure, we'd have to assess the severity of that rockfall and what the implications are for the emplaced waste packages. If it's trivial, then obviously we go on. But, if it's significant, then we would probably have to recover those waste packages and do some sort of inspection. We would recover those, do an assessment of whether they need to be repackaged, and then re-emplaced in the mountain, and then the drifts would have to be remediated. So, there's some more actions that might occur in that event.

LATANISION: Thank you.

Okay, let's continue. We're going to talk about localized corrosion of the waste package. We have two speakers, and we'll follow the approach of the previous session. We'll have first Charles Bryan from Sandia speak, and Gabriel Ilevbare of Integrated Science Solutions speak.

So, Charles, you have the floor.

BRYAN: Okay, within the in-drift environment, there are two conditions that could potentially result in contact with aqueous solutions with drip shield outer barrier, waste package outer barrier. The first is seepage and the second is deliquescence of salts found in dust that are deposited on the waste package during the operational, ventilation or even post-closure periods.

The second is what we're going to talk about in this talk. What I'm going to present is an evaluation we recently performed on the potential for waste package failure due to localized corrosion caused by deliquescence. And, to evaluate that, we evaluated five different propositions.

In order for waste package failure to occur, each one of these propositions would have to be answered in the affirmative. First, what are the conditions that these deliquescent brines could form under? Can they exist at high temperatures? Second, if they do form, will they persist on the waste package surface or are there processes that will dry them out? Third, if they do persist, are they corrosive? Fourth, if potentially corrosive brines were to form, would they initiate localized corrosion? Are there processes occurring that would inhibit that? And, finally, if localized corrosion were to initiate, would penetration occur?

Okay, what are the conditions that we're talking about in the drift? This is a plot of temperature on the waste package surface, increasing to the left here, versus relative humidity as a unit fraction. And, it shows the temperature and relative humidity history of the waste package through time. Immediately after closure, and prior to ventilation, or prior to ventilation, the waste package is relatively high. As soon as ventilation begins, the waste package cools rapidly to temperatures near ambient.

We have a range of relative humidities and temperatures here representing potential, some are high and low relative humidity and temperature ranges.

As soon as closure occurs, the temperatures immediately ramp up very quickly, and within 20 years reach the maximum temperature of the waste package surface. For the highest waste packages, this is about 203 degrees Centigrade. It's a little below 160 for average waste packages, and the coolest waste packages might get only slightly above 100.

Following the maximum temperature, then these things will slowly cool over thousands of years, and the relative humidity will increase. It's during this slow cooling period that deliquescence will occur on the waste package surface. And, to determine exactly when that occurs, you have to figure out what types of salts are present, and what the deliquescent properties of those salts are.

To do that, we need to identify where the dust came from and what salts it contains, and then by experimentation, determine when those salts will deliquesce.

Now, we looked at two different sources for dust.

The first the Yucca Mountain tunnel dust collected by the USGS over the last several years. These have been analyzed and they are predominantly rock flour, material generated during tunnelling and experimental work in the drift, and they contain less than 1 percent highly soluble salts. In fact, of the 50 or so samples that have been analyzed, the highest salt content was about a half a percent.

Based on chemical analyses of materials leaked from these dusts, and based upon EQ3/6 modeling of what the initial salt assemblages was, we have identified three assemblages that are important with respect to deliquescent properties of the dust. Sodium chloride, potassium nitrate, 3 salt assemblage, including sodium nitrate, and then a 4 salt assemblage, including the other three, plus calcium nitrate.

Because there's so little salt in this, we believe that atmospheric dust brought in during ventilation are likely to comprise the majority of the salt load that's present in salt deposit on the waste packages.

We have site specific atmospheric salt data from Yucca Mountain, which shows that there's about 10 per cent salts in the atmospheric dust, and we have compositions from the National Air Fall Deposition Program. This is actually regional precipitation data, which show the composition of salts washed out of the air by rainfall.

And, you can see, here's some compositions representing the year 2000, 2001 and 2002, average values, that these salts washed out of the air are primarily calcium, ammonium, nitrate, sulfate salts. Okay? This is a little different from our initial assemblage, especially because of the ammonium here.

KADAK: At Yucca Mountain?

ANDERSON: No, this was from Red Rocks, just outside of Las Vegas here.

KADAK: Why wouldn't you do that at Yucca Mountain?

ANDERSON: Because the National Air Fall Deposition Program doesn't have a collection site at Yucca Mountain.

Okay, well, what happens to the ammonium minerals that are present in the dust, it's going to be important with respect to deliquescence. Ammonium minerals are actually very deliquescent. Actually, we don't consider those as part of the deliquescent assemblage because they are thermally unstable. They decompose at relatively low temperatures very rapidly. Ammonium nitrate decomposes to form ammonium plus nitric acid, and ammonium chloride decomposes to form ammonium plus hydrochloric acid. And, ammonium sulfate undergoes a more complex decomposition, which is somewhat slower, but it also decomposes.

We have some project data here for ammonium chloride, which shows at 150 degrees C., it decomposes completely after about ten hours. Both ammonium chloride and ammonium nitrate decompose very rapidly, within hours or days, they are completely gone from the dust system at temperatures of a few hundred, at 150 degrees C., or so. Ammonium sulfate decomposes somewhat more slowly, but decomposition continues, and over months or years, it will also be gone from the waste package surface.

Because of this, we're not including ammonium minerals in our deliquescent mineral assemblages. And, we actually subtract these out then of the atmospheric dust compositions before we examine the mineral assemblages of interest.

When we do this for the atmospheric compositions that I gave earlier, from the National Air Fall Deposition Program, we end up with mineral assemblages, deliquescent mineral assemblages, that are similar to those for the tunnel dust. In particular, the third assemblage containing calcium nitrate.

Okay, so under what conditions do these things deliquesce. In the January 2005 letter to the NWTRB, we noted that we had reason to believe that these things could deliquesce at elevated temperatures. And, this is a summary of the next few slides of the data that reports that.

Experimental data for the two salt system, we achieved a maximum boiling point, which would be a maximum deliquescence temperature of one atmosphere, a maximum boiling point of one atmosphere, which would be a maximum deliquescence temperature of 134 degrees Centigrade.

For the three salts assemblage, it didn't dry out. It transitioned to a hydrous melt at 220 degrees Centigrade, and after continuing to boil until it dried out as an anhydrous melt at 300 degrees Centigrade. This is well above the maximum waste package temperature of 203 degrees Centigrade.

So, these salt assemblages could potentially exist on the waste package under all conditions that will occur in the repository.

What about the four salt assemblage? Well, the four salt assemblage was even more extreme. It continued to boil up until the point at which, again, as a hydrous melt effect, to the point at which we could not measure anymore with the thermometer available. Again, this is much higher than the 203 degree maximum temperature on the waste package surface.

So, in response to the first question then, yes, high temperature salts can form on the waste package surface? Will they persist if they form? Well, there's some processes on the waste package surface that can cause these salts to dry out. Specifically, the brines give off acid gases. In some cases, this is sufficient to cause the brine to dry out.

Now, as was presented to the NWTRB in May 2004, calcium chloride brines at high temperatures degas readily. They are buffered by a calcium hydroxide chloride base at relatively neutral pHs, which keeps the acid gas partial pressures high because now the hydrogen ion concentration remains relatively high, and they convert entirely into this calcium chloride hydroxide phase, which is non-deliquescent. They dry out.

