UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

FALL 2001 BOARD MEETING

September 10, 2001

Holiday Inn Crowne Plaza
4255 South Paradise Road
Las Vegas, Nevada

NWTRB BOARD MEMBERS PRESENT

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

SENIOR PROFESSIONAL STAFF

Dr. Carl Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. David Diodato
Dr. John Pye

NWTRB STAFF

Dr. William D. Barnard, Executive Director
Joyce Dory, Director of Administration
Karyn Severson, Director, External Affairs
Ayako Kurihara, Editor
Linda Hiatt, Management Analyst
Linda Coultry, Staff Assistant
Davonya Barnes, Staff Assistant

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I N D E X
(Cont.)

P R O C E E D I N G S

8:00 a.m.

COHON: Good morning. My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board, and it's my pleasure to welcome you all to this meeting of our Board.

Before I talk about the meeting itself, I'm going to give you some background on the Board, and introduce the members of the Board and some special guests that we have with us today.

In 1982, Congress enacted the Nuclear Waste Policy Act, which, among other things, created the Office of Civilian Radioactive Waste Management, or OCRWM, within the U.S. Department of Energy. The Act charged OCRWM with developing repositories for the final disposal of the nation's spent nuclear fuel and high-level radioactive wastes from reprocessing.

Five years later, in 1987, Congress amended the Act to focus OCRWM's activities on the characterization of a single candidate site for final disposal, Yucca Mountain, on the western edge of the Nevada Test Site, about 100 miles north of here.

In those same amendments in 1987, Congress created the Nuclear Waste Technical Review Board as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. The full Board generally meets three or four times a year, usually in Nevada, and most often in Las Vegas. The Board is required to periodically furnish its findings, as well as its conclusions and recommendations, to Congress and to the Secretary of the Department of Energy. We do this in Congressional testimony and through our reports.

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

Before I introduce the members to you, I want to emphasize a couple of things in what I just said. First, in its wisdom, I believe, when Congress created this Board in 1987, it did so to create an independent federal agency. We are not part of DOE, and we're not part of any other federal organization or department. We are independent.

Secondly, our focus is on the science and technical aspect of OCRWM's activities. We are not a policy making body. We do not deal with policy. We do not approve the sites or approve anything that DOE does. We don't issue a license the way NRC may. Rather. through our advice and reaction to what DOE does, we influence, provide confidence for, where it's called for, in DOE's programs. So, we're independent, and our focus is on science and technology.

Now, let me introduce the members of the Board to you. And, in doing so, let me remind you that all of us serve on the Board in a part-time capacity. This is not our full-time jobs. In my case, I'm president of Carnegie-Mellon University in Pittsburgh, Pennsylvania. My technical expertise is in environmental systems analysis and especially water resources.

Now I'm going to introduce each member in turn, and when I do, I'll ask them to sort of raise their hands. John Arendt is a chemical engineer by training. After retiring from a long and distinguished career at Oak Ridge National Laboratory, John formed his own company. He specializes in many aspects of the nuclear fuel cycle, including standards and transportation. John chairs the Board's Panel on Waste Management Systems.

And here let me point out the Board maintains five panels like subcommittee's, through which we pursue specific issues. And as I introduce the members, those who chair one or more of those panels, I will point out.

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

Norman Christensen recently stepped down after serving for ten years as Dean of the Nicholas School of Environment at Duke University. That's why he's smiling, by the way, because he recently stepped down. He continues to serve as a member of the faculty at Duke, and his areas of expertise include biology and ecology.

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

Debra Knopman is a senior engineer at RAND Corporation in Arlington, Virginia. Her area of expertise is groundwater hydrology, and she chairs the Board's Panel on Site Characterization.

Priscilla Nelson is Director of the Division of Civil and Mechanical Systems in the Directorate of Engineering at the National Science Foundation. Her expertise is in geotechnical engineering.

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

Donald Runnells is professor emeritus in the Department of Geological Sciences at the University of Colorado at Boulder. He's also a technical consultant to Shepherd Miller, Inc., a firm providing environmental and engineering consultation primarily to the mining industry and, as well, to government agencies and other concerns. His expertise is in geochemistry.

Alberto Sagüés is Distinguished University Professor in the Department of Civil and Environmental Engineering at the University of South Florida in Tampa. He's an expert in materials engineering and corrosion, with particular emphasis on behavior of steel in concrete and infrastructure durability.

Jeffrey Wong is Deputy Director for Science, Pollution Prevention and Technology in the Department of Toxic Substances Control of the California Environmental Protection Agency. He is a pharmacologist and toxicologist with extensive experience and expertise in risk assessment and scientific team management. Jeff chairs our Panel on Environment, Regulations and Quality Assurance.

Those are our members. The Board is supported by a superb technical and administrative staff who are sitting in the second and third rows over there. Bill Barnard is the executive director of the Board. Unlike the members who are part-time, the staff serve in a full-time capacity.

You've probably met them already. Unfortunately, they're not in the room. But you know who I'm talking about when I say Linda Hiatt and Linda Coultry are sitting just outside the door at the desk out there. They're the two hard-working staff members who are responsible for all meeting arrangements and logistics. If you have questions about the meeting, one of the Lindas will be happy to answer those questions for you, or they'll find someone who can.

At this meeting, we have invited three consultants to assist the Board in its review of igneous activity. William Melson. I will mention them. They'll be introduced again later in the meeting, in any event. Our three consultants are William Melson of the Smithsonian Institution; Meghan Morrissey of Colorado School of Mines; and Clarence Allen, emeritus professor at Cal Tech and a former member of this Board. The consultants, as I said, will join us later when we focus on igneous activity, and we'll give them a more proper introduction at that time.

On behalf of the Board, I am very pleased to welcome two honored guests from the Swedish National Council for Nuclear Waste, also known by its acronym, KASAM. KASAM is a review organization with responsibilities similar to the Board's, to this Board's. In 1989, our two organizations entered into an informal agreement to exchange information--a relationship that continues today.

Camilla Odhnoff has chaired KASAM since 1985. She holds a doctorate in plant physiology and has taught and conducted research at notable institutions around the world. She entered Swedish politics in 1967 and served as governor of Blekinge, a province in the eastern part of Sweden, for almost 20 years. Dr. Odhnoff's many accomplishments were recognized earlier this year when she received an Honorary Doctorate from the University of Paris, a great honor.

Nils Rydell received a degree in Technical Physics from the Royal Institute of Technology. He spent over 20 years in the nuclear industry, culminating in the position of project manager for Sweden's first commercial nuclear power plant. Since 1976, he has worked in many different capacities in research and development related to nuclear waste, including posts with the International Atomic Energy Agency and the Nuclear Energy Agency of the OECD, the Organization for Economic Cooperation and Development, in Europe.

We're very pleased that you can both be with us for this meeting.

Let me turn now to the agenda for this meeting. Copies of the agenda are available on the table outside. That indicates the subjects we'll be covering and the times allotted to each of the topics, and we encourage you to follow along.

At first glance, the agenda might appear to be something of a grab-bag, and that's probably correct, and it's for good reason. This is the last meeting of this Board before the scheduled decision by the DOE whether to recommend development of a repository at Yucca Mountain. As a result, there are many topics we want to review before that decision is made.

There are, however, three broad themes that unite many of the presentations on our agenda. The first theme of the meeting is a collection of updates on the program. As is our custom at our meetings, we will hear about the overall status of the program and updates on on-going scientific studies.

This afternoon, we will also hear reports on peer reviews on the Program's work in three important scientific and technical areas. We will also be updated on Nye County's drilling program.

The second broad theme is documentation of information to support or evaluate a possible site recommendation. Tomorrow, the Environmental Protection Agency will tell us about the recently finalized standards that would serve as the acceptance criteria for a Yucca Mountain repository, if one is recommended.

Then, we will hear about plans to address the

Nuclear Regulatory Commission's Key Technical Issues. We will also hear about the Preliminary Site Suitability Evaluation, the PSSE, and supporting documents, including the Supplemental Science and Performance Analyses, SSPA.

Some of you might have noticed that there are no presentations on the agenda dealing with the Nuclear Regulatory Commission's licensing criterias for a Yucca Mountain repository, or DOE's siting guidelines. We hoped to have separate presentations on those subjects, but work on those documents has not yet been completed.

The third broad theme of the meeting is potential repository performance issues. We have scheduled a session on igneous activity on Wednesday morning. In addition, several times throughout the meeting, we'll be hearing about other potential issues, including groundwater flow and metals performance in a Yucca Mountain environment. We have also included several opportunities for representatives of the State of Nevada to discuss their work in areas that could affect a suitability decision.

Finally, let me tell you about some aspects of how we will conduct this meeting. First, it's important that I offer a disclaimer so that everybody in the audience understands what you're hearing when Board members speak, and the significance of what they say. This does not go to the content or substance of what they say, but rather a procedural point.

Our meetings are spontaneous by design. That is, the reactions of Board members to presentations, the questions they ask are not in any way scripted or determined in advance. Those of you who have attended our meetings before know that the members of the Board do not hesitate to speak their minds. The important point is that you understand that that's just exactly what they are doing. They are speaking their minds. They are not speaking on behalf of the Board. When we are articulating a Board position, we'll let you know.

An important feature of this meeting, and all of our meetings, is the opportunity for the public to make comments and to ask questions. We try to provide as many opportunities as possible for the public to participate in our meetings. We have an unusually large number of such opportunities at this meeting. Public comment periods are scheduled at the end of each half-day session, but we would prefer that if you can you hold your comments until the end of the day. The mid-day comment periods are primarily for those who cannot stay until the end of the day's session.

However, even if you are not required to leave, but you feel that your comment would be more timely at mid-day, by all means speak up and we'll be happy to include you if we can.

Those wanting to comment should sign the Public Comment Register at the check-in table outside where the Lindas are sitting, and they'll be happy to assist you in signing up and to prepare you to comment publicly when the time arises.

Let me point out, and I'll say it again later, that depending on the number of people signing up, we may have to set a time limit on individual remarks.

As an additional opportunity to question or to comment, you can give a written question to either Linda during the meeting, during the meeting itself. We'll make every effort to ask the question or read the comment into the record, as appropriate. In other words, the Chair of the meeting at that time will raise the question or offer the comment at the appropriate time rather than waiting for the public comment period. But we'll only do that if time allows. We have a very tight agenda, and it may be necessary for us to defer those written questions or comments until the public comment periods.

In addition to written questions to be asked by us during the meeting, we always welcome written comments for the record. Those of you who prefer not to make oral comments or to ask questions during the meeting, or if you have something particularly long to add to the record, you can choose this route at any time. We strongly encourage you to choose this route if you have a lengthy statement for the record. Again, just submit the written comments to either Linda, and they will be happy to make sure the materials is handled appropriately.

Tomorrow morning, we will provide yet another opportunity for public interaction. Starting at 7:15 in this room, 7:15 in the morning, we will serve up coffee, donuts, and the Board to any members of the public who would like an opportunity to express their views or just have an informal conversation with a Board member. We have found previous interactions of this type to be very useful, and we hope you'll be able to join us. That's tomorrow morning at 7:15 in this room.

Finally, in closing, let me repeat what I said earlier. This is the last meeting of this Board before the scheduled decision by DOE on whether to recommend development of a repository at Yucca Mountain. As a result, this is an especially timely meeting, and we have a very full agenda. We hope that the information presented during the next two and a half days will be as informative for those of you in the audience as we expect it to be for the Board.

Now, with those preliminaries out of the way, let's get into our agenda. It's my pleasure to introduce to you Lake Barrett, the Acting Director of OCRWM. Lake?

BARRETT: Thank you, Chairman Cohon. Good morning, Members of the Board. It's my pleasure to be here this morning to provide you an update on the status of the Program.

There have been a number of significant developments and milestones since your meeting last May. Just prior to that meeting, Secretary Abraham initiated the formal site consideration process with the release of the Yucca Mountain Science and Engineering Report. In my remarks at that meeting, I noted that we would release several additional documents during the summer. We have since completed that effort.

In July, we published Volumes 1 nd 2 of the Supplemental Science and Performance Analyses. On August 21st, we released the Preliminary Site Suitability Evaluation. With the release of the Preliminary Site Suitability Evaluation, the Department announced the schedule for public hearings in Nevada to receive comments on whether or not the Secretary should recommend the Yucca Mountain site.

The first public hearing was held here in Las Vegas last Wednesday. This hearing was video conferenced simultaneously to Reno, Carson City and Elko, and broadcast live over the internet to all interested parties. The hearing was also video conferenced from the Senate, allowing the Nevada's entire congressional delegation to contribute their views.

Another hearing is scheduled for this Wednesday in Amargosa Valley, and the final hearing will be held this Thursday in Pahrump. In addition to the schedule for public hearings, on August 21st, the Department announced the ongoing public comment period, which began on May 4th, would extend until September 20th. Subsequently, on September 5th, the Secretary extended the comment period an additional 15 days, until October 5, 2001. The comments received during these hearings, as well as those submitted in other forms, will be an important part of the site consideration process.