What about the sale assemblages that are in the ducts? Well, for monovalent salt brines, as degassing occurs, the pH rises to about 10, or slightly higher. Then, the composition of the brine is buffered to a non-variant composition by sodium carbonate precipitation. However, the pH has risen to 10, hydrogen ion concentration has dropped. And, now, acid degassing, which was initially the partial pressures of acid which were initially relatively high, have dropped and the process effectively shuts itself off. Acid degassing is no longer sufficient to dry out the brine.

One of the effects of this process, however, is that Hcl is lost more readily from solution than nitric acid is. The nitric to chloride ratio in the remaining brine increases.

The brines with calcium nitrate, calcite precipitates in these at near neutral compositions, and the acid gas partial pressures are predicted to remain high, continued degassing may occur.

Now, we haven't seen any evidence of acid degassing in experiments or dryout of these brines. But, in both of these cases, it requires that carbon dioxide diffuse into the brine in order to buffer the pH, as opposed to the calcium chloride system which is internally buffered, and the experiments weren't really designed to examine this aspect. The fact that we haven't seen this experimentally doesn't mean that it doesn't occur.

However, we don't believe it will occur sufficiently for dryout to occur within these brines. And, for that reason, the brines, at least with respect to acid degassing, will persist.

Other reactions on the waste package surface. They could react with silicate minerals in the dust. We did some modeling, EQ3/6 modeling with the tunnel dust to see what the effect of this was. And, what we see is that silicate dissolution buffers pH. Calcium, magnesium were removed from the brine. They form silicate phases, and this results generally, but not always, in an increase in the deliquescence RH.

So, in many cases, the brines would be predicted to dry out, but not in all cases. Again, based upon this analysis, we must conclude that the brines will persist on the waste package surface.

Another reaction which we haven't really investigated is the potential for chloride consumption by silicates containing chloride on the waste package.

Okay, so, they persist. If they do persist, are they corrosive? Well, compositionally, we know that initial deliquescent brines are sodium chloride, potassium, nitrate, plus or minus sodium nitrate, and calcium nitrate. The brines that form are very nitrate rich, with increasing temperature of the eutectic composition is more and more nitrate rich, and the processes that modify the brines on the waste package surface, such as acid degassing and reaction to silicate minerals, do not add a generally beneficial effect with respect to corrosion. They raise the pH. They increase the nitrate chloride ratio.

Now, I'm not going to spend a lot of time talking about this, because Gabriel is going to talk about it on the next talk. But, we had an abundance of experimental data at temperatures below 160 degrees C., which indicate that nitrate rich brines will inhibit localized corrosion.

Well, what about higher temperatures? We have some new data, some preliminary data at high temperatures, but we're not going to present it here. We'll present it at a future meeting once we have finished evaluating it, and determine what effect it's going to have on our current model.

Okay, so, then if potentially corrosive brines do form, would they initiate localized corrosion? Well, there's some processes related to the scale, very small brine volumes that form which may inhibit localized corrosion initiation. In order to evaluate this, you have to estimate the brine volumes, and we do this by determining how much dust will be deposited on the waste package surface, how much salt it contains, and then estimating what the brine volume that forms would be.

This is our model for dust deposition based upon the amount of dust that's in the air at Yucca Mountain, what the ventilation rates are over the 50 year ventilation period, and some estimates of what the particle sizes are that are brought into the repository.

This graph here shows, with change in drift position, the rate at which dust will be deposited on waste packages in the drift, or the amount of dust that will be deposited on waste packages within the drift. The inset here shows how that varies with particle size, and what we've chosen for our model here are the particle sizes that result in the most deposition of dust on the first waste package.

So, for an upper bound for dust deposited, we're calculating, for the first waste package in the drift that gets the most dust, the particle size distribution that yields the highest distribution of dust, the highest amount of dust, and we're assuming then, what we end up with then is about 26 milligrams of dust per centimeters squared on the waste package surface.

Using this then to calculate the volume, we know that it contains about 10 per cent salts. We subtract out our ammonium minerals, which we believe will sediment away, or decompose prior to brine deliquescence, and we end up with about 1.8 microliters of brine per centimeters squared on the waste package surface. This calculation is based upon a temperature of 120 degrees C. But, our brine volume decreases slightly at higher temperatures, not much, because it's going to be limited by the volume of the pure salt present and you're pretty close to that already by the time you're at 120 degrees C.

This is equivalent to about an 18 micron thick layer brine on the metal if it were spread throughout entirely on the metal surface, just to give you an estimate.

Now, what's the impact of these small brine volumes, though? First, is that within the dust, you have an unsaturated condition. Okay? The dust is not saturated. What we have are small droplets of brine in the dust. We've done an analysis of the potential for capillary retention in the dust, looking at pore sizes in the dust, and we find that the dust capillary response is characterized by pore sizes about 1 micron. This means that the brine will be suspended in the dust. Small particles, small droplets or layers may be contacting the waste package, but they will be very small. And, the contact of the brine with the metal will be limited by capillary retention in the dust.

Another effect is that there are scale limitations on the gradients within these very small droplets on the waste package surface. There are two parts to this. The first is oxygen. In order for localized corrosion to occur, there needs to be development of a depleted zone, an oxygen depleted zone in the brine. And, within these brine volumes, the distance that effusion has to occur from the surface of the brine to the center of the brine droplet are insufficient to support the development of a compositional zonation, a significant zonation.

On this graph, this is just part of a parametric study we did, in which we looked at a variety of different factors, salting out factor, brine viscosity, and we looked at how much of the depletion in the initial oxygen concentration that we could get with temperature. And, you can see that we could only get depletion of a factor of about 1/10th in the oxygen concentration in the brine going from the edge to the center of this theoretical droplet.

The effect of this can be seen in this photograph here of a piece of Alloy 22 that's undergone localized corrosion, crevice corrosion. Now, the yellow arrow indicates the original edge of the crevice corner, and the green arrow indicates the start of the zone of corrosion underneath the crevice. And, you can see that there's a zone about 100 microns thick between the two where crevice corrosion doesn't occur, because oxygen diffusing in was able to keep the corrosion from occurring.

So, here's the environment then on the waste package surface. We have an unsaturated dust with little particles of brine, little droplets of brine suspended in it. Against the waste package surface, we have small droplets of brine trapped underneath pieces of dust. And, the point of this argument is that these don't form a crevice. Okay, the brine droplets are so small. The dust particles themselves are only microns to a few tens of microns across, and the brine droplets are even smaller. It's not sufficient, there's not sufficient brine there for development of compositional gradients.

In order for an aggressive crevice solution to form, there has to be oxygen depletion, dissolution of metal, and precipitation of metal blocks have to occur in different areas in order to have a driving force for chloride into the crevice. So, without these zones occurring, the salt particles, the dust particles will not constitute crevice performance, and localized corrosion will not initiate readily on the surface of the waste package.

What about engineered crevices, the way it's contacting the pallet? Now, in these areas, it's possible that instead of having a millimeter thick layer of dust, you may have several millimeters of dust filling the crevice. It's possible that oxygen transports through the dust will be sufficiently reduced. However, again, we have very small, tiny droplets of brine trapped in this area, too small to maintain separate cathodic and anodic zones.