Over the next several months, the Secretary will carefully consider a large body of scientific information, as well as views from the public, in deciding whether or not to recommend the site. While committed to making progress, the Secretary has also committed to ensuring that sound science governs each decision.

The public's views on the validity of our work are important in any decision by the Secretary. To encourage public participation in that process, the Department has sent a letter to government officials and members of the public whose interest in commenting we had anticipated. This letter includes a list of suggested topics for public consideration regarding a possible site recommendation. The list is not intended to be comprehensive, nor is it intended to inhibit the public from commenting on any relevant issue related to a possible recommendation of the site. We also published a copy of that letter in the Federal Register.

Last year, the Board identified its priorities for improvements in our technical programs. In response to the concerns of the Board, we have completed a substantial body of technical work on the four areas that you recommended. The status of these efforts were presented at the Board's Panel meeting in June. The feedback we received during that meeting is being strongly considered in the development of our work plans for FY 02 and beyond. We look forward to receiving further comments from the Board regarding its review of those documents that we have published.

The analyses supporting the Yucca Mountain Science and Engineering Report were based on a flexible design that could operate over a range of temperatures, with the primary analysis and a mode that allowed the drift wall temperatures to exceed boiling after closure. This design and associated analyses were used as the basis for the Analysis and Model Reports supporting the Total System Performance Assessment.

Over the past several years, the Board and other peer review panels have raised good questions regarding the quantification of uncertainties associated with coupled processes caused by the thermal pulse. Concerns have also been raised that certain corrosion processes may be accelerated at higher temperatures, and that these processes could introduce additional uncertainties. To respond to these concerns, we performance analyses to demonstrate the ability to operate the existing design concept over a range of temperatures by varying operational parameters such as ventilation, waste package capacity, and waste package spacing. By adjusting these parameters, the impact of the thermal pulse can be managed to maintain rock temperatures and waste package temperatures at levels that may reduce uncertainties.

While completing these analyses, the Program has been evaluating options for a path forward with regard to the flexible design concept. The goal of this effort is to refine our approach toward developing a license application with a sufficient technical basis, while balancing broader programmatic constraints. These constraints include schedule expectations for both submitting a license application and receiving waste, should the site be designated, and the limitations in available funds based on the level of likely appropriations as well as the time lag in the appropriation process.

We believe the needs of the Nation may best be met by preserving the ability to select, from a broad thermal range, a design for repository licensing and initial operations. Preserving this ability, however, may require testing and analytical efforts to support production of license quality documentation for the lower end of the thermal range. This documentation would supplement the analysis for the upper end of the thermal range, and the technical and programmatic information developed would be used to further support the lower end of the thermal range in a potential license application.

Accordingly, we have directed technical direction to Bechtel-SAIC Corporation to begin evaluating this work so that the overall cost and schedule impacts of this effort can be fully understood. Our 2002 budget, which at this point is very uncertain, will strongly influence our ability to on when to implement this work. We will evaluate these cost and schedule impacts in light of these broader programmatic constraints and make decisions regarding the schedule for the implementation of the technical work. The main issue to me is not doing the work, but it's the timing on when we will be able to do that work. We will keep you informed on our progress and the decisions on this important topic.

As I have noted, the Program's Fiscal Year 2002 budget is a very key concern. The President's Budget Request was $445 million for the Program. Over the summer, both Houses of Congress have considered the budget request, and each has taken decidedly different action on our budget request. The House mark was $443 million, very close to the

President's request.

The Senate mark, however, was $275 million, which is far below the funding level that is needed to maintain the Program's schedule and momentum toward fulfilling the government's legal obligation to manage and dispose of spent nuclear fuel and radioactive high-level waste. In addition, the Senate mark also contained language concerning the Program direction accounts, which if we were ever to attempt to do what was requested, we'd basically have to lay off the entire federal staff. I've assured federal staff that that is a very unlikely scenario. But this is just some of the uncertainties that we have to deal with.

The Senate mark, however, is tempered by a manager's amendment that contains a "Sense of the Senate" statement suggesting that a funding level more consistent with the Administration's request should be worked out in conference. The conference on the appropriations bill is expected late this month, or possibly even early October. This continues an uncertainty which is very difficult for us to manage within the program. We look forward to having some national resolution on this very important matter.

Should the actual appropriation reflect the Senate mark, the Site Recommendation would be very much in jeopardy, because the technical work to address the Board's issues and the Nuclear Regulatory Commission's KTI issues would have to be deferred for some substantial period of time.

We recognize that our commitments toward addressing these concerns are central to the Commission's views and your views concerning the site recommendation process. The schedule for other key milestones, such as the license application and receipt of waste, would slip indefinitely while a new program is structured with a totally different funding level.

Although the main points of focus have been a possible site recommendation and the budget issues, there are other issues going on within the Program that I would like to mention to you. The Nuclear Regulatory Commission last Friday in a vote voted to affirm the final regulations for a possible repository at Yucca Mountain with 10 CFR 63. This will bring the NRC regulations into conformance with the EPA's 40 CFR 197 standards, which include the drinking water standard. This is an important step in the regulatory structure toward a possible site recommendation. We will follow as quickly as we can behind the Nuclear Regulatory Commission with our own site and guideline revisions, 10 CFR 963.

Also on the NRC front, I believe it's important for you to know that the NRC has been extremely aggressive and critical of the Program concerning our difficulties in implementing the NRC required quality assurance disciplines and documentation in our Yucca Mountain technical progress.

Last summer, we started a program initiative to transition into a potential license application development nuclear organization. However, we were unable to make as much progress in that area as I had hoped. Just after this initiative started last year, we had to shift our primary focus to strengthen the technical basis for the SR decision, and deferred some of the infrastructure management improvements that we had tried to accomplish.

We have restructured and re-invigorated both the Bechtel and the DOE programs to establish the necessary attributes required to be a potential licensee before the Nuclear Regulatory Commission. We described this program in detail to the Nuclear Regulatory Commission staff in two very intense open to the public meetings last Thursday and Friday. Your staff was in attendance for most of those meetings. The actual length of those two meetings totalled over 13 hours last week.

Also on the legal front, I'd like to mention some areas that happened there. This is a very complex and time consuming area also. There are 17 damage lawsuits that our Program is now being deluged by court mandated interrogatories and discovery requests considering fuel receipt and also regarding our contractual obligations. There will also be a very important oral argument on December

5th before the Eleventh Circuit Court of Appeals, where Alabama Power, et al claimed that we did not have the authority to settle the Pico agreement with an adjustment of charge. They claim that the reduction of payments by Pico into the waste fund increases the possibility that they may have a fee increase if the current waste fund cannot support the total life cycle system costs for the program.

I would also mention that the General Accounting Office is performing a very in depth inquiry into our schedule, work, scope and control processes, with emphasis on controlling the total system life cycle costs and our ability to achieve the 2010 fuel receipt goal. And also the DOE Inspector General is currently evaluating our spent fuel acceptance technical interfaces with the utility contract holders, and separately investigating, at the request of the Nevada Delegation, a conflict of interest allegation against our licensing legal support contractor. But all of these other areas I'm trying very much to isolate the project technical work from this, so the project's primary focus is and remains establishing a sufficient technical basis for the decisions that are at hand before us.

I should also mention that last week, we sent a fairly important report to Congress, that has the alternate means of finding and managing this program. This was requested by Congress last year, and it was forwarded to the

Congress by the Secretary last Wednesday. I expect we will have copies of it available for the Board tomorrow here, not the technical issues, but managerial and financial.

Now, I would like to turn toward repository development strategies. As we approach the Secretarial decision on whether or not to recommend the site, the Department and other organizations are evaluating refinements to the longstanding strategies for repository development.

For example, during the National Research Council Board on Radioactive Waste Management's international workshop in 1999 on geologic disposal, the concepts of reversibility and staged repository development received considerable attention. The term "repository staging" describes a process by which decisions concerning repository design, development, operation and possible closure are made in a stepwise and potentially reversible fashion, with adequate technical bases for each step.

The decision to proceed at each step in the process is made commensurate with the level of technical and policy understanding that is available at that time, and in a manner that allows for subsequent reversibility if that is necessary from a societal perspective. The Department believes that this approach may have significant benefits, including the opportunity for continuous learning and improvement over the life of the Program.

Concepts involved a repository staging strategy. However, this concept is not well understood in an operational sense. To advance this understanding, we have contracted with the National Research Council for advice on design and operational strategies associated with the concept of a staged geologic repository facility. Last week, the National Research Council held a workshop in Washington on this topic. We expect an interim report from this panel late this year, and a final report next year.

In March of 1996, this Board issued a report focused on the balance between storage and disposal of commercial nuclear fuel. The impetus of that report came from legislative proposals which would have effectively deferred the development of a repository in favor of central interim storage, thereby shifting the national policy focus from permanent disposal to temporary storage.

After reviewing dozens of technical and non-technical issues, the Board at that time concluded that although there was no compelling technical reason for moving fuel to a centralized facility during the 1990s, that federal interim storage capacity would be needed late in this decade.

The Board's report also emphasized the need for a balance between the efforts aimed at permanent disposal and those associated with timely acceptance of commercial spent fuel. You further recommended that consideration and development of spent fuel acceptance and storage capabilities await the decision on the suitability of the Yucca Mountain Site. The Board is aware that that decision is near.

I am also sure you are aware that strong constituencies, including the entire Nevada Delegation, believe that spent fuel should stay stored at reactor sites, and that our budget authority should be shifted to research on accelerated transportation of wastes. Others claim that there will never be a sufficient technical basis for Yucca Mountain, or any other repository, and that our program should be abolished.

Therefore, I expect a renewed debate regarding strategies for developing the Yucca Mountain facility, and also with the option of a redirection of the policies set out in the Nuclear Waste Policy Act. Decisions which this Board will be an important part of will balance the technical, programmatic, institutional and fiscal requirements that are facing this nation and the entire globe in the post-cold war environment that we find ourselves in. I'm sure you are aware of developments within the Russian Federation regarding the law changes on high level waste, and the virtual recent collapse of the German repository program.

On a related subject, the Department has received a proposal for a study by the National Academy of Sciences to examine the long-term surface storage of civilian spent fuel and defense wastes. The Academy has proposed a careful analysis of the technical, institutional, economic and policy dimensions of the complex issues of surface disposition. We presently have that request under study.

Should we decide to fund that study, I expect the Board's 1996 report, as well as current deliberations, will be considered and be an important part of any such study. This study would also add to the work that we did on the Draft Environmental Impact Statement on the no action alternative, where we evaluated in a crude sense the environmental impacts of perpetual surface storage of spent fuel at its existing locations.

We and you have met dozens of times over the past decade, and throughout those meetings, we have described our plans for characterizing the Yucca Mountain site and evaluating its suitability for development as a repository. The Program has made considerable progress and conducted what I believe is a world-class investigative science program to determine whether or not the Yucca Mountain site is suitable for development as a nuclear fuel facility.

Despite these enormous challenges, we have maintained essential momentum to implement the nation's policy for the responsible management of spent nuclear fuel and high-level waste, and we are now reaching key decision points. We look forward to receiving your comments on our work and your views on the sufficiency of the work that we have done to support the decisions at hand.

Throughout this process, your constructive feedback on our activities has been instrumental to provide decision-makers and ourselves with an independent perspective on the technical basis for decisions regarding geologic disposal. I believe your recommendations have led to a further strengthening of our technical program, especially in influencing the evolutionary, stepwise design process.

The stepwise development of a geological repository facility, with design and operational flexibility and reversibility, coupled with continuous learning feedback loops, could be extremely important for a first of a kind program like this.

Looking ahead, I believe stepwise development provides a societal approach for accommodating uncertainties in decision-making without foreclosing designs and operational approaches that could provide superior protection for the public health and safety and the environment. I encourage the Board to consider this critical issue in the coming year, and look forward to your input on the work that we've been doing.

I thank you for your past and future contributions, and would like to entertain any questions you have for me now, or I intend to remain here through the entire two and a half days, at any time you wish.

COHON: Thank you, Mr. Barrett. Paul Craig?

CRAIG: Lake, in Jerry's initial remarks, he noted that the Board is concerned with scientific and technical aspects of Yucca Mountain. And, in my view, this includes the accuracy of DOE's communication with the public.

This question relates to instances where the DOE provides incorrect information to the public. It occurred recently that my local newspaper had a report in which a project spokesperson gave information that was inconsistent with what we've heard here, specifically that there was--that the measurements on C-22 were incorrect, and there were some rates of motion of water through the UZ, which orders of magnitude below the reports that we've heard here.

Now, what I'd like to understand is what is DOE's policy with respect to errors of science and technology that are made to the public and to the press, or your policy with respect to correcting errors when they come to your attention.