Okay, so if localized corrosion were to initiate, would it be possible to penetrate the waste package outer barrier? There are several processes occurring that would limit the extent to which corrosion could occur. The first is stifling. Corrosion experiments, virtually all corrosion experiments show some evidence of stifling or slowing of penetration over time. Second is physical retention of brine within the corrosion products that form, essentially capillary retention. And, the third is chemical sequestration of brine components within the corrosion products.

Corrosion stifling. I'm not going to discuss this much, because again, Gabriel will discuss it, but this is just some constant potential experiments that show that again corrosion is initially very rapid, and then decreases very rapidly with time, the rate of penetration. And, here is one of those curves with the power law for localized corrosion penetration.

Physical retention in corrosion products. Remember, we were discussing very small volumes of brine? Let's say we have a conical or hemispherical penetration through this 2 centimeter thick Alloy 22 outer barrier. In order for the brine to persist long enough to penetrate through the entire barrier as corrosion occurs, the maximum volumetric liquid content to be retained within the corrosion products is about .1 percent, less than .1 percent.

Now, is this a reasonable volumetric liquid content? Is this a reasonable amount for brine to be retained within the corrosion products? It's very unlikely. Fluid inclusions alone generally account for .1 to .5 percent in crystalline materials.

Now, we have no porosity measurements on Alloy 22 corrosion products. No one has ever corroded it sufficiently to get enough of this material to measure it. But, on an iron and steel analogues up to 800 years old, the porosity of these materials is on the order of 10 to 15 percent, and a significant fraction of that is very fine pores, which you have a very high capillarity, and a high potential for retaining the brine.

What about chemical sequestration? The salt loads are really small here. This is what we're talking about as a maximum. Remember our dust deposition model is based upon the absolute maximum amount of dust deposited. These values here, .1 milligrams of chloride per centimeter squared, 1.3 milligrams of nitrate, are based on the initial salt composition before we subtracted off the ammonium minerals, because we're not really sure at how the ammonium minerals partition between the chloride and the nitrate. So, this is the maximum amount of material deposited per centimeter squared.

The Alloy 22 outer barrier, 2 centimeters thick, if we had a conical penetration, there would be 5.9 grams of metal per centimeter squared of the surface that would be consumed and converted to corrosion products, 8 to 10 grams of corrosion products.

In order for the corrosion products to consume all the chloride present, we would only have to incorporate within them, structurally within the minerals, less than 20 parts per million chloride.

Do we see any chloride containing corrosion products in any of our experiments? Well, we haven't seen a lot, but we do see one layer of double hydroxide has been identified experimentally. Layered double hydroxide is probably a nickel chromium layered hydroxide. Layered double hydroxides are anion exchangers, and they contain anions and interlayer sides. And chloride is actually preferred over nitrate, although depending on the composition of the brine, both would be incorporated into this structure.

Okay, so, the conditions then on the waste package surface associated with dust deliquescence, we're dealing with unsaturated dust, open system with respect to gas exchange, thin layers and small droplets suspended in the dust, very limited solution volumes.

Chemical environment, sodium, chloride, potassium. Sodium chloride, sodium nitrate, potassium nitrate, calcium nitrate brines are very nitrate rich. They can form and persist at very high temperatures. Degassing or other processes on the surfaces of the waste packages do not make them more corrosive. And, we have very limited salt/brine components.

So, to go through those initial five propositions then? Can the brines form? Yes. Will they persist? Yes. Will they be corrosive? Based upon temperature data at less than 160 degrees C., we don't believe they will be corrosive. However, there is higher temperature data which we're still evaluating. If potentially corrosive brines did form, would they initiate localized corrosion? Scale limitations would prevent the development of separate anodic and cathodic regions within these tiny droplets of brine underneath the dust. If it did initiate, would it penetrate? No. We're dealing with extremely small amounts of salt on the waste package surface, and we'd have to drill through 2 centimeters of Alloy 22 to get penetration.

So, on the basis of these arguments, localized corrosion due to dust deliquescence has been screened out of Performance Assessment.

DUQUETTE: Okay, thank you. Let's move on, and we'll have our second presentation by Gabriel Ilevbare next, and then we'll open it up to questions.

ILEVBARE: Thank you. The focus of this talk will be to answer questions. First, can deliquescent brines support localized corrosion at high temperatures in repository environments? And, second, if initiated, will localized corrosion stifle?

The testing objective is to evaluate the bounds of Alloy 22 localized corrosion resistance. And, this has been done in simulated dust environments at 1 atmosphere. We have also looked at them in environments not possible in the repository conditions at very high temperature and pressure using autoclaves.

Now, autoclaves provide high temperature and high pressure environments. In this case, approximately 14 atmosphere, which allow for greater chloride concentration and lower nitrate to chloride ratios than possible in the repository conditions at 1 atmosphere.

In addition to this, autoclaves allow us to evaluate the very high temperature, high pressure environments in a safe manner over a long period of time.

There are no limitations on solution volume or amount of reactants. I'm sure you see the significance of that, based on the last talk that you've heard, about the volumes present on the containers.

The type of tests that were used for this evaluation include cyclic polarization test and immersion tests at 1 atmosphere, and elevated temperatures.

We also looked at tests in autoclaves, immersed in the liquid and vapor phase, at elevated temperatures and pressures.

Geochemical data from Yucca Mountain relevant environments, as shown, minimum nitrate and chloride ratios will increase with temperature. This graph is a graph of minimal nitrate to chloride ratio as a function of temperature, and these are salts in aqueous solutions containing potassium and sodium nitrates and chlorides. And, as you can see, at a temperature of 120 degrees Celsius, the minimum ratio of nitrates to chlorides is about 3. At 160, this minimum ratio goes up to about 25. And, as we all know from this equation of science, the higher the nitrate concentration, the less likely it is that localized corrosion will occur.

We also know from this that there is an abundance of nitrates in this environment, which contain sodium, potassium, chloride and nitrate. This graph is a graph of temperature in the boiling points of this nitrate, chloride solution, which is a function of nitrate concentration.

The nitrates are composed of equimolar amounts of potassium and sodium nitrate salts. And, as you can see, as temperature increases, the amounts of nitrates you have increases.

For example, the maximum solubility of chlorides in nitrate brines is about 9 molar at zero nitrate, and actually at 160 degrees Celsius, the maximum concentration of chlorides decreases to about 4. These deliquescent brines are, therefore, nitrate rich and chloride poor. And, these deliquescent brines are not expected to support localized corrosion, and I will show some data to support this in subsequent slides.

The first of our experiments that I shall be showing you today are from results of Alloy 22 in deliquescent environments conducted at 1 atmosphere. These are all benched experiments. Of course, the aim is to investigate resistance of Alloy 22 in deliquescent brine solutions.

The temperatures I will be looking at today ranged from 110 to 150 degrees Celsius. Chloride concentration was zero to 8 molal. Nitrate concentration from zero to 100. All the solutions are deaerated. This, of course, as I've said, represents deliquescent brines. We have unlimited solution volume or amount of reactants. They're all at atmospheric pressure. We conducted open circuit potential monitoring for 24 hours, and then followed with cyclic polarization tests on Alloy 22 samples with multiple crevice assemblies.