BARRETT: Our intention is not to make errors. We're human. If we make errors, as soon as we find out about them, we try to rectify and correct them. When it comes to reports in the press, that is a challenge. If it's an error that we honestly made, either honestly or dishonestly, I don't believe we've made any dishonest, if we ever made an error, we will correct it as soon as it's aware. If there is something you have, if I can have the facts on it, I will try to deal with it, and, you know, give it to the editor or errata sheet or whatever the case may be.

CRAIG: It would be helpful. In this particular instance, there were direct quotations, so it was not a question of misinterpretation.

BARRETT: Well, if it's a--let us have the facts, and we certainly would like to rectify that. We do not want to have the public to have any more misinformation than they already get.

CRAIG: It would help the credibility of the agency.

BARRETT: Please point out those to us, and we will deal with those, you know, as quickly as we can if we made errors.

COHON: It's certainly an important point, but it's a new thought to think of the media as infallible, as getting quotations correct. Dan Bullen?

BULLEN: Bullen, Board.

Lake, I wanted to kind of follow up a little bit on your comments with respect to the evaluation of the lower end of the thermal loading, and your direction to BSC to kind of come up with a plan and a time table for that work. And I guess I wanted to ask specifically with respect to the integration of that work with sort of the revision of the QA issues that you have with the Nuclear Regulatory Commission, are those coupled issues? Basically, when you go back and take a look at the AMRs and PMRs to resolve those QA issues with the NRC, is that a prime opportunity for you to then go ahead and fill in the needed areas of information for those AMRs and PMRs for the low temperature operating mode?

BARRETT: What we're trying to do is integrate these together. Yes, there's opportunity where that can go together. We first ask what are our available resources and what is the national direction regarding this program in our 2002 work. Whatever monies we have available in 2002, we want to make the best we can in the balance program, balancing the nuclear discipline that the NRC requires of the potential licensee, coupled with, you know, this date, the art and science that this Board has pushed. So, we're trying to get these balanced. If there are synergisms in those, we will certainly do so. But, right now, we've asked Bechtel to look at this, integrate it together, and let's give us options and then we'll evaluate this all as soon as the budget becomes clear to go forward.

BULLEN: Thank you. Just one more quick little question. One of the issues that sort of engenders a little bit more confidence in the public is that if you do later on find a fatal flaw, do you have an exit strategy. And so would the hope be that you'd have to take a look at this stepwise approach and then at some point along the way, evaluate it with criteria to determine whether or not you proceed, and at what point you decide to proceed, or decide not to proceed? Are those kinds of things that the NAS is going to evaluate for you?

BARRETT: Yes. A lot of that is already in the basic aspects of the Act as it exists. For example, the site suitability decision, followed with the license application, followed with a rigorous licensing process involving the public, then a license amendment for receipt of material, and then later on after a lot of the confirmatory work is done and adjustments as necessary, closure, which would be in our plans nominal a hundred years from now, when we will have a lot more information regarding what some of the uncertainties are. So, this would be introducing more of that. We generally do not use the word staged licensing, which had history of another time jargon, so we don't use that term. But, yes, I think do go together.

COHON: Thank you. A couple of questions, one with regard to budget. You noted the great uncertainty of course with regard to the FY 2002 budget, and there's not much you can do about that. I have sort of a general procedural question, not specifically what you're going to do about that budget, but in a case where you have such widely varying marks between the two Houses of Congress, what do you do on October 1st? What kind of assumptions do you make so you can keep moving forward?

BARRETT: I'm afraid that there will probably not be resolution in the Congress on October 1st. So, it likely will be under a continuing resolution, and there are a great many different interpretations of how to implement a continuing resolution within the Executive Branch. The classical way is you operate on the lower of the two marks. For us to do that, we couldn't really do that, so we would probably operate on last year's, which would be a continuing resolution, so it would operate on the nominal 400 level.

What's made this a little difficult for us is we started the year low in costing, which was by design, with the new Bechtel contractor, and we're accelerating on up when Bechtel was bringing things on. So our burn rate is higher now than 400, so we're going to have to kind of look at it. We've instituted, a month and a half ago, we instituted a hiring freeze in BSC to try to contain those costs. But it would be my intent to operate at the current level, the nominal 390, 400 level. Hopefully, it would quickly resolve itself.

What happens if it goes on like a month or so burning at the higher rate, and if we end up with a very much reduced one, then the layoffs become much greater, because I've already burned, say if it's a month, one-twelfth of the year at the higher rate, and makes larger impact. So, the quicker the nation resolves, the better. But I intend to operate at the 400, unless I get written instructions from somebody higher than me not to do that. We still have the laws that we have to abide by, or Dyer and I go to jail. But we can deal with that.

COHON: We wouldn't want that. You mentioned in your remarks NRC's approval of 10 CFR 63. What are DOE's plans with regard to its second guidelines?

BARRETT: Well, the NRC--what happened, the Commission voted to affirm the 63, and they gave instructions to the staff, and within the next week or so, the staff will actually issue, through their administrative processes, 10 CFR 63.

I am hopeful that within the next 30 days or so, the Commission will act on our request of May of last year, or the year before, for concurrence on our siting guidelines, 10 CFR 963. I expect that they will do so within that 30 day period. As soon as they do, we will then make whatever modifications we need to to 963, which are very minimal in any substance, because we just referred to the EPA and the NRC, and put that through the federal process, which would be OMB and out. So, I'm hopeful--we'll move as quickly as we can, and to put the regulatory structure in place to support decisions toward the end of the year. There's litigation already in the EPA. I suspect there will be more litigation when the NRC actions become final, and our actions become final, and then it will be a matter of course. But we will move expeditiously, which I suspect would be within the next month.

COHON: Thank you, Lake. Debra Knopman?

KNOPMAN: Lake, would you comment a little bit about how the schedule now is adjusted with the extended comment period, just the way that the Department intends to respond to public comment prior to a decision?

BARRETT: We will first of all follow the letter of the law exactly and do all the comments and NEPA regulations and NEPA case law, as well as the Nuclear Waste Policy Act laws. So we will do all of that.

In addition, the hearing last week was not a satisfying experience for any party. Hopefully, the one this week will be a little better, but this is not an easy communication with the public. For example, there's not been really time in the structure that we have of formal hearings for any answer of questions, for those who wish it. There are many who don't wish it.

So, the Secretary has asked that we look at ways that we can increase the public process, and we are presently looking at that, and we'll see what implications that would have regarding the schedules. But from a technical point of view and a programmatic point of view, we are still continuing to try to prepare the necessary foundation aspects for a decision around the end of the year.

KNOPMAN: Will there be a written response to public comments prior to the decision?

BARRETT: There will be a response--in the decision package, there will be responses to the comments. Exactly how we sequence and what we can do ahead is a balancing between legal considerations as well as our desire to get back and communicate matters that people have brought up. So, we have to balance that out in consultation with the general counsel. But this will all be litigated.

COHON: Any other questions from the Board?

(No response.)

COHON: We have a little bit of extra time, and I know Judy Treichel had a question for Lake. I don't know if you want to ask it now, Judy, taking advantage of the little extra time we have.

TREICHEL: Thank you. Judy Treichel, Nevada Nuclear Waste Task Force. This is a good time to ask it, particularly following on the questions that were just asked.

On behalf of the Task Force and other public representative groups here in Nevada, we would like to know if it is possible to get a copy of whatever the guidance is, or the process is, that you are evaluating, cataloging, doing whatever it is that you do with public comments. There must be some sort of written guidance for that, and we would very much like to see what it is you're using as a score sheet, because there is, as you know, the meeting was a disaster, and there are a lot of people who are very concerned about how those comments get evaluated.

BARRETT: Our process on that follows the law. As I said, we are taking the comments. We will catalog the comments and we will respond to the comments as we go forward. We will--so that's what our process is, and we're in the process of doing that, and looking for ways that we can better connect and communicate with the public.

TREICHEL: Is there anything written down? Like you're waiting now to receive from the NRC the Yucca Mountain Review Plan. Did you have any sort of a plan or a written guidance for the acceptance--I know you have 30 to 40 people working on public comments, and there is nothing that's written there.

BARRETT: The general framework that we have for responding to the NWPA comments, let me say, is patterned after the approaches that we do for the NEPA comments. Basically, we receive the comments in, we catalog the comments, and we respond to the comments in a written form. So, that is the general approach we use. We're mirroring the approach we're using on NEPA for the FEIS response. We're mirroring that process for the NWPA comments. And we're preparing basically internal issue papers that address the same issues, which are common themes in both processes, but they're separate legal processes.

TREICHEL: All right, thank you.

COHON: Thank you very much, Lake.

BARRETT: Thank you.

COHON: We'll now hear from Russ Dyer. Russ is the Project Manager at the Yucca Mountain Site Characterization Office. Dr. Dyer?

DYER: What I'd like to do today is to walk through an update for Yucca Mountain. I'm going to hit on some of the same things that Lake was talking about in a little more detail, and set the stage for some of the talks that will come a little later.

I'll talk about a perspective on site recommendation, the process, where we are, talk about path forward and plans for Fiscal Year 2002. As Lake said already, there's considerable uncertainty in the Fiscal Year 02 budget. And we'll talk about, set the stage really for the actions completed and planned work in the four areas of Board concern.

First, just a little summary of some of the information that has come out supporting the site recommendation decision process.

Between the 4th of May and the 21st of August, there has been quite a bit of information that has been released for public review. The Yucca Mountain Science and Engineering Report came out on the 4th of May which describes the results of site characterization studies completed to date, the waste forms, the repository and waste package conceptual designs, and updated assessments of long term performance of the potential repository.

A Supplement to the Draft Environmental Impact Statement also came out on the 4th of May which gave additional information on design evolution. A most recent version of the Total System Life Cycle Cost Report came out also on the 4th of May.

And the last thing that was released on the 4th of May was the Nuclear Waste Fund Fee Adequacy Report which looks at the most recent estimate of the adequacy of the Nuclear Waste Fund Fee for covering the cost of the Program.

There have been a series of reports that have come out since then. The Supplemental Science and Performance Analyses report, two volumes, Volume 1 came out on the 30th of June, which updates the scientific bases and analyses, describes new and updated information developed since the Science and Engineering Report, and the results of the unquantified uncertainty and lower-temperature operating mode analyses.

Volume 2, performance analyses, describes the performance assessment analyses using the updated information described in Volume 1.

And the Preliminary Site Suitability Evaluation was released on the 21st of August. This describes the results of a preliminary evaluation of the Yucca Mountain repository system against the Department of Energy's proposed suitability guidelines at 10 CFR 963, which we were just discussing.

There were three public hearings announced that would inform the site recommendation decision. The first of those hearings was held last week on Wednesday, September 5th, here in Las Vegas. This included, as Lake said, interactive audio/video link-ups to Carson City, Reno and Elko. It was also webcast, a one-way webcast, non-interactive.

We have two more public hearings this week, Wednesday the 12th in Amargosa Valley, Thursday the 13th in Pahrump, Nevada.

This chart lays out the public involvement opportunities in 2001 for both the Environmental Impact Statement, this is the NEPA process, and also the Site Recommendation Decision Process.

We held a series of public hearings after the Supplement to the Draft EIS. The comment period is closed. We are in the process of considering the public comments, with a final Environmental Impact Statement to accompany the possible recommendation.

The Site Recommendation Process, whenever we release the suite of documents on the 4th of May, we opened a public comment period. When the public hearings were scheduled and announced here in mid September, there was a closure date identified for the public comment period. The Secretary has since extended that by 15 days. So the current closure date for the public comment period is the 5th of October. And as Lake was saying, we'll be considering the public comments to inform the Secretary for his decision.

Just a reminder in the public comment process, we're taking comments just by any means available, either written or oral, at the public hearings. Comments mailed in, this is the address. Carol Hanlon is out officer in charge of that. We'll also take e-mail, comments by e-mail, or by fax, and here's the relevant addresses to get comments to us associated with the site recommendation decision.

Just a quick reminder of where the process stands here. This is the charge of the Nuclear Waste Policy Act. These are the public hearings in the vicinity of the Yucca Mountain site which are called for by the Nuclear Waste Policy Act. The Secretary's decision whether or not to recommend the site to the President lies ahead of us somewhere.

If the Secretary decides to recommend the site, he must notify the governor and the legislature of the State of Nevada at least 30 days before submitting the recommendation to the President.

And then there are two paths here. There's a decision tree to go through. If the Secretary or President does not recommend the site, they notify the governor, immediately stop site characterization, and the Secretary reports to Congress within six months on recommendations for further action.

If the Secretary recommends to the President, and the President recommends the site to the Congress, should the governor or legislature not submit a notice of disapproval, the site designation becomes effective. If the governor or legislature submits a notice of disapproval within 60 days, then the site would be disapproved unless Congress passes a resolution of siting approval within the first 90 days of continuous session. And this would be a simple majority of both Houses of Congress required to override the veto.

Let me talk a little bit about the planning focus for Fiscal Year 02. We will be focusing our resources on strengthening the infrastructure to respond appropriately to the results of the site recommendation decision process. There is essentially three ways that decision can come out, as we kind of went through on the decision tree in the previous slide.