This is a table of all the environments, that experiments were conducted in aqueous dust deliquescent environments. All salts are added as equimolal concentrations of the sodium and potassium salts.

The ones we read are the environments that we would not expect to see in the dust deliquescent environments, and would expect to start seeing solutions that have a nitrate to chloride ratio of about .4. And, this, while unlikely, have been added for completeness and for us to be able to get a good feel for the data. And, as you can see, chloride concentration ranges from 8 molal to zero. Nitrate concentration up to 100. The ratio up to 100 also, most of the experiments were conducted at 110 degrees Celsius. A good number at 125, and some at 140 and one at 150 degrees Celsius.

LATANISION: Just a point of information, where were these tests done? Where was this program done?

ILEVBARE: Livermore, Lawrence Livermore.

LATANISION: At Livermore, okay.

ILEVBARE: Yes. And, I'd also like to draw your attention to this environment right here. It has no chloride in it at all, and that's used as a basis for comparison. So, we'll compare with environments in which we have chloride, but no localized corrosion, and also to show that when you have just nitrates in the environment, you don't get localized corrosion.

The passive region lengthens, increases the nitrate concentration in the environment. This is shown--of Alloy 22 were taken in solutions containing chloride at nitrate and chloride ratios of 0.1, .2 and .3, at 110 degrees Celsius. And, as you can see, there are a couple of things that are very important. First of all, the corrosion potential is very similar for all these environments. So, something you would notice is that ever so slightly, the passive current density decreases as increased the amount of nitrate in solution.

You also see that as you go from zero to .3 nitrate to chloride ratio, you increase the breakdown potential. One of the things that happens is that you also increase the passivation potential.

LATANISION: Another point of information. Without arrows, I can't tell which is forward or reverse. Do you have a positive hysteresis in each case?

ILEVBARE: Yes, this is forward on the red, positive hysteresis, on the blue, this is forward, hysteresis, reverse. On the green, this is forward hysteresis. And, on the black, this is forward. I apologize for the lack of arrows.

LATANISION: Okay.

ILEVBARE: The breakdown potential and repassivation potential rise with nitrate to chloride ratio at constant temperature and cyclic polarization test. This graph is a graph of potential, and it's showing the corrosion potential, the breakdown potential, and the repassivation potential as a function of nitrate to chloride ratio at--chloride concentration at 110 degrees Celsius. And, as you can see, as you increase nitrates to chloride ratio, you increase the breakdown potential, in red, and the repassivation potential, in blue, the open circuit potential is fairly constant.

What you'll also start to notice is that as you increase the nitrate to chloride ratio, the breakdown potential starts to top out at about 600 millivolts, given an allowance between the open circuit potential and the breakdown potential of at least 1000 millivolts.

Now, when you have an abundance of nitrate in solution, what you will see is that there will be complete inhibition of localized corrosion. This--the curves here, those are Alloy 22, N, just for 2 molar nitrate, at 110 degrees Celsius. That means no chloride at all. And, I have compared that with the polarization curve of Alloy 22 in 100 molar nitrate and 1 molar chloride at 150 degrees Celsius.

The nitrate to chloride ratio here is 100, and as you can see, there is no hysteresis at all. And, this polarization curve is very, very similar, which signifies that there is no localized corrosion, and as you increase the nitrates concentration, there becomes a level that increases it to--and localized corrosion completely shuts down.

The corrosion resistance improves with increase in the nitrates/chloride ratio in cyclic polarization tests. This graph is the summary of all the tests that were carried out from Livermore. They contain all the concentrations I showed you on the table previously.

And, as you can see, as you increase the nitrate/chloride ratio, you start with where you have crevice corrosion, and outside particular nitrate concentration has been exceeded, you no longer have localized breakdown. This is the data from zero concentration of chloride with 42 molar nitrate, the breakdown and repassivation potential, and the open circuit potentials.

And, one thing that you start to notice very quickly is that once you are able to shut localized corrosion down, both the crevice breakdown and the repassivation potential are very similar to those from systems where you have no chloride at all. So, this shows that Alloy 22 is highly resistant to crevice corrosion in this simulated dust environment.

Now, I'll talk briefly about the behavior of Alloy 22 in non-repository environments. By non-repository environments, we mean environments that are not possible physically at 1 atmosphere, which is around the atmospheric pressure of what will happen in the repository.

This is a table of the conditions, experimental conditions in which Alloy 22 was tested in the autoclave environment. Now, the aim of this test was to investigate the behavior of Alloy 22 at very high temperature. Obviously, we are pushing the limits of Alloy 22 to see how far it will go.

Now, the specimens that were used were non-creviced foil specimens immersed both in the liquid phase and in the vapor phase. The test temperature was between 120 and 220 degrees Celsius. Test duration was eight months, and, in nitrate to chloride ratios between 0.05 and 6.7.

Now, generally, these environments cannot exist except at high pressure, and are not possible in the repository, and that is why we required the autoclave in order to carry out this experiment. Again, the atmospheric pressure in the autoclaves was calculated to be around 14 atmosphere. And, the environments were used to prove the limits of localized corrosion susceptibility of Alloy 22.

They were deaerated, and the specimens were foils that were 51 microns thick. That's approximately the thickness of a human hair.

In order to drive home my point about the inaccessibility of this environment at atmospheric pressure, this is a graph I showed previously of temperature. The boiling points of this nitrate/chloride mixture is a function of nitrate concentration. I have put the environments on this graph, and as you can see, most of them, except this one right here, fall in this shaded zone which is inaccessible at atmospheric pressure. And, that's why this environment are not possible in the repository under atmospheric conditions.

And, the result is that there was no localized corrosion observed on all the boldly exposed foils specimens for a period of eight months at temperatures as high as 220 degrees Celsius.

I'll briefly go over stifling of localized corrosion in Alloy 22 and then I'll conclude.

There is preliminary evidence that suggests that localized corrosion will stifle if it occurs on Alloy 22. This graph is a graph of current density as a function of temperature. There are creviced samples polarized at around 100 millivolts above the repassivation potential that was determined through cyclic polarization tests. And, as you can see, even with no cathodic limitation or reactant limitation, crevice corrosion initiated, as you can see, propagated, as you can see from the sustaining of that elevated current density, over a period of time, and then stifled in these tests.

Nitrate enhances the stifling of localized corrosion. This is a graph of current density as a function of time. It shows two curves, both with 3.5 sodium chloride, but with different concentrations of nitrate. One has a nitrate to chloride ratio of 0.05 and the other has three times that nitrate to chloride ratio. And, as you can see by this experiment, the current density is similar. As the experiment progressed, the metal that had more nitrates in it had current densities that went down to lower values. Also, the point at which the current excursion started was much later and had more nitrates in the environment. Once localized corrosion initiated, the system that had more nitrates in it produced lower current density.

And, what this shows is that increased nitrate/chloride ratio of 1 delays the initiation of localized corrosion, and also results in a lower or decreased current density once localized corrosion has been initiated. These enhance stifling.

The enhancement of stifling by nitrate is apparent from this slide. This graph is a graph of current density as a function of time. The potential was about 100 millivolts above the repository, potential determined by cyclic polarization test. The difference between these two environment as you can see is the differential in the nitrate to chloride ratio. The environment in red, 3.5 molal nitrate and 0.175--I'm sorry--3.5 molal chloride and .175 molal nitrate, nitrate to chloride ratio of 0.05.

The other environment contains significantly more chloride, but also more nitrate, but mostly, it contains a nitrate to chloride ratio of .5, ten times. And, as you can see, you have a higher current density--so extends for a longer period of time when you have less nitrates, and when you have more nitrates in the solution, the current density is much less.

The result is that you have more damage here from this environment compared with when you had .5 nitrates to chloride ratio.

And, with that, I'll conclude. Deliquescent brines are nitrate rich and chloride poor. The nitrate solubility increases and chloride solubility decreases as temperature increases in sodium, potassium based deliquescent brines. Nitrate rich brines do not support localized corrosion. And, there is evidence that stifling occurs, that if localized corrosion occurs on Alloy 22, that stifling will likely occur.

Thank you.

LATANISION: Thank you. Let's see, Charles, we'll open the floor up to discussion from the Board at this point. Thank you both very much.

Let me turn to Dave Duquette.

DUQUETTE: I'm not sure where to start. Let's start with Number 11, if you would. I'm not sure what the meaning is of 100 molar nitrate solution. Back to the envelope calculation that I just did, it indicates that would be 85 grams of sodium nitrate per gram of water. That's a solid. Did you do your experiments in a solid?

ILEVBARE: Well, I did not personally run these experiments. But, I ran some experiments at extremely high concentration of salts, 6 molar chloride, and also 9 molar--18 molar nitrates. And, in this salt, when you are at temperatures below 120, they are actually solids. So, I do not expect that's electrolytes that exist, say, at 90 degrees Celsius, for instance. So, it will probably be solid.

DUQUETTE: What's the solubility of sodium nitrate and water, the maximum solubility?

ILEVBARE: I cannot give you that value off the top of my head. But, I'm sure that I can check the book of constants, and give it to you.

DUQUETTE: Because I'm a little concerned, you're using those two curves, one of them at a 42 molal concentration of nitrate and 110. 100 molal of nitrate plus 1 molal of chloride. Is it your contention that in the environment, you will have--that if you're going to use this to support your model, that in the environment, you should have 100 to 1 ratio of nitrate to chloride?

ILEVBARE: What I'm saying is that these environments are possible. And, if they are possible, this is what we should expect from Alloy 22.

DUQUETTE: I'll pass for right now.

KADAK: Are those environments possible?

ILEVBARE: They are possible, yes. Yes, they are possible.

LATANISION: Let me follow up. Let me ask you to turn to Slide 18. The whole issue of stifling is something that I'm very unclear on. In general, with localized corrosion, people typically talk about these phenomenon as being autocatalytic. In other words, once the corrosion process initiates locally, the environment becomes more aggressive with time as a consequence of metal and hydrolysis, and you've heard all that.

ILEVBARE: Yes.

LATANISION: And, in general, the rate of propagation of a pit or a crevice or even a crack typically increases with time. What is different about this situation that leads to stifling? I have to admit I've never run into the phenomenon of localized corrosion stifling. So, what is your interpretation mechanistically of what's happening?

ILEVBARE: Well, first of all, I have come across systems in which localized corrosion rate, once initiation, once propagation starts, does not increase, but actually decreases. And, there are a couple of reasons for that. First of all, it increases the path as you burrow down into the middle, so it expects that because of that, the localized corrosion will decrease.

Second of all, I see a breaking down creates deposits.

LATANISION: Creates what?

ILEVBARE: Deposits, corrosion products. These corrosion products themselves might act as barriers to diffusion. There are systems in which when you grow a thick enough scale or you grow enough corrosion product, you actually do, completely carve out, or drastically slow down the rate of corrosion. And, stifling is not repassivation, because I know a lot of people confuse stifling and repassivation. Stifling is not repassivation. Stifling is just the slow-down of the rate of corrosion.

LATANISION: But, I still do not see a mechanism that would lead me to think that slow-down should occur. On the contrary, I would think that the rates ought to increase. I mean, I see your data, and there's obviously something there, but I'm not sure I understand what it is.

Let me just continue. You're doing this in deaerated solutions; right?

ILEVBARE: Yes, we are.

(Commotion in Audience.)

LATANISION: Sure. By all means. Come up. Sure. This is a friendly dialogue that we're having.

GOPAL DE: My name is Gopal De, and I work for BSC in the Waste Package Modeling Department. Gabriel is a colleague of mine.

LATANISION: Okay.

GOPAL DE: Whenever the localized corrosion begins by the action of chloride--what you meant by rapid catalytic, right?

LATANISION: Okay.

GOPAL DE: It starts. But, inclusion of nitrate, that helps to slow down and finally, most likely, it is repassivated--because the hydrolytic product--which produces acid--that is the cause of localized corrosion; right?

LATANISION: I'll buy that.

GOPAL DE: Now, in the presence of nitrates, that is going to be simply nitric acid. If you please recall, the first experiment on corrosion done by Faraday, with iron, in dilute nitric acid, he had corrosion, but in concentrated nitric acid he didn't have corrosion. So as the process progresses with time, there will be more nitric acid generated, and nitrate is well known inhibitor in the corrosion condition.

LATANISION: You're suggesting that you actually produce something like anhydrous nitric acid as a function of time? Is that what you're saying?

GOPAL DE: No, it should be hydrous, because the hydrolysis will occur with water there.

LATANISION: We're going to need to talk, because I don't follow your argument. You know, I mean, there's something here. I think we would all agree you've got some data that's very interesting. I would have suggested, thinking about the following, you did this in deaerated solutions; right?

ILEVBARE: Yes.

LATANISION: In fact, all of your crevice experiments were done in deaerated solutions?

ILEVBARE: Yes.

LATANISION: So, I mean, the crevice that you have you would expect that you're getting not an oxygen concentration cell, but a metal ion concentration cell. And, in that case, the metal cathode would typically be in the crevice, and the anode would be distributed outside the crevice area. So, it could well be that if you did this in an aerated environment, you'd see quite different results.

ILEVBARE: Well, these are driven experiments. They are not open circuits. They are driven. They are potentiatic.

LATANISION: I understand that. But, you still have concentration effect of some kind.

ILEVBARE: There is a concentration effect; correct. Yes, there is. But, I would also expect that there will be concentration, as well as an oxygen--an anion, as well as an oxygen concentration differential.

LATANISION: Mr. Chairman, this is the kind of conversation that could go on for days. And, actually, I would welcome that because, frankly, we have heard quite a lot of information in this last hour, and, you know, we've been asking for data from the team, from the project, for a long time. I understand what you're trying to do in terms of screening corrosion, localized corrosion out of your licensing application, you're trying to do that, and I understand the logic from your perspective. But, to convince me, you're going to have to show me data that I understand. And, frankly, I have to tell you right now I'm not very convinced.

I mean, I'd love to talk more about this, and I think maybe we should do something like that, have a workshop in which we get all the corrosion people, including this gentleman, together in a room and talk about these things, because we've seen a lot here today and, frankly, there's a lot that I've seen that is not very compelling. It's an interesting argument. There's data that--you know, obviously, there's data that I take it to be good data. But, there's a lot of discussion and understanding that I don't have right now. Maybe you folks do, and I'd like to gain that understanding before I would sign off to agree that you have demonstrated that corrosion penetration will not occur under these circumstances. I'm just not convinced. But, that's my problem.