If the Secretary decides not to recommend the site, DOE has six months to report back to Congress with recommendations and begin site reclamation.

If Congress does not designate the site, DOE will respond to Congressional direction. We assume that they will provide some direction back to the Department.

If the site is recommended and designated, DOE will proceed toward licensing through a planned set of pre-licensing activities. And this is what our planning basis is primarily focused on, but understanding that these are also possibilities.

Let's talk about some of these pre-licensing activities. Now, one thing I want to point out is that these are not just Fiscal Year 02 activities. Many of these are multi-year activities.

Complete technical work to meet the Nuclear Regulatory Commission/DOE agreements that are addressed in the Key Technical Issues, in NRC's Key Technical Issues.

Develop a level of design detail appropriate for inclusion in a license application; update process models and continue analyses of uncertainties; conduct an Integrated Safety Analysis for preclosure operations; conduct Total System Performance Assessment for the license application.

Support the Nuclear Regulatory Commission's adoption of an Environmental Impact Statement; develop and certify licensing support network under 10 CFR, Part 2, Subpart J; resolve outstanding quality assurance issues.

Develop descriptions of post-licensing programs. Some examples given, performance confirmation, safeguards and security issues; and prepare and submit a license application.

Let me give a summary and kind of a preview of some of the discussion that will be held later, primarily today, about some of the Department's work in response to some of the four major concerns that were raised by the Board.

Since the Viability Assessment, the Board has identified, expressed, and focused their views and concerns on DOE's basis for a possible site recommendation.

Of course, in January 2001, in Amargosa Valley, these views and concerns coalesced into four specific areas of concern, which I'm going to hit on in a little more detail in the following slides. Meaningful quantification of uncertainties; progress in understanding corrosion processes; comparison of lower and higher-temperature designs; and multiple lines of evidence developed independent of Total System Performance Assessment.

DOE has worked aggressively to address the Board's concerns. We developed the Supplemental Science and Performance Analyses, which treat the uncertainty issue. We convened an independent peer review of corrosion processes. I think Joe Payer will talk a little later about some of the interim results there. We have reviewed the work needed to evaluate a range of operating modes, and Lake talked about some of the work that's being brought into at least in the planning stages here. We more clearly recognized the importance of and explained the multiple lines of evidence. And we convened an international Total System Performance Assessment peer review, which concluded last week.

Let's go to the first area of concern that the Board laid out, meaningful quantification of uncertainties. The Board has stated that meaningful quantification of uncertainties associated with performance is essential for policy makers. Bill Boyle has talked about this several times in May and also in June, and will I think be the first presenter in the panel following this.

We began an effort to quantify the unquantified uncertainties in late 2000. Bill will talk about our progress in this arena.

We'll continue to revisit uncertainty evaluations as new information comes to light to ensure that the effects of minor uncertainties do not have a non-negligible cumulative effect.

We've addressed the Board's concern and believe that uncertainties are sufficiently quantified at this time to provide policy makers with an adequate basis for their decisions.

The second area of concern, progress in understanding corrosion processes, and Gerry Gordon will talk a little later about this.

The Board's concern is with the understanding of the underlying fundamental processes involved in predicting the rate of waste package corrosion. DOE agrees it's important to develop an understanding of the underlying physical phenomena of corrosion processes.

We convened a peer review in May of this year on the corrosion processes. As I think Joe Payer will talk a little later, the interim report is due in September, with the final peer review report due in April of 02.

We benefitted from the Board's international workshop on long-term extrapolation of passive behavior, which you held in July of this year. We believe that the bounds on waste package corrosion that will be used to support the site recommendation decision are adequate and a confirmatory testing program is now in place.

The third area of concern, lower-temperature design comparison. The Board's concern is that performance projections are very uncertain, due primarily to the high temperature repository design and uncertainty with processes operative at these higher temperatures.

DOE is addressing this concern by evolving the higher-temperature base case design into a design that could be operated over a range of thermal objectives. And Jim Blink will talk about this a little bit later.

Assessments of repository performance across a range of thermal environments are documented in the Supplemental Science and Performance Assessment Report, and described in the Yucca Mountain Science and Engineering Report.

DOE believes that the performance projections are adequate for the range of operating environments considered in the site recommendation basis documents.

The fourth and last major area of concern of the Board, multiple lines of evidence. The Board views DOE's safety case as overly dependent on performance assessment, and strongly endorses efforts to develop multiple lines of evidence.

Multiple lines of evidence have been integral to the development of process and performance models, but this may not have been effectively communicated.

In April of 01, Board members and staff met with the Department to discuss the meaning and applicability of these multiple lines of evidence.

We believe that discussion and evaluation of multiple lines of evidence have been more clearly explained and is being more successfully incorporated into documents and plans.

In conclusion, I've got three points to make here. DOE has initiated, in accordance with the requirements of the Nuclear Waste Policy Act, and is proceeding with a process for a Secretarial decision on whether or not to recommend approval of the Yucca Mountain Site.

DOE's path forward depends on the results of the site recommendation decision process. However, the Department is prepared to respond appropriately.

DOE understands and has benefitted from the Board's concerns and issues. These concerns have been addressed, and DOE believes that there is an adequate technical basis for a site recommendation decision.

COHON: Thank you, Russ. That was very useful framing of where you are and what's to come in this meeting.

Questions from the Board? Dan Bullen?

BULLEN: Bullen, Board.

Russ, could you go to your Slide 7, please? This shows the timeline for public opportunities for comments. But I guess just looking at this timeline, could you also kind of show us where you expect to see a comment by the NRC with respect to the sufficiency requirements for the site recommendation? Where along this line do you expect that?

DYER: If I remember right, I think early November is what is expected from the Nuclear Regulatory Commission.

BARRETT: Lake Barrett, DOE. At the management meeting last week with the NRC, the staff said that they were proceeding along the lines of our request, which was for November 1st for NRC's sufficiency, and they said they were proceeding according to schedule.

BULLEN: Thank you. Just another followup basically, you alluded to the meetings in May and June that we've had with respect to the evolution of the process, and I guess the question that I have with respect to the license--or excuse me--the siting recommendation determination is exactly what design do you expect to put forth with respect to thermal operating modes? I mean, I know the range is there, but it seemed to me that the NRC was very concerned about having one design to evaluate. And have you resolved that issue with them, or not?

DYER: The license application design needs to be developed, and it needs to be developed through a discipline process.

BULLEN: Okay. But then I guess does that tie into the sufficiency requirement from the NRC with respect to do you have to identify a design for SR, or are you just going to go ahead with the flexible design and you think that the NRC will buy off on that?

DYER: We have a flexible design for SR that the NRC seems to find acceptable for that decision.

BULLEN: Okay, thank you.

COHON: Russ, a question while this slide is still up. Is there a public estimate as to when the Secretarial decision may come?

DYER: Our goal has been to support that by the end of this year. There have been some, whenever the public comment period was extended, that's still the goal, is to do it as soon as possible.

COHON: So, effectively, I don't mean to put words in your mouth, but I understand that to mean that the Program will have done what it thinks it needs to do to support that decision by the end of the year. And then when the Secretary actually makes that decision is up to him.

DYER: That's correct. His actions are not on my schedule.

COHON: Russ, you talked about the pre-licensing activities. Is there an estimate yet about how much time it would take to do all of that? And related to that, is there any time limit on that coming out of the law with regard to when DOE must apply for a license after approval by the President and Congress?

DYER: Two responses there. First off, let me, in the law and the Nuclear Waste Policy Act, there's a linkage of 90 days from the time of site designation, not from the site recommendation. Our planning process now, we're in the final phases of our planning process. Of course, as you heard there is some uncertainty with that.

When Bechtel SAIC came on board, they have looked at essentially re-baselining the project out through license application, and we're negotiating right now to see what their recommendation is as to what it takes to put together a comprehensive and docketable license application.

COHON: Okay. Could we go to Slide 19, please? I want to take semantic exception with that first bullet point. There's some spin control that I think is important. I don't think the Board has ever stated that the uncertainty related to Yucca Mountain's performance is due primarily to the high temperature repository design. But, rather, we suggested that it's worth looking at whether a cooler design would reduce uncertainty related to a high temperature design.

The implication of that is there's still going to be high uncertainty, even with a low temperature design. Do you see my point?

DYER: Point taken.

COHON: Okay. Any others? Jeff Wong, and then Debra.

WONG: Russ, can we go to Slide 16? This is Jeff Wong, Board.

You, in that last bullet, you state that you believe that the uncertainties are sufficiently quantified for policy makers. How do you feel that this is so? Does this mean that your current QA problems are--they don't contribute much to uncertainty? Or all the KTIs are easily fixed or addressed and they don't contribute to uncertainty? I mean, before we would make--there's a stack of material that you have to read, and now we talk about stacks of CDs to read. It's very complex, a large amount of uncertainties embedded in that. So, again, what's the justification for making the statement that it's quantified for a policy maker to understand?

DYER: I guess I would split those into two things. First off, in the quality arena, we've gone back and done impact analysis on many of the analyses and reports that we've done. And, so far, we haven't found any that of the discrepancies that have been identified that impact the major results or conclusions from those analyses.

As you're well aware, the approach that we had as of a year ago was to look at bounding analysis. And by looking at more realistic estimates in the quantification of uncertainty approach, I think we have a better understanding of what the probable behavior of a repository system might be.

COHON: Of course, we'll be hearing much more about this from Bill Boyle?

DYER: Yes, Bill will give you an update on that. You look unsatisfied, sir.

WONG: Well, it's the issue that, you know, policy decision making in the face of uncertainty is always difficult, and I'm just curious as to how you prepared your policy makers to understand the uncertainty embedded in this particular analysis. You had given them training, had discussions with them? Do they actually recognize uncertainty? How do you think this is going to be played out?

DYER: I'm not sure. Nobody from Congress has asked for a seminar on uncertainty so far.

WONG: Thank you, Russ.

COHON: Debra, did you have a question

KNOPMAN: Knopman, Board.

Jeff took the words right out of my mouth. I was going to ask virtually the same question. And I was as little bit taken aback by the certainty with which you talked about uncertainty, that unless you have had a go around with your policy makers, both in the administration and Congress as well, I would say that's an open pending as opposed to a closed pending kind of judgment to make about whether you're there yet, and you may want to back off a little bit.

I just also wanted to highlight on Slide 18 a similar concern about maybe being a little bit more definitive than perhaps you're in a position to make at this time when there's a lot of peer review activity going on, some comments already in on some of these various things. And, again, I was surprised to hear this pronouncement. It sounds like you're done.

DYER: No. But in conversations with Dr. Payer, there are things that can be brought into the program that can augment and supplement that need to be done.

COHON: Any other questions? Don Runnells, and then Carl, and then we're going to close it at that point.

RUNNELLS: Runnells, Board.

Russ, this question is a bit tangential, but I have to ask it of you and Lake. I'm concerned about the people in the program, the enormous pressure that's obviously on everyone to get so many documents out in such a short time. The work load must be tremendous.

In addition, there are concerns about the budget. Lake used the word layoff a couple of times, which always scares people. Have you suffered, are you suffering attrition? Are people leaving? Or is the staff essentially still intact? Have you seen a loss of morale, such that people decide that this is just too hard, or not? And I introduced that by saying it was tangential, but it's of concern to me.

DYER: It's a concern to us also. There has been an enormous amount of pressure. It has had an impact on morale.

I am not aware of any great flux of people away from the project. It's a challenge of leadership to keep people energized and active in these times of turmoil. These are very dedicated people.

COHON: Carl?

DIBELLA: This is Carl DiBella, Board Staff, and I'm glad you've got Slide 18 up, because my question is also on that second bullet that Dr. Knopman referred to a moment ago. That's an enormously powerful statement that you have, and I want to investigate it a little bit.

What do you mean by bounds? You could mean bounds on the corrosion rates. You could mean bounds on the environment that the waste package is going to experience over the years. Or you could mean bounds on the possible mechanisms that might be occurring over time. And what do you mean by adequate? Because certainly in none of those areas do you know everything.

And now I'll let you off the hook. Are the subsequent speakers going to address this specific issue?

DYER: Of course. I see Gerry scribbling very rapidly here. Bounds can apply to any of the things that you were talking about.

COHON: Maybe Gerry will also address what you mean by "the."

DYER: You said Gerry.

COHON: I'm kidding. Go ahead, Car.

DYER: I'm sorry. You said Gerry, but you also mentioned Professor Payer and his report as belonging with that bullet somehow. So, I'm very interested in hearing what independent, and I think we all are, independent views would be on that particular issue.

DYER: Right. We'll hear from Dr. Payer shortly, I'm not sure whether this afternoon or tomorrow.

COHON: Thank you very much, Russ.

For the next phase of the meeting, Board Member Paul Craig will chair. Paul?