Any other questions from the Board? Dr. Kadak?

KADAK: I was trying to understand, the earlier gentleman said there was some data at 200 degrees Centigrade, but you're not presenting it, but what was this presentation? Does this have anything to do with what you were talking about earlier?

ILEVBARE: That was crevice corrosion.

KADAK: Oh, this is localized corrosion?

ILEVBARE: Right.

KADAK: Okay. Could you just tell me why, when you know the peak temperature of a canister was to be about 200 degrees Centigrade, you didn't do your experiments at that level initially? That's for both of you guys.

ILEVBARE: Well, we are gradually working our way up to 200 degrees Celsius. I should say we have nicely come up to 150 degrees Celsius, and we are gradually doing that.

BRYAN: Also, remember that we only realized that these brines are capable of existing at 200 degrees C. within the last year.

KADAK: I see. And, how convinced are you that you've got the last bunch of chemicals that could deliquesce in the sense that last time you thought the issue was solved, and then you discovered with Dr. Chu, there were some other things that were discovered in the environment? How sure are you now that you've captured the chemicals that might cause this kind of effect?

BRYAN: I think we are pretty certain that we have a good understanding of the salts that are present in both atmospheric dust and in the tunnel dust, and, the salts that are likely to contribute to deliquescence on the waste package.

KADAK: Okay. Could you have said that same thing about a year and a half ago?

BRYAN: We didn't have the understanding of atmospheric dust at that time.

KADAK: And, how comfortable are you relative to this nitrates being such a powerful inhibitor, I think is the right word, for all of your findings relative to no localized corrosion. Suppose you're somewhat off on the nitrate concentration?

ILEVBARE: Well, as you can see from what I presented, nitrate does inhibit corrosion of a wide range of potentials, and over a wide range of environments. Is it possible we're slightly off? That's a possibility. But, the footprint we have currently is very--

KADAK: So, you have a very good understanding of the dust that's in the repository, even though some of it may come from Red Rock?

BRYAN: Well, we looked at other dusts as well. The Red Rock, that was the closest one to the repository. Atmospheric dusts are characterized by high ammonium sulfate, ammonium sulfate, ammonium nitrate, much lower amounts of ammonium chloride. We have a good understanding of the atmospheric air salts, I think a good enough understanding of the atmospheric air salts throughout the area.

LATANISION: Dr. Duquette then Dr. Garrick.

DUQUETTE: Could I go to Slide 17, Charles, of yours?

BRYAN: I'm sorry?

DUQUETTE: Slide 17, please. And, then, I want to come back to that conclusion slide.

The model you have shown here shows about equal wetting of the water to the dust as to the metal, and most metals are pretty tolerable to water. They like to spread it out. It's been my impression in dealing with dust done on your level, and by the way, I'd like to congratulate you on knowing a lot more about the dust than you did 18 months ago, because 18 months ago, deliquescence was ruled out because it couldn't occur. And, now that it can occur, it's being ruled out for different reasons, and that's okay, too.

But, the fact of the matter is that this model assumes that the water went to the metal the same way as it does the dust, and if I look at that model, and if I could spread that water out and make the dust particles touch instead of being separate, I think I could develop a conceptual model, which you presented in this cartoon, that would in fact wet the metal completely and allow you to have a crevice with dust particles touching dust particles. Is that possible or not possible?

BRYAN: I would say that even if the--we have no information on the wetability of brines at 200 degrees Centigrade either to metal or to silicates. But, even if it does wet the metal more, you still have to remove that material down from where it's suspended in the dust to the metal surface. So, is it possible for larger regions to occur? Probably. How much larger? I don't think very large.

DUQUETTE: And, so, you don't like capillary condensation down through the gaps you've got down to the metal, where it wets the metal entirely? Because my impression from some chemistry of a long time ago, silicates don't like water very much, and I know that metals do.

BRYAN: We haven't evaluated that.

DUQUETTE: Okay. Can I go to your conclusion slide, please?

ILEVBARE: May I answer that before we move on, please?

DUQUETTE: Sure.

ILEVBARE: Even if the scenario as described was true, and that does occur, there is also the issue of whether or not we'll be able to have a tight enough crevice between the dust and container to try to sustain a crevice on Alloy 22.

DUQUETTE: Yes, there's the issue of that, but that hasn't been measured.

Okay, the conclusion slide, please, your conclusion slide?

BRYAN: Mine or his?

DUQUETTE: Yours, Charles. Sorry.

Okay, I don't mean to be facetious in asking this question, but Number 3 says, "If deliquescent brines persist, will they be corrosive?" And you've answered the question no, at less than 160 degrees. Dr. Kadak asked the same question. You seem to be bounding that. You've given me a taste of what I'm going to get sometime soon at 200. Am I going to be happy or unhappy?

BRYAN: Well, let me say that what that actually intended to say is based on data at less than 160, the data that we have evaluated so far, corrosion wouldn't occur. If we extended that to higher temperatures, what are the results at higher temperatures? I'll let Gabriel discuss those.

ILEVBARE: We have some data at about 220 degrees Celsius. I presented some of that data today with free-standing samples that are non-creviced, but after eight months, it shows that there's no localized corrosion.

The other data that we are talking about is data that we collected on the same foil samples with multiple crevice assemblies on them. And, in those, we did see localized corrosion. Now, we are not ready to--we're still analyzing data. We won't fully understand the data, and when we fully analyze the data and we understand it, we will present it to you.

Also, the data in question is not data that is representative of dust that represent brines that could exist at 1 atmosphere under natural conditions. This experiment has been driven very hard in an autoclave.

DUQUETTE: Thank you.

LATANISION: Dr. Garrick?

GARRICK: Garrick, Board.

In keeping with the idea to discuss this for several days, God help us, one of the things that would really help me to understand more of what's going on, this not being something that I have a real expertise in, it's clear that these temperature thresholds are very important with respect to making the case for screening or not screening. But, it's also clear from looking at your curves and data that there's quite a bit of uncertainty involved, and I was unable to propagate that uncertainty quick enough to develop a sense of what that uncertainty really is. Because these thresholds are certainly not 160, it's okay, and 161, it's not okay.

I'd kind of like to have some sense of what is the uncertainty here for these critical threshold values. And, I realize there are many variables involved. But, even if you took a set of six variables, and attempted to focus on what the range of uncertainty was for these critical thresholds, that would be very valuable. Is that something you can do in the spirit of the Performance Assessment, which is supposed to be probabilistic, it would seem that--and given the importance of very low probabilities in this work, it would seem that that could be quite illuminating.

In other words, there is a probability out there that deliquescence corrosion is going to occur under the circumstances of the repository, and it would be nice to have a better sense of what that probability is.

ILEVBARE: We'll certainly take that under advisement.

LATANISION: One last question, and then we have some questions from the audience. Mark?

ABKOWITZ: Abkowitz, Board.

I wanted to focus on this last conclusion. Based on the questions and your responses over the last 15 or 20 minutes, is that still a statement that you stand behind, or is that maybe a little too early to say?

BRYAN: Well, it's important to remember that this is a layered argument. Yes, they do exist. They persist. Are they corrosive? We don't believe they are, but we have some new experimental data that we still have to evaluate, and may have to adjust our position. If they were to form, would they initiate? Again, our calculations or modeling suggest that they wouldn't. But, even if they did initiate, the fifth step, we're still talking about very, very, very small amounts of material having to do a lot of damage. It's extremely unlikely that that occurs, and we will continue to evaluate this screening argument as we gather more data, but based upon, again, the layered argument, I believe that yes, we can screen it out, even currently.

LATANISION: I do have some questions from the audience. But, before I turn it, let me just add a corollary to the comment I made earlier.

I hope you take that comment in the spirit that I intended it, and that is to say that we have seen these conclusions in draft form or another form before, perhaps not with the answers, but nevertheless, the five items. And, we understand, you know, what direction you're headed. The Board has consistently asked to see data to demonstrate that your answers are viable, and what you've done today, once again I applaud. You've responded to our questions. You've presented some data, and you've got some useful information there. But, frankly, I think we need to talk more before it's clear to me that your data supports your answers. That's the spirit in which I made that comment.

And, I'm really quite serious. I think, John, that would be a very good topic for some sort of workshop, some format. I don't know what format, but some format, in which this could be aired more fully.

GARRICK: Good item to add to our business meeting.

LATANISION: All right, let me turn now to the questions from the audience. There are three.

One of them bears on an issue that I brought up a little earlier, and that is that the autoclave tests were conducted with no oxygen. Do you expect differences if the solution is not deaerated? Gabriel?

ILEVBARE: I don't know.

LATANISION: Okay, fair enough.

The second question. Do you consider potential accelerated uniform corrosion much higher temperatures, deliquescent conditions? Do you consider potential accelerated uniform corrosion under high temperature, say 200 degrees Centigrade, deliquescence conditions? Did you consider that?

BRYAN: I think that if I understand the question correctly, they're asking if generalized corrosion--

LATANISION: Did you consider potential accelerated uniform corrosion under higher temperatures, on the order of 200 degrees, where deliquescence will occur?

ILEVBARE: We have no data for 220 degrees Celsius in deliquescence brines. However, the data I showed for Alloy 22 in the uncreviced samples in about 220 degrees Celsius, we came up with a corrosion rate, generalized corrosion rate, on the order of one-quarter of 1 micron a year.

LATANISION: Yes, it seems to me the testing you did on foils that were uncreviced, was a response to this.

ILEVBARE: Yes.

LATANISION: Okay. Finally, do you have data showing degassing with decreased chloride ion concentration, but increased the nitrate concentration? Do you have data showing that degassing with decreased chloride and increased nitrate?

BRYAN: No. That's based on the thermodynamics. It's based on the thermodynamic data we use for the formation constants for Hcl gas and HN03 gas. It's the data that's in our Yucca Mountain database, and we have not observed that experimentally. We have not observed any degassing experimentally. But, gas to gas concentrations are very low, and, again, this is only something that we've considered recently as being a potential process.

LATANISION: Okay. Dr. Garrick?

GARRICK: Thank you very much.

All right, we now enter into the public comment phase of our meeting. And, to this point, I have information that four people would like to make comments, and the first one on the list is Dr. Jacob Paz. And, the procedure will be to come to this microphone, give your name--or the podium--and your affiliation, and a few moments.

PAZ: Good afternoon. It's quite late. I'll be short.

My main emphasis would be on the proposed EPA regulation, which I am challenging on scientific and legal grounds.

GARRICK: Would you say your affiliation, please?

PAZ: My name is Dr. Jacob D. Paz, J&L Environmental Services, Incorporated.

First of all, Yucca Mountain is, in my opinion, is not a nuclear waste site, but ultimately, a mixed waste site. A large amount of heavy metals are going to be deposited, 300,000 or more. In addition 70,000 tons of high nuclear waste. The canisters will undergo corrosion both chromium, nickel and molybdenum, plutonium, ultimately will be mixed and form mixed waste.

Second, deficiencies in the Final Environmental Impact Statements, actinides such as plutonium 239, plutonium

237 as time progresses will be converted to lead. Therefore, the radiation standard for YMP must be based upon mixed waste.

The EPA using the natural background radiation. Why the EPA did not base the proposed radiation standard on drinking water standards, why?

Several reports in the literature clearly noted high scientific uncertainty associated with epidemiological study using the natural background radiation and cancer risk. Cox 2005.

Why did the EPA ignore the National Academy of Science BEIR VII recommendations, which stated exposure to background radiation causes some concerns, additional exposures cause additional risk.

Second, why the radiation bystander effect and genomic instability were ignored by the EPA.

The EPA is using for analogy exposure to justify the proposed radiation. This analogy can be misleading. Plutonium 239 is deposited in the entire body, including the bone marrow and other tissues. While x-ray is a passing transit through the body.

Make no mistake. If we're looking x-ray of 250 kilowatts and gamma ray produce only 300 reactive oxygen free radicals per nanogram per second. In contrast, alpha emitter of 4 millivolts, such as Plutonium 239, generated 70,000 ROS, less than a microsecond.

Finally, in 1906, the State of Missouri sued the State of Illinois, that discharge of sewage from Illinois's river into the Mississippi River, increasing typhoid fever in St. Louis. The State of Missouri lost their appeal to the U.S. Supreme Court, due to the fact that experimental data raised at least serious doubts.

The same analogy can or may apply to YMP, the new EPA proposed new radiation standards.

Last, review of the literature indicated that titanium metals can produce genomic instability. Chromium exposure--exposure to Chromium 6 in drinking water increases susceptibility to UV and skin tumor in a hairless mouse. Genomic instability has been observed in the uranium. The same also applies for chromium and nickel.

Thank you.

GARRICK: Thank you, Dr. Paz.

Dr. David Dobson?

(No response.)

GARRICK: All right, let's go to our next speaker, Sally. Sally Devlin.

DEVLIN: Thank you, Mr. Chairman.

My name is Sally Devlin. I live in Pahrump, Nye County, Nevada, the home of Yucca Mountain, or at least 50 miles from there, from my house. And, I want to thank everybody for coming, as I always do, to Nevada. It's always a pleasure. And, it's a double pleasure to see so many new handsome faces.

We've been on this project since 1993, and I introduced the colloids and the bugs in 1995, and the lightening last year. So, we've had a lot of fun together. We've grown tremendously together. But, I have a question for you, and John always knows he never knows what I'm going to ask. And, that is how many of you on the Board are married? Are you married, or have been married, or have girl friends, several times, many times. Everybody raises their hand. Okay.

My profession was sales, and I've always owned my own business, and so on. And, my feeling always was when I taught sales was if you're married, you made a major sale. So, I consider each and every one of you gentlemen, and each and every one of you ladies here a salesman. And, the question is when you married, or whatever, for a short time, a long time, or whatever it was, you had a happy relationship, and my question is we have been married to Yucca Mountain all these years, but do I have a happy relationship?

And, the question is, if I can use the analogy, and that is my marriage is questionable. And, the reason that it's so questionable is how long is this thing going to continue on. I don't have a place to live. I don't have a canister. I don't have all the money in the world, but we have spent 12 billion. And, the worst part of it is what is it going to do to my health, as Jacob just pointed out.

And, living in the county that as a primitive camper and a pioneer woman, and if you know Pahrump, that's what we are, you have to understand we have no Health Department. We have nothing. All of these numbers and measurements, and what have you, come out of the air to us. We breathe that stuff every day, and we have winds of 100 miles an hour.