CRAIG: Okay. So, the emphasis is going to be on updates on what's been going on in the program. We're going to have presentation, including updates on uncertainty analysis, materials, that work done both by the Project and by the State of Nevada, three reports on peer reviews, waste package materials, biosphere model and TSPA, a report on the Board's workshop on passive behavior, and a comparison of higher and lower temperature operating modes for the repository. So, that's a full agenda.

My assignment as Chair is to keep the speakers and the discussion on track. To this end, let's see, Bill Boyle--where's Bill--your introduction isn't finished yet, so don't start talking.

I have a little noise maker here, and when you've got about five minutes left, this thing is going to start beeping until such time as I get your attention, and then you can continue for a while. And, with any luck, this won't be embarrassing. We have stronger approaches if that doesn't work. That remark isn't just for you. You just happen to be the person who's standing there.

It's kind of fun when I--I was looking at the biographies, and I realized, gee, my state is doing really well here. Russ Dyer has a Ph.D. from Stanford University, and Bill Boyle from my own campus, but my own university, did everything at Berkeley, Bachelor's, Master's and Ph.D. And with both Stanford and Berkeley at the top of the program, how can you go wrong? There's somebody from other institution sitting here. He's sensitive.

BOYLE: With that, I'll start. Thank you for the introduction. I don't know how Stanford did this weekend, but Cal lost in football, so I'm glad this isn't football.

Thank you for this opportunity. I have 27 slides and 30 minutes. I'll be fast. So I hope to leave time for questions, and I actually brought my own clock to keep myself on time. So, this will be an update on uncertainty analyses, which Dr. Dyer had mentioned I've presented on before, and I'll briefly go over what was the original concern, what we said we would do to investigate it, what we did, and what we will do.

This first slide shows at the top Abe Van Luik presented, I think it was in this building, but at any rate, a meeting here in January of 2000, and the Project and the Program's approach to uncertainty, to manage, communicate, assess and analyze them. So, treatment of uncertainties has always been of interest and concern to the Project.

So as not to misstate in summary fashion the Board's concerns, I have resorted to quoting them. And this is from--Lake, to answer your question, it was May of 2000 that we sent the material to the NRC, because this March 20th letter of 2000 was the Board's comments on 963. And the first paragraph briefly states the general concern, but the longer paragraph below is a very cogent, succinct description of why people should be concerned about this issue in the first place for policy makers. And you're free to read it on your own, and if you ever want to get the whole letter, NWTRB keeps its correspondence on their website.

And as recently as this year, the Board, in identifying their four priority items, at the top, they had the meaningful quantification of conservatisms and uncertainties in our performance assessments. So, it's still a priority item to the Board and the Department.

Okay, this is the first slide that deals with what we said we'd do. For those of you that don't know, the acronym PORB stands for Project Operations Review Board, and it's a group of senior managers in Las Vegas in the Department that get together and come to agreement on scope, schedule, cost. And if you really want to get work done, it needs to be approved by the PORB, which you can see that there were a number of PORB activities related to looking into uncertainties, specifically unquantified uncertainties, and the first action was back in May of last year, and that was largely to initiate a review, and as Russ Dyer had shown on one of his slides, it was in late 2000, it was this action that we also decided to look into quantifying the uncertainties.

And to summarize what we were going to do in all these tasks was to review what we had done, to do some work on quantifying uncertainties, which was largely eventually accomplished in the Supplemental Science and Performance Analyses. But in the end, all the PORB papers always referred to providing guidance for what to do in the future.

These next two slides, Slides 7 and 8, provide a history of the various presentations and activities that have taken place. As I've already mentioned, Abe made a presentation in January of last year. There were the PORB actions of May and October. Abe also made a presentation up at Carson City at a Board meeting.

We provided a copy of a report we had done for Undersecretary Moniz. We provided that to the Board, and the

Board reviewed that. At the January meeting in Amargosa Valley, there was a presentation on work done to date.

And then came the SSPA. Up until this point, the uncertainties activity had been pretty much an item all by itself. But when we started the SSPA, Supplemental Science and Performance Analyses task, the uncertainties work became joined with it, and the SSPA is a much larger body of work than just looking at uncertainties.

As part of the original uncertainties work, I mentioned we were reviewing what we had done, and in May of this year, our management and technical support contractor, MTS, provided a report on how we had treated uncertainties in the TSPA-SR.

In May, I made a presentation at the Board meeting in Virginia. In June, here at the Panel meeting, the two Panel meeting that was held here in Las Vegas, Kevin Coppersmith made a presentation. In July, we released the Supplemental Science and Performance Analyses, and this is the most up to date work on quantification of uncertainties and conservatisms that we have.

And the last item deals with what we will do. As I have mentioned, in the original PORB actions, there was always an item to provide guidance on what we would do in the future, and this report will contain that guidance. And I'll say more about this report later.

I should have done this to start with. The work I present here always represents the work of many, many others. In the original uncertainties work, it represents a lot of good work by Kevin Coppersmith and Karen Jenny and Ralph Rogers. And then the SSPA, it's such a large body of work that it represents many, many people, many of whom are in this room, but it could have never been produced without people like Jerry McNeish and Rob Howard is here, a tremendous accomplishment.

It's in two volumes, and Volume 1 itself is actually in two parts, if you have a hard copy of it. And I usually start with Volume 2. It's Total System Performance Assessment analyses of the higher temperature operating mode, HTOM, a lower temperature operating mode. That allows us to--well, those TSPAs are based upon new information, either new information related to quantifying uncertainties, or just update in scientific information, or information related to the thermal operating mode itself.

We took all that information from Volume 1, plugged it in, created two new TSPAs. That allows us to compare those new TSPAs with the TSPA-SR to see if we really were conservative, as we had claimed, and how conservative were we, and were there any changes in uncertainty as we had added these new models.

For those of you that like looking at the results first, here are three what are called horsetail diagrams. The upper left is the TSPA-SR published last December, higher temperature operating mode results, lower temperature operating mode results, and it's easy to see any number of things which are described in words on some of the following slides. But in general, the higher temperature operating mode, lower temperature operating mode as determined by the SSPA are very similar to each other, and yet both are markedly different from the TSPA-SR. And, in general, it shows that the TSPA-SR is conservative relative to these calculations.

And if you look out here at a million years, the far right on each of the plots, you can see that the spread in uncertainty is much larger for the SSPA calculations than it is for the TSPA-SR calculation. Be careful with the Y axes. They're the same in these two plots, but different from that one.

So, how did we get to those calculations in HTOM and LTOM? As I've mentioned, we added new data. We put in the models. We removed some bounds or conservatisms with new data or models. And this is an example I've presented before, but this is actually how it got captured in the final SSPA, and this is the representation of Neptunium solubility. I think I presented it at the January meeting. Neptunium solubility on the Y axis is a function of pH on the X axis, and this curve is what we had used in TSPA-SR, such that at any given pH, solubility was known with certainty. It was a constant value. And for the SSPA, we represented it instead with a distribution of values at any given pH.

So, what effect does such a change have? And that's shown in this figure. This is one of the sensitivity analyses from SSPA, Volume 2 that incorporated that change in Neptunium solubility, as well as changes in Plutonium solubility, Thorium solubility, Uranium and Technetium solubility. But the one that predominantly drives the results is the change in the Neptunium solubility, and you can see that out at the far right, it's as much as an order of magnitude difference. But what isn't shown here is how the horsetail diagram changed. We're showing the effect on conservatism in this plot, but not the effect on uncertainty.

These next few slides cover uncertainty, and the information is the same as what's contained in those horsetail diagrams. It's just presented in a different way. As a matter of fact, these two slides represent a vertical slice through the results. It's at the time of peak dose, and it's plotting the results as a cumulative distribution function here and as a histogram down here. And these two plots show the same information, just presented differently. People like them in different ways.

But the main points are that the TSPA-SR is conservative. It's shown that it's to the right of the HTOM and LTOM in red and blue, so, higher doses. And it's also less uncertain, which is shown by the steepness of this curve, and the fact that the SSPA results are more uncertain, as shown by the flatter slopes and covers more orders of magnitude.

The same thing in the histogram down below. The black bars from the TSPA-SR are more to the right and narrower to each other, more closely clumped than are the red and the blue of the SSPA, which are spread out more, and are shifted to the left, lower dose.

Here was another way to slice through the SSPA results, and this was a horizontal slice through the horsetails, if you will, plotting the time it takes to reach a particular dose in these plots, the time it takes to reach 0.1 millirem per year. And the main point to get here is also that the TSPA-SR results are conservative, as was claimed, relative to the SSPA results. They're shifted to the left on this slide, that is, they show up quicker, the dose, and also that they were less uncertain for TSPA-SR or more uncertain for SSPA, in that the spread of the results, the flatter slope in the cumulative distribution plot in the upper left.

And, again, the other two plots are just histograms of the same information, if people would rather look at bar charts rather than cumulative distribution functions.

The next slides, three slides, cover in written words what I have just been saying about the horsetails and the two slices. In general, one thing that we got out of the SSPA is that the LTOM and HTOM, the low temperature and high temperature operating modes, were very similar to each other, much more similar to each other than either one was to the TSPA-SR. And we got the broader range in uncertainty by adding in additional uncertainties and models, the additional uncertainties by new models with new datasets, or old models with new datasets.

And if you compare, it's the slide with the horsetail diagrams, which is on Page 10, if you spend your time and look at it, you'll see that after the first 10,000 years, the TSPA-SR, that's the base case, is conservative with respect to the SSPA results, as measured by the SSPA results, had lower dose after 10,000 years, and the explanations and the amounts are given right there, as much as three orders of magnitude conservatism, as measured differences between the means. And at some years, it was even greater than that.

But during the period prior to 10,000 years, if you examine Slide 10 closely, it will show that the TSPA-SR appears to be slightly non-conservative, that is, with respect to the SSPA results. That's because with the TSPA-

SR, there were no doses prior to 10,000 years, and yet the SSPA has some, but they are very low doses. And we'll come back to this in another slide.

Now, I was asked to talk a bit about conceptual model uncertainty, and I have two slides on it, largely to get across what it is we have done, and also that it is a difficult problem. There's the definition of conceptual model uncertainty. The conceptual models are developed by our technical principal investigators, based upon available data and information.

In general, where there are multiple viable models, one is picked. The others are documented, but one is chosen as the preferred one. And the Neptunium example, solubility example, on Pages 11 and 12 is an example of two different conceptual models, if you will, one of which is a constant at a given pH, and the other has a distribution.

Usually, the principal investigators chose one model that was the most defensible. Trying to incorporate multiple weighted models in our calculational scheme is computationally difficult. SSPA captured many different alternative representations, hopefully based upon ones that were more physically realistic. And we used them in sensitivity analyses. Again, the Neptunium solubility example on Page 12 is an example of one of those sensitivity studies. And so we can get some measurement of the difference between two different conceptual models, but in terms of quantifying the uncertainty with the two different models, it still is a challenge.

I just wanted in this slide to get across--this has to do with the work remaining to be done, and we had over the last year and a half many meetings between the Nuclear Regulatory Commission and the Department of Energy on key technical items, KTI, meetings on key technical issues. And this is just a brief summary of some of the items that have been agreed to by NRC and DOE, and you can see that many of them, if you read these closely, they deal with uncertainty and how it is we've treated uncertainty in our documents, and the work that we have to do to satisfy the NRC/DOE KTI agreement.

And April Gil, I think, talks tomorrow on the work to be done to address those agreements. Abe Van Luik will talk later today on this peer review of our TSPA-SR. And what's been done here is just some of the questions from this peer review group have been summarized. This isn't all of them, but just to give a flavor that this group also has questions about how it is we treat uncertainty based upon their reading of our documentation.

This is still in the work to be done. This is the report that I showed that would be done in November of this year by BSC. The next three pages have the draft outline for it. This would largely be a summary, Chapter 2.

This is also still, the first part of Chapter 3 is, well, what have we learned from SSPA and other studies on uncertainties, and this is the meat of it right here, in part, providing guidance on what to do in the future with respect to uncertainties, how we treat them.

And also the final chapter, Chapter 4, and it in some ways highlights something already brought up by Dr. Wong and Dr. Knopman. It has to do with communication of the uncertainties. And, so we have an entire chapter that deals with communication of uncertainties.

And my next two slides deal with that. I just want to present this to show a different way to try and communicate with decision makers, if you will. The next two slides are 25 and 26, and they present exactly the same results, but in different ways. This slide is our typical way of presenting it, which is logarithmic axes for time, and a logarithmic axis for dose rate, as measured by millirem per year.

I've put the EPA 15 millirem standard on a solid line up to 10,000 years, and then dash beyond 10,000 years. This is for combined nominal and igneous doses from the Supplemental Science and Performance Analyses document, with the TSPA-SR mean results shown in black, the HTOM in red, the LTOM in blue. The red is here the entire way, but it's plotted underneath the blue and you can't see it now. I'm going to put up this same slide on the overhead projector briefly.