And of course the people of Amargosa, with their strange habits, as DOE says, are really in trouble. They had three brain dead kids from cancer and no place to report it. This is Nevada. We are a very unique state, and we are Nevadans. I've been here over 40 years.

So, all I can say to you is I want a happy marriage with Yucca Mountain, but I question, after all these years, whether as the public, and that's me, I'm usually the only one or two or three of the public, if this is a fraudulent program. And, this has come up more and more because I want to see something. You have no mine 800 miles. You have no canister, and you're still talking about it. You have no prototype for a canister and you have no time limit for testing it.

The other question is on the waste, and that is since 60 per cent of the states are going to keep their waste on site, and probably many more, will there be a need for Yucca Mountain. Are you all aware of this? I get the NRC reports and the EPA reports, and I did a report on the millirems, and what I said was to me, any of this stuff is an invasion of the body, and it's dangerous.

So, we have not a simple thing here. We have a very complex thing, and that's what marriage really is. And, so, again, I say is this a fraudulent project, and should it be stopped. And, I think this question should come up, if there are any people out there with any influence with the Congress, where is what you call accountability. And, after 13 years, I'm not finding it. As a matter of fact, over the years I'm finding less accountability.

We heard lovely, lovely presentations on metals, on the Alloy 22 and the bugs, and the creep, and the this, and the that, but what does it say to me, the public? These canisters are going to be exposed and explode, and where does this carbon 14 and all the other stuff go? It kills you. How many deaths have there been? There's been no accountability.

All the people working on the Test Site, and for you that have never been there, that's 1730 square miles, and everything in the world has been dumped on the Test Site.

They just dumped all the stuff from Sandia and Los Alamos. I've been up there to Piute Mesa three times and got a lethal dose of plutonium. The thing went off the scales.

So, it is a very hot test site, and that's the only term I know to use, and it may be the vernacular. But, it is hot, and it is dangerous. And, so, I'm asking you as a Board when you want to have these long discussions about all the nitty gritty of the metallurgy, have a long discussion about the reality of this project. My field has always been transportation, and I did--Susan Moore of Nye County did a lovely thing on the railroads, except for one thing. There hasn't been a railroad built in this country since 1974, and there is no tunnelling standard, and you can't go through the mountains.

So, the railroad from Caliente would be well over 600 miles at 5 to 8 million a mile. That's $3 billion, last I heard.

So, we're talking again tons of money. Where did this $12 billion go that you've been courting me with for the last 13 years? I don't know. Do you? I'd like to know.

I love the science. It is a challenge learning. But, where is it getting us? Is it necessary, and so on? So, I do rely on this Board to ask, and I always ask John questions, and I love it because I love him, but I'm just saying that if you propose to me and I propose to you, what are you selling me. Have you got a viable product? And, I am questioning this after 13 years.

So, that's what I'm going to leave you with. I'd like to hear more from the Board and Staff, and we're all close friends, but I've never said this before because I've always come here to learn, how much more can you learn? When does reality set in? When you've got to pay the bills and you need a home and you need food on the table and you need all the rest of the stuff that goes with the marriage, do you have it with Yucca Mountain? And, my answer is no. We don't have a marriage anymore, and even though I have to say I want to separate at this point, and I'd like to hear more from the Board because I do not think this is a viable project. I do think it has been fraud on the public, and I don't see where it's going. And, I hate to say it, but that's my feeling now.

Thank you.

GARRICK: Thank you, Sally. Your comments are always very interesting and certainly always very welcomed.

Our next speaker is Grant Hudlow.

HUDLOW: I'm Grant Hudlow. Those few of you that are still on the Board know, have listened to me for I guess as many years as Sally, and I'm usually not as funny as Sally, but today I have some bad news and some good news.

And, the bad news is primarily for the new Board members. After 20 years of bad science, and we're getting comments from the older Board members of I don't understand, the arguments are not compelling, that's a very polite way of saying that 20 years of bad science still does not add up to a viable project.

In my industry, the chemical industry, if we had this level of science to build a plant, I would expect to have a big hole in the ground within two months. And, I'm not kidding. I am amazed at the lack of technical expertise.

For example, on this Alloy 22, we've used that in the chemical industry since the Fifties, or so, and we replace it every year because it doesn't last. The reason it's used in the nuclear industry is because it can stand the vibration, and it's the best thing to stand the vibration from the steam pressure.

In the chemical industry, we have corrosion problems with it, and the reason we use it still is because it's cheaper to replace it every year than it is to use the good stuff, strictly an economical decision.

When you want something to last 10,000 years and it only lasts a year, you have a major problem. It's absolutely ridiculous that anybody would even consider using that. And, I say that for the new Board members. You have a really serious learning curve to get up to speed with the kind of nonsense that you've been told today.

Now, the good news is it looks like in a couple years, we'll start transmuting the waste. Sandia has developed a transmutation program that can turn the waste into a trillion dollars of electricity. And, to me, that's a no brainer. I would a lot rather see a trillion dollars worth of electricity than having all this stuff in my water.

The second thing is that there is a way that you can use Yucca Mountain if you really want to, and that is it can become a warehouse. You're going to have to replace the canisters probably every 20 years due to corrosion and microbial corrosion, chemical and microbial.

There's nothing on this planet that can withstand the embrittlement due to the neutrons for over 100 years that we know at this point.

So, if it's a warehouse, you set up a test facility, and when you start getting failures, you pull everything out of the mountain, and put it into new canisters. Plus, if you're going to use Alloy 22, it will cost you a billion dollars a year. Use something like the Cermet, DU Cermet, for example. You would save most of that billion dollars a year.

So, there is a way that this could be done if for some reason you want it done.

And, the really good news is that you have industry is going to put all of this waste into dry cast storage on site. So, Yucca Mountain then becomes irrelevant.

And, even better news than that is that you have two brilliant people involved in this project. One of them is John Arthur, and the other one is John Garrick. And, the reason that they are so outstanding is because they're industrial turnaround experts. They understand not only the technology behind and science behind all of this, but they also understand how to use it.

And, for those of you that would like to be up at that level, there's a trick to that. And, what you do is you go around and you ask everybody that's within two feet of you, we call it the two foot rule, what do they think about this project, the janitor, the housewives, everybody. And, when you get used to doing that, you then figure out that instead of telling people what to do, which is what other people do, that if you ask them what do you think, they will come up with brilliant ideas, brilliant answers that you yourself would never think of in a million years, and it's their idea and they will go and do it. All you have to do is get things out of their way.

And, I would certainly like to see a room full of people of John Garrick's and John Arthur's on this project. I think that it would be a no brainer and would be done very quickly. The two of them, I really, my heart goes out to them, that they are able to put up with all this nonsense and still function. And, they are able to get people involved to learn and do things that ordinarily is the people that are working with them would not have a clue of what to do or how to do it.

GARRICK: Thank you. That completes the names I have on the list. Is there anybody else in the room or the audience that would like to make a comment?

(No response.)

GARRICK: Any further comments from the Board members?

(No response.)

GARRICK: Staff?

(No response.)

GARRICK: Well, it sounds like we're about to the end of the day.

So, with that, we look forward to seeing all of you in the morning, and we are adjourned.

(Whereupon, at 5:50 p.m., the meeting was adjourned.)