Okay, what I was trying to get across, this is nothing new about logarithmic plots, but that little teeny mark right up there represents 10,000 years and 10,000 millirems per year, just that one little mark by itself, which is what's represented by all the rest of this plot.

My point here is is that logarithmic scales and log/log scales, most people don't deal with them on a daily basis. I challenge people that read the Wall Street Journal look for a logarithmic scale, and you won't find them, that most people deal with linear scales.

These are the same results, but on a linear/linear scale. You'll notice it's out to 100,000 years, and the dose rate with the EPA standard shown down here now, with the TSPA-SR in black, HTOM and LTOM down here. You don't see the red because it's being printed underneath the blue, and it's right on the zero axis. And this gets back to the question by Drs. Wong and Knopman, it's, well, how would we communicate the uncertainties to decision makers. If I use this slide, the one on the left, even if I were to put the horsetails on there, because we're showing means here and our means are biased up towards the top, the horsetail is going to plot right on the zero axis as well.

And, so, to the extent that our models are correct and they represent what we're doing out there, I think most decision makers would look at this plot on the left and say it's zero, the uncertainty about it is almost immeasurable, as shown on this plot, and they would feel comfortable with saying go ahead.

But, it begs the question of, well, what confidence do we have that our model actually characterizes the system accurately. This topic came up at an Advisory Committee for Nuclear Waste meeting a couple of weeks ago, where Chairman Hornberger asked Rob Howard and myself for the SSPA, if we continue removing the conservatisms, could we get a few more orders of magnitude performance. And, you know, we could debate how much more performance we could get, but I did make the point that if you use this chart, we're already at zero. I mean, it's hard to go any lower.

So, with respect to communicating, one item I might bring up is for decision makers in particular who don't deal with logarithms on a daily basis, we might be better off going back to linear scales.

In summary, treatment of uncertainties has been recognized by DOE as important. It's been more than a year we have focused our work on the conservatisms and non-conservatisms, if any, and TSPA-SR.

The SSPA is the most recent up to date work on this, and the Uncertainties Analysis report will probably contain no new analyses, but the important part of this report would be the guidance and how to communicate. And, as I said, this guidance part would be part of that report.

CRAIG: That's fantastic. Your five minute warning just went off. You're doing great.

Yeah, the data is so compelling now when you present it this way that it really does suggest that your remarks about the model uncertainty assume a really high level of importance, because we're now in a situation where the model uncertainty on the metals is really a very, very big deal in these things. And, of course, it's one of the things that we're going to be talking about a lot here.

Okay, let's see, let me go in sequence here, Jerry and Priscilla, Dan.

COHON: Cohon, Board.

Bill, I have several questions and comments. I'll triage and go quickly. First of all, I don't think you intended it this way, but it sounded as if you were saying that the base case, by having less spread, was somehow superior to the other cases which had more spread. You made two points, that it was more conservative, those histograms showed that, and that it had less spread. But just to get a check here, you weren't arguing that less spread was a virtue in this case?

BOYLE: Well, that gets back to the quote from the Board's letter of March 3rd of 2000. Some decision makers actually may prefer that less spread. I'm not saying that I do, but it's--

COHON: No, no, wait a minute. Your job here, of course, is not to produce some model that produces less spread and make decision makers happier. It's to produce an analysis based on the most realistic, most credible understanding you can develop of the underlying phenomena. So, solubility is a case in point.

Presumably, you use the probabilistic, the stochastic representation, because you thought it was better in a scientific sense, than the single model approach. That's what I meant.

BOYLE: Yeah, that is true. But I would like to, with respect to the Neptunium solubility example, the NRC has raised a question about, well, the data that underlie the probabilistic model, and it's this whole issue, myself personally, I prefer fully realistic, fully probabilistic, but in defense of those who prefer the bounded approach, as represented on Slide 11 with the Neptunium solubility with the constant value, it's sometimes the probabilistic models, the underlying dataset, not everyone is convinced, so people tend to say okay, I'll take a bound. And that's the dilemma we have right now, is convincing people sometimes of the underlying probabilistic models, and if we can't, then I think there's been a tendency to switch to bounds.

COHON: Okay, I'll pass on that now. Maybe others will have something to say. But at least the issue has been joined. I have two more things.

First of all, on Page 25, we can look at 25 or 26. Let's look at 26. I think it's easier to talk about for just the reasons you said. As we know, as you've acknowledged many times in these meetings, communication of this information to non-technical policy makers will be a challenge. And let me offer some suggestions. This is a better representation than the previous slide, but it still doesn't go far enough. You should, I believe, and this is one person talking, that this should be the basis then for making statements to policy makers like the Secretary that the mean, Mr. Secretary, at 10,000 years, to use your case, is zero, or essentially zero.

In addition, though, there is a probability of "X" that the dose could be greater than 15 millirems per year. And I think that that is key information, which is, of course, not captured by this. Horsetail diagrams have that implicitly, but I think it's incumbent on the Program to say that as clearly as possible to the Secretary and to the public.

In like fashion, I still have a problem--I mean, I don't object on any technical terms in the weighting that you do, the probability weighting that you do of the nominal plus igneous, but I think the Secretary should be told that there is the probability of "Y" that the dose could be "X" thousands of millirems in the event of igneous intrusion, that you need to sort of de-construct your overall probability statements and this kind of thing.

So, that's a sermon. That wasn't a question. But the virtue of what you've done, and something that I think you should be congratulated for, is developing the database for doing that. Though, again, there's still very serious problems of the sort that you've known about all along, and I think you're dealing with as effectively as you can, about what is the correct, quote unquote, or most defensible representation of that uncertainty.

My last point, and this is a question, your presentation did not cover this at all, but the Board has communicated to the DOE our concern about having looked at certain parameters or elements of TSPA with regard to their uncertainty, and the sensitivity of TSPA results to those, and then choosing not to carry those forward because the sensitivities seem to be small. Our concern was that because of such a complicated interconnected problem, that in putting aside a particular parameter phenomenon, we might miss, you might miss its contribution to a larger systems uncertainty.

Have you--you know what I'm talking about?

BOYLE: Oh, yeah.

COHON: Have you dug into this? Do you have something to say about it?

BOYLE: Well, I'll deal with that immediately. We are aware of the issue. It was communicated in a recent letter, and also the international peer review group, or the ACNW, may have commented on it as well. And I might characterize it in part as, you know, removing things because they seem locally to have no effect in and of themselves, but when combined, perhaps they do.

And if we had a fully probabilistic model that perhaps, you know, not weeding things out prematurely, perhaps we could see, you know, leave them in if it's not too computationally difficult. That's probably a large part of the reason why things are removed now. It's just they don't seem to make a difference. Leaving them in, you know, makes the problem more intractable, so if--I think it's an issue that's tough to deal with.

But, I do want to bring up some other items you brought up here. The separate igneous, we do have the results separately for igneous. I didn't ask them to be plotted this way, or didn't show them, but, I mean, we could. I mean, the data are there. It's simply a matter of, you know, if we wanted to focus on the igneous, we could.

COHON: Bill, but for the record, I think it's very important that doses be non-weighted.

BOYLE: Okay. And this gets back to--I think your reason for asking for that gets to another point you made about these where a decision maker--where we would tell the decision maker, look, it's zero or close to zero, with a probability of "X" of it actually being greater than the EPA standard.

And it's interesting how people within the project respond to that. When I asked them that question after your testimony to the Nuclear Regulatory Commission earlier this year, this same topic came up. There are some people who would look at the 300 realizations from the calculations, none of which were greater than 15 millirems, and using a frequent approach to probability, would say it's zero, zero probability, which I think that there are some people in the room that don't buy into that, myself included, because that is the correct answer, provided 300 calculations was enough. But also, more importantly, that the underlying model actually is correct. And to the extent that it isn't correct, there is some non-zero probability that is very difficult to get people to estimate that we would actually be above that line, using our model.

CRAIG: Priscilla, and then I have Dan and Alberto. Anybody else?

NELSON: Could you show Slide Number 13? I appreciate here that you talk about high temperature operating mode and low temperature operating model as opposed to designs. I still do not consider this to be a low temperature design. But on the upper plot here and on a subsequent slide, you talk about the steepness of this curve being an indication of the reduced uncertainty.

BOYLE: Right.

NELSON: And this is confusing to me, and it's part of the confusion I think that still is in my head about mix of different kinds of representations of simplifications in your model, that you've got some cases in there in the base case where perhaps a property or an input parameter has been bounded, or a model output has been bounded, and may be so conservatively, and in the process of that, the representation of the uncertainty in that is really lost to many respects.

So, if you had said that that's an indication of the conservatism, that's one thing. But to say that the steepness there represents the uncertainty in the value as opposed to the uncertainty that it is a bounding representation of mean dose, is--

BOYLE: Well, that steepness is driven by the bounding representations which in, you know, plain terms, we are throwing information away. We are throwing away uncertainty at the price of worse representation of results, you know, which is the curve is shifted to the right. And, so it's one of those things by going to bounds, the advantage to using a bound is that it eliminates some uncertainty, just throws it away. We don't use it anymore, but it comes at a price of worse performance, I think both of which are represented there. The black is shifted to the right, higher doses, but it's got a steeper curve, which it is steeper, less uncertain, because we threw some of our uncertainty away by switching to bounds.

NELSON: But I still have trouble with this, because it says annual dose at the peak of the mean. Now, to me, what that is a representation is it's--it's not uncertainty. It's confusing, and when you said that, I just got stuck on that.

BOYLE: What's really represented there for the black curve is is that we've found the time for the TSPA-SR at which the mean was at a peak. It's roughly 275,000 years. And we just took the horsetails and plotted them up as a cumulative distribution function, and we sliced the SSPA results at approximately a million years, because that's where the highest doses were.

NELSON: Well, I understand what you did then, how you got there. It was the use of the assertion about the reduction in uncertainty and what that ought to have is significant is one that stuck with me.

Okay, I want to ask you just one other thing. You have on model uncertainty, you talked about on Slide 18, I guess what I see here is two aspects of model uncertainty, and I'm wondering to what extent you're separating them. One is the uncertainty that's associated with the actual conceptual model representing the system that is being modeled.

And the second one really is the uncertainty associated with the simplification of the model for inclusion in TSPA. And they may be approached differently in terms of evaluating the impact of that uncertainty on the output. Are you separating things at all that way? I mean, because you say where there are multiple viable conceptual models, you choose one; right? Now, that means that the uncertainty associated with the choice of the conceptual model that represents the system is maybe not evaluated, except for in the engineering judgment, or the scientific judgment of the selection.

So, is most of your uncertainty evaluation for the conceptual model related to the simplification aspects for using in TSPA?

BOYLE: I don't know that I still get it, but what I'll try and use here are examples to get across what's done, and a few examples come to mind. One is how should we treat the rock deformation? Should we view the world as an elastic continuum and use finite elements as a conceptual model and the numerical tool to get at the answer, or should we use a discrete element model? The mathematics are different, the models are different.

And, so our investigators have looked at both types, and I don't think either one actually factors into TSPA. But, for design, they will end up probably choosing one, but using--knowing that the other model is available, and looking at them. But they will usually in the end select one.

Another example is from unsaturated zone flow where we could use a discrete element representation for the flow. We could use an equivalent continuum model from a continuous approach, or we could use a dual porosity model. And our PIs have investigated multiple models, but in the end, they usually pick one, and all of them involve simplifications, and all of them have uncertainties.

NELSON: Let's just pick one of those. You choose one of those models, those conceptual models, and then you generate a whole lot of analyses that tell you how that system performs. And then you have the choice of how you simplify that.

BOYLE: Yes.

NELSON: In terms of putting it into TSPA. That's another question of uncertainty, because there's more than one way of doing that.

BOYLE: Exactly. Sometimes the models aren't simplified at all. They're used as is. But in other cases, they are simplified, which just compounds the problem of, well, how much uncertainty is there in the result. So, I'll agree with that.

NELSON: Okay. Well, it seems like that one is important to quantify the uncertainty associated with that kind of a simplification for TSPA. I could see why if you're going to a picking of one when you're considering multiple viable conceptual models, that the documentation of the basis for the selection is a different way of handling that uncertainty. But, there is an uncertainty that can be quantified.

BOYLE: Sure.

NELSON: Associated with simplification once you've chosen one.

BOYLE: Yeah. And one change that Bob Andrews made after February 12th is to put--this is my understanding--those people that do performance assessment, that are making that second simplification, if you will, they don't belong to performance assessment anymore, they belong on the PI side of the fence. So that the people that are coming up with the detailed representation of the process model, and if there is a second, you know, a subsequent abstraction, that they're in the same group. So, at least it's one group working with it now, and hopefully that will lead to some consistency in its treatment, rather than the way in the past, it was the principal investigators would come up with one representation that might have been very detailed, and then it was a completely separate group that did the simplifications for performance assessment.

CRAIG: Okay, thank you, Priscilla.

Dan Bullen. We're talking about core issues for the Board, so we're going to let this session run on until we get through.

BULLEN: Bullen, Board. Just a couple of quick questions, and then I'll defer to my colleagues.

Could we go to Slide 10, please? The comment was made here that basically--well, actually, the first question I have is you mentioned that you got close to peak dose at a million years. But how do you know?

BOYLE: Right. It actually has not bent over yet, but as the various scientists have looked at it and know the processes involved, you know, they figure, well, it's somewhere beyond a million, but exactly where, they don't know. But, you know, looking at the processes involved, and also at the steepness of the slope, and depending on how you plot the results, it's are they beginning to turn over. But it does beg the question. We did not go out to, let's say, 10 million years, the next cycle over, to see where they actually--

BULLEN: Bullen, Board.

I realize that, you know, the uncertainty gets even more pronounced as you go to 10 million years, but is there a plan or an approach at least to take a look at trying to identify where the peak dose actually is? I mean, some lawmaker might like to know it's 2.3 million years, and others might not care that it's after 10,000. So, I guess I just wanted to know.

BOYLE: Right. I think we showed this at the meeting June 20th and 21st. As is clear just looking at these three figures, particularly the TSPA-SR to either of the other two, depending on what models we put in, we could make that peak move, and we can bring it back in under a million years by simply removing that temperature dependence on the general corrosion. We've done the calculation without that thrown in, and it brings the peak back in under a million years.

BULLEN: Bullen, Board.

Just a quick followup question, because you made the comment, and you don't have to go to Slide 15, but you did comment at the last bullet that the low temperature and high temperature operating modes show similar effects of incorporation of uncertainties. And I guess the question that I have is is there reason for that, the inability of the models to differentiate between the two? Or is there a masking effect, as Gerry alluded to, from some other total scope of the model? Or have you dissected it to the point where you take a look at the individual submodels and identify that there's actually the same performance for LTOM and HTOM?

BOYLE: Well, I'm glad you asked that, because it was covered back at the June meeting, and Jim Blink, Dr. Blink, will cover it again today. My bullet on 15 dealt with at the system level as measured at 20 kilometers with respect to millirems per year. They look essentially the same.

Dr. Blink will show many calculations, or they are certainly in the SSPA, where at the process level, things vary a lot, hot to cold, you know, whether it's chemical constituents in the water or whatever, but when you add it all up with our model, at 20 kilometers, you don't see much of a difference.

CRAIG: Okay. Next is Alberto Sagüés.

SAGÜÉS: Yes, I have a couple of clarification questions. And Number 11, if we can look at it, please? First, you cannot see very well over there, but do I understand correctly there is a data point right there at the pH 7, and about 10 milligrams per liter and logarithm one. I think there is a little black datapoint right there in the--right there on the old curve. Is that correct?

BOYLE: That's the way it looks to me. I'm not that intimately familiar with the dataset.

SAGÜÉS: I mention that because if that's correct, that one datapoint there, because all the--it's like all the other points combined right there.

BOYLE: Sure.

SAGÜÉS: Which brings up the question first of all, just to make sure I understand this right, the little circles are the former assumptions; is that right?

BOYLE: The little circles are what?

SAGÜÉS: Yeah, the little circles represent the input used for the former model; right?

BOYLE: Yeah, that's TSPA-SR.

SAGÜÉS: For TSPA-SR. Okay. And then the solid red curve represents the new abstraction; is that right?

BOYLE: Correct.

SAGÜÉS: And that's based on the Argonne data?

BOYLE: Yes, Argonne, as it says there in the second bullet.

SAGÜÉS: Okay. I just thought that if I understand this correctly, then, you know, one of the conclusions one could derive from this would be that the technique used to try to measure this parameter is inadequate.

BOYLE: Repeat that?

SAGÜÉS: One of the conclusions one could draw from looking at this is that the method, the technique used to obtain solubility data is just not good. What could you say to such a statement?

BOYLE: I'm not a fuel dissolution chemist, and they are the ones, I'm sure they're highly capable and qualified, and they can probably present and discuss why they did things the way they did. And, for example, as I think Professor Sagüés described it well enough, there appears to be a datapoint right there. And let's assume that it is a datapoint, and again, this shows, you know, as he had correctly mentioned, this is a long scale, so this one completely dwarfs all these other effects. This is a factor of ten to a hundred, versus small fractions down here.

Well, I don't know why the scientists did not include that, but it could be that it's an outlier, you know, something went wrong in the test, but for completeness, they plotted it anyway. But I don't know.

And as to that the method is just not good enough to measure the solubility, again, I am not a fuel solution chemist, but in general, I think it's fair to state we've probably got some of the best people in the world working on this, and they're probably doing the best that they can.

SAGÜÉS: Anyway, just we're talking about uncertainties, so I guess that sort of extreme interpretation should be sometimes addressed as well.

Okay, the second issue was in Number 26, which is, if I'm not wrong, is the linear version of this one that you have here on the right.

BOYLE: Right. It's that one right there.

SAGÜÉS: Right. You know, and I agree that, of course, when you look at it in that manner, things appear to be just totally the same for the first 50,000 years, and why worry about it, and maybe that was part of the message that you wanted, if not to convey, at least to bring up the idea. And suppose that you were to translate that curve used in some appropriate model into, say, expected incidents of some disease, like cancer, or whatever, then in that case--and, again, this is my personal question. I'm not making any Board statement here. But in that case, I think that they probably will have a very different meaning. You will have, for example, a cumulative number of cancer incidents as a function of time. And then the thing that you're going to start getting into numbers, which are not going to be negligible numbers, indeed, the whole concept that such a thing is negligible or not is a totally different issue. So, I just wanted to point this out and see what you think about this kind of an observation.

BOYLE: Sure. And the fundamental data are there, as is shown in the plot to the right, which is the same data, but plotted on a logarithmic scale. I'm not an expect in, you know, what increase in cancer fatalities or cases of cancer would be caused by 10 to the minus however many millirem per year that is, 10 to the minus 2, 10 to the minus 1, but people can do that. And I don't have that result. I plotted it this way, because I could just send a quick e-mail and ask them to do it. But, people can do the plot.

SAGÜÉS: Thank you.

CRAIG: Norm?

CHRISTENSEN: Christensen, Board.

Bill, this is maybe more by way of a comment, but I think these two slides speak to the different roles of Board and decision making, and the comment really harks back to the comment that was made by Jeff Wong and Debra Knopman a bit ago.

Now, I think the Board sees its role as being able--as a technical proxy, in a sense, for decision makers, of being able to assure the technical validity of what is being done, and in that case, the Slide Number 25 becomes very interesting and important to us, because it reveals things, and the issue of quantification becomes really critical.

I think the issue, though, that comes up is another kind of uncertainty that you've mentioned, and it really is a conceptual uncertainty, and it's not so much whether the horsetail diagrams get absorbed in those lines in Number 26, but whether we feel more confident about the conceptual models that underlie the two different curves.

In other words, I think being able to articulate that the decision to go with an analysis that produces the blue and red line is based on increased confidence, and being able to articulate that. That's not always a quantitative issue. It really has a lot to do with how we feel about the theoretical underpinnings for the various parts of the TSPA.

So, I would argue that, in fact, from a decision maker's standpoint, the question that might come to me is why are there two curves, and what was the--why are we more confident about one approach versus the other?

BOYLE: Well, thank you for the comments. And I agree that even though I have now presented the results on the left in linear/linear space, I'm not advocating not doing these plots, because these little differences here, the scientists know--it's important to them to know why they occur. And so we'll probably always do it this way, but it's not clear to me that we would want to use these plots for the decision maker.

And the point about being non-quantitative, in using that to help convince somebody that we're more confident, that point is well taken, too, in particular with respect to conceptual model uncertainty, because I think it's very difficult to get unambiguous quantitative measure of it. And we might in the end base a large part of our convincing of people upon, you know, arguments rather than quantification.

CRAIG: Leon Reiter, please?

REITER: Bill, two short questions, one on communication. I'm just wondering why in that second plot on your right, you chose to exclude the four 1,000 years when you didn't use those peaks?

BOYLE: I just asked for the results, and this is the way they came. I mean, we do have the results. There wasn't any--

REITER: But that's an important consideration in conveying to somebody how you choose what to show and not to show.

BOYLE: Sure. Right. Well, we could have gone down to 100, 10, 1; right.

REITER: Well, you just left out the peak, left out the peak dose before 10,000 years.

Second question. In terms of conceptual uncertainty, you know, there are elements of the Program which have made a great effort to include different kinds of uncertainty, conceptual uncertainty. I'm particularly referring to the PVHA and the PSHA. Just give us an idea why you didn't use some of those techniques to try and deal with the conceptual uncertainty and model uncertainty.

BOYLE: I mean, we could. This uncertainties work has been going on for, you know, over a year now, as I've shown, and it started small and grew. But doing it the way it was done for Probabilistic Volcanic Hazard Assessment and Probabilistic Seismic Hazard Assessment is an approach, that they do take time and money, and in the end, perhaps they are debatable as well, not with respect to the process, but the results.

And that, in part, going to the bounds, as was done in the TSPA-SR was a method to just say, look, at the price of throwing away some information, some of the uncertainty, we're just going to go with the bound to make the problem simpler. And, in doing so, it came at a price of perhaps muddying the waters with respect to what we knew. But that was the approach that was taken.

CRAIG: Dave Diodato?

DIODATO: Diodato, Staff.

Bill, you've spoken about the pros and cons of a bounding value approach versus a statistical distribution. But here in SSPA, many of the parameters have more statistical distribution representation versus a bounding value representation. So, the statistical distributions have more variance obviously, they have variance. The bounding values don't. One might imagine that using that approach to try to achieve stability in your PA analyses, you might have to have more realizations with a parameter that has variance versus one that doesn't.

Now, what I'm wondering is is there a technical basis for choosing 300 realizations as the number? For example, did you have a target of stability in the first and second moments of the dose calculations to choose that 300 realization number?

BOYLE: I would defer that to Bob Andrews or Jerry McNeish, or somebody like that. But I am aware of the issue. The IAEA/NEA Group brought this up in terms of their written questions submitted, and it is quite--you know, they even did an example calculation for how many runs you would have to do for a given, you know, probability of some item occurring. And as to what we did to address those questions, like do we really--are the 300 calculations enough, you know, if all the curves, all those bell shaped curves, or whatever other distributions of data we have, some of which vary over orders of magnitude, is 300 enough? I'll defer to the TSPA experts. Rob Howard is here. He could always call back and get the answer from one of his colleagues, and you could bring the question up again when he talks about the SSPA.

CRAIG: Bill, I think you clearly got the interest of the Board with your presentation. My own last comment here, I must say I find myself very uncomfortable with this representation here, even though I understand the technical reason, one having to do with the low probability but very high risk, which is omitted from this, as you correctly pointed out, and the other has to do with the massive model uncertainty that we have on certain areas. And this kind of graph just doesn't let you understand that kind of consideration, and it seems to me that this is a prescription for possible trouble.

So, it's well worth worrying about, and well worth putting in all the time that we did this morning. Jerry has given us a dispensation to do all of that, and correctly so.

We're now going to take a 15 minute break--a 10 minute break. A 10 minute break. Please come back at 10:35.

(Whereupon, a brief recess was taken.)

CRAIG: Our next speaker is Gerry Gordon, and our Chairman isn't here, I can't legitimately make a remark about how the center of gravity is moving eastward, because Gerry Gordon got his education at Ohio State. We're getting closer to the famous place in Pittsburgh.

GORDON: Good morning. I'll start. I'm a little behind, so I'll try to speed up a little bit. These are the topics that I hope to cover in the next 45 minutes, or so. There are five areas. I could have covered a lot more territory in the way of materials update, but time limitations, I'm going to just focus on these five areas.

We've talked with the Board at the last couple meetings about the margin for localized corrosion, very important consideration, inasmuch as the localized corrosion rates can be very high. This corrosion potential versus the critical potential, that's the margin. The larger that difference, the more margin.

Consequently, it's important to evaluate the corrosion potential and the critical potential as a function of exposure time. And the longest term samples we have to do that are the samples from the Long Term Corrosion Test Facility at Lawrence Livermore, and the samples we looked at had four years of exposure. The five year samples are due to come out this February for descaled weight loss measurements.

There are also samples in separate tanks up to about a year and a half in very concentrated J-13 type water.

You've seen these compositions of the test solutions. I don't intend to dwell on them. The yellow ones are the Long Term Corrosion Test Facility solutions, and the basic saturated water is the approximately year and a half exposure of very concentrated, approximately 50,000X J-13. And these solutions have the range of chloride up to very high values, fluoride up to about 1600 ppm, and they also have the anions, nitrates, sulfate, carbonate in different ratios.

This is a rack out of the Long Term Corrosion Test Facility. I think you've seen this picture before. The water line tends to be about at the middle of this rack. And to obtain samples with long term exposure, we selected the U-bend samples, because they're easy to get out of the racks. And, in fact, we selected the samples below the water line. In removing the samples, we also removed the solution, the hot solution, to keep the samples from drying out, and we transferred the samples with their solutions to special electrochemical cells for measurement.

The next slide is a sketch of one of these cells. In the cells, we included the four year, approximate four year exposed U-bends. Each of these cells had a different long term test facility environment and temperature. There's also an archive, fresh U-bend, if you will, of the same heated material. There's a platinum electrode, and of course there's reference electrodes. And we're able to do cyclic polarization on the new samples and the old samples, as well as monitor the corrosion potential as a function of time from the time they went into this vessel.

This is a summary of what I'm going to present in terms of data. We found a relatively large increase in the corrosion potential in one of the three environments, the simulated acidic water. Potentials in the other environments, the dilute water, the concentrated water, increased slightly from the initial potentials. And what was initially somewhat surprising in the four year exposed solution, platinum electrodes, the potentially open circuit potential was very similar to the corrosion potentials of the Alloy 22 in this four year old acidic water solution.

That indicates that it isn't just the sample that's changed, but also the solution. It's become more oxidizing. I'll explain this as I go into the next few slides.

These are cyclic polarization curves. The two on the right are for new samples. The bottom one, new sample in a newly mixed solution. The next one up is a new sample in four year old solution. And then the upper two are four year old samples, one in new solution, and one in four year old solution.

What we find if we look at the new samples in new solution as we scan the potential, measure the current, it reproduces very well the cyclic polarization behavior described on the waste package degradation PMR. So, our system is working well.

The new sample in the old solution, the passive portion of the curve is very similar to the new sample in the new solution. The corrosion potential is starting to go up, and in fact with time, it continues to go up.

If we look at the old samples, both in the old solution and the new solution, the corrosion potentials are high. Remember, this is in the simulated acidic water environment. There are about 350 millivolts, and also the passive current density, which is a measure, in a sense, of the corrosion rate, has shifted to roughly two orders of magnitude lower rate.

Also note that we scanned these samples up to about 1000 millivolts, or slightly higher, these old samples, and after the tests, they were examined with stereo microscopy at fairly high magnification, and there's no evidence of localized corrosion. And these U-bend samples do contain a crevice where the bolt restrains the legs of the U. There's a Teflon spacer that's pressed against the Alloy 22, and even in that creviced region, there's no evidence of localized attack.

I've tried to show schematically part of what's happening with these samples. This is a very schematic active/passive metal polarization curve. Again, you're scanning the potential up and you're measuring the current, and you find in this passive range, that the current is relatively constant over a broad potential range. This is the cathodic reduction reaction. In this case, oxygen reduction. And the corrosion potential is set where these two currents are equal. It sets on mixed potential, which in this case is the corrosion potential.

If you look at this passive current with time, if you hold the potential at a fixed value and monitor that current with time, you get a plot of this type where the current is proportional to the time to exponent N, which in this case is minus .76. This is for stainless steel, just to show this schematically.

So, the current decays in the log/log plot in a linear manner, down to very low values for a passive material. So, this passive current is decaying with time, and as it does, this intersection which sets the corrosion potential is also increasing.

So, part of the increase in the corrosion potential is due to this passive film. It's becoming somewhat thicker, but as we'll see, it tends to reach a constant thickness. But also, the perfection of the film increases, the defect concentration tends to decrease with time, and that decreases mobility in the effective current, which is related to the corrosion rate.

So, if we look at the potential, it's increasing with time. This is, again, stainless steel and sea water, or synthetic sea water. At some point, this film reaches either a constant thickness, or the current becomes so low that effectively the shift reaches some acintodic value. So, eventually, the corrosion potential levels off.

These are the potentials measured in the four year old Long Term Corrosion Test solutions, both the acidic water, the concentrated, roughly 1000X J-13, and the dilute water, which is approximately 10X J-13. And you can see for the concentrated waters and the dilute waters, the potentials that these samples start at in new solution, and this is out of the waste package degradation PMR, lie in this fairly narrow range from about minus 100 to minus 250 millivolts.

So, these materials started out in this range, and they've shifted up a relatively small amount. In contrast, in the simulated acidic water, which started out about pH 2.7 and ended up about pH 3, or a little over, after four years, in that case, the potential started here, and it ends up at 350 millivolts, an increase of about 500 millivolts.

In spite of that increase, we know that from the cyclic polarization scan, that the critical potentials are significantly higher. So we've still maintained margin, but we've gotten an increase in potential that's higher than I think we would have expected.

This is the basic saturated water in a separate tank not part of the Long Term Corrosion Test Facility. These are double U-bends rather than single U-bends. They're two strips of metal bent over a radiused mandril into a U, and then the vertical legs are restrained with bolts. And you can see here an increase. You start out with these roughly 13 month old, and they're continuing in this cell. We continue to monitor the potential.

When we put new samples in, they were at very low potentials initially, and after a few months, the potentials are increasing, and it's obvious they're levelling off. This is a pretty low value. And, again, with this environment, the critical potentials are much higher than roughly zero millivolts. Because we do have up to 17 months total exposure on these U-bends, we recently took several pair out of these tanks and took them apart, since they represent a very tightly creviced metal/metal contact crevice.

And the next slide is just moderately high magnification photographs of the inside of the--let's see, this is the inner U-bend, so it's the outer radius of the inner one, and the inner radius of the outer one, represent this metal/metal crevice. And these were looked at again at up to 80X stereo microscope. No evidence of any localized corrosion, no pitting, no crevice corrosion.

So, that's very encouraging, because this is a pretty concentrated high pH environment at 105 centigrade after 17 months.

This is another set of data in fairly concentrated J-13, showing again the cyclic polarization curves. The critical potentials are up at pretty high values. We do have a series of potentiostatic rather than scanning a potential, holding a potential, at a given value and monitoring the corrosion current. And when one gets above the critical potential, you start to get film breakdown, and the current takes off. So, that's probably a more conservative way to get at this corrosion potential. We are in the process of doing that.

In this case, we have Titanium Grade 7 and Alloy 22 and a platinum electrode for comparison, and we see an initial increase as we did in the previous couple slides ago in basic saturated water, a large transient increase, and then the potential levels off. This is over about 80 days for both the Titanium and the Alloy 22.

We were surprised to see the platinum change, because it's inert in this environment, but it turns out that there's a slow build-up of deposit, and the analysis indicates it's largely silica on the surface. And when we took this sample out, ultrasonically cleaned it and very quickly put it back in the solution, it went back to this centrally threshold or plateau value.

Another set of data, I don't want to dwell on this plot because there's a lot of data, this is part of a GE Corporate Research Center stress corrosion crack growth study that's been ongoing for almost 12,000 hours. As part of that, these compact tension samples are monitored in terms of electrochemical potential, and the potential is very, very stable. This is an Alloy 22 sample against the silver/silver chloride reference electrode.

At about 8670 hours, we added lead to this particular test. At 1000 ppm, lead is lead nitrate. And we saw no change in the corrosion potential. Also, we saw no effect on stress corrosion, which I don't have time to really talk about, but no effect of the corrosion potential, except on the platinum sample, the potential did shift, indicating that probably some of this lead is plating out on the platinum surface.

This is a complicated bar chart, and I only want to--these are the potentials of four year old samples, new archive samples, and platinum samples immersed in these various Long Term Corrosion Test Facility solutions. And if you'll focus on the two bars on the left for each case, they're the simulated acidic water at 90 and 60 degrees centigrade. And what you see is that for the archive, the platinum and the four year old sample, all in these four year old solutions, have this very high corrosion potential, or open circuit potential, which as I mentioned earlier, indicates the solution has become very oxidizing.

When we analyzed the solution, this is the simulated acidic water after four years, looking at metal ion concentrations. Remember, these racks have a number of nickel base alloys, not just Alloy 22, as well as some Titanium alloys. Some of the alloys corrode at a higher rate than Alloy 22. At any rate, we're getting a build-up of iron, chrome, nickel and molybdenum in this simulated acidic water, somewhere from maybe 3 to 5 ppm for iron, chrome and moly, and up to 20 ppm for nickel.

In this bottom plot, we've contrasted the simulated acidic water compositions on the left with the dilute water, which tends to have a pH of about 8, it's pretty neutral, where there's very little corrosion product, metal ion build-up. And in this higher pH, roughly pH 11, concentrated water, there is a build-up, but to a lesser, or somewhat lesser extent than the acidic water.

So, we are seeing a build-up in metal ions, and that can affect the potential, as we see in the next slide.

These are some data from the literature in 4 per cent sodium chloride, pH 1. There are open circuit potentials as a function of temperature, and this base solution has added to it different amounts of ferric and ferrous ions. The base solution is .3 molar of each of these ions, and then it's diluted by 100 to 1, 1000 to 1, and 10,000 to 1. And the potentials for each of these dilution ratios are measured as a function of temperature.

And you see with the 10,000 to 1 where you have very little dissolved iron, this is a deaerated solution and the corrosion potential of this alloy, G-3, which is very similar in composition to Alloy 22, and in fact responds similar in terms of corrosion potential, at any rate, with very low or no dissolved ferric/ferrous couple, the potentials down near the hydrogen redox potential where you might expect it, with as little as 17 ppm addition of ferrous and ferric ions, the potential jumps up about 200 millivolts, and then goes up more with increasing concentrations.

This couple sets up a redox reaction similar to the oxygen reduction reaction that I showed on that schematic. That sets a mixed potential on the surface of the sample, and depending on the amount of the concentration of the redox couple species, you can drive the potential up significantly.

I should just summarize that section that we think that the dissolved ions from the corrosion process are building up, particularly in the acidic water, and are contributing to the increase in the corrosion potential, in addition to the increased resistance of the passive film.

As I described at a previous Board meeting, the corrosion rate database that the Project is using is based on descaled weight loss after two years exposure in the Long Term Corrosion Test Facility. Because the rates are so low, they tend to lie, the median is about .01 microns per year, and the upper bound is at 10 microns per year. Because of that, the uncertainties and dimensional measurements and weight loss measurements, and so forth, and trying to make the measurements, tend to mask trends in the data like temperature dependency, or environmental dependency.

So, in our Path Forward efforts, we are looking at several different techniques to evaluate with greater resolution the corrosion rates. And one of these techniques involves potentiostatic polarization, and I'll show you some of the preliminary results in the next couple slides.

These are data, this upper curve is in simulated concentrated water. They're all deaerated to get rid of the contribution that otherwise would occur from oxygen reduction reactions. And, again, we fixed the potential in this case at about 100 millivolts over the corrosion potential, and we're monitoring the passive current as a function of time.

And when we extrapolate this plot out to two years, we find that we're getting a rate significantly higher than we would have expected based on the descaled weight loss. This is converting the current to metal loss. This 10 to the minus 8 is a tenth micro per year. So, the two year data lie down in this decade here. There's a distribution.

On the other hand, if we use water that doesn't have all of the dissolved salts the various cation and anions in it, but we use, rather, pure sodium chloride, and we use this concentration to duplicate some NRC Southwest Research Institute data to demonstrate our technique was working, and we find that in this case, we do extrapolate down into the range of two years that we might expect from descaled weight loss. And, in fact, it looks like it may be dropping off to perhaps a constant thickness high impedance film.

To explore what's causing this higher apparent current, we went to a platinum electrode in this same simulated concentrated water environment, and we found that the platinum also yields a current as a function of time that's up in the range of the actual Alloy 22.

This is occurring because of redox reactions in this very complex mix solution. We have a number of anions and cations. Some of them can under redox reactions. So, we need to correct this type of data to subtract out the redox reactions to get the contribution that would occur just due to dissolution of the metal itself, and not oxidation reduction reactions that are independent of the corrosion reaction itself.

In this case, I just put this up to show you we're starting to look at the temperature dependency. And, again, these are uncorrected data at this point. But at 25 degrees C., at two years, these data do extrapolate down to .01 to .02 microns per year. And you'd expect these reduction reactions, since they're thermally activated to be lower at the lower temperature. So, you might subtract still more off of this after we correct it, but it's down in the right ball park.

We plan to use techniques like using a pre-filmed Alloy 22 with a fairly thick film for the anodic dissolution contribution down to a very low value. And during this type of process, we'll see primarily the oxidation reduction reactions. Then we'll be able to subtract them out.

In terms of the effect of the environment on corrosion, it's very important to define and bound the potential environments. And as you know from previous presentations by Greg Gdowski, there have been a series of laboratory evaporative concentration experiments on a range of Yucca Mountain relevant waters, and I'll describe some of those results a little later.

But, in addition, there are a large number of naturally concentrated water analogs in the vicinity of Yucca Mountain in lake beds that have evaporative salt deposits, and we find that these waters, the groundwaters are carbonate dominated. Depending on the carbonate content, they tend to divide into two generic categories, the higher pH carbonate, sulfate dominated waters that tend to have a pH anywhere from 8 to 12, or even 13, depending on the partial pressure of CO2, and near neutral waters, pH of maybe 5 to 8 that are sodium, magnesium, sulfate or sodium, calcium, magnesium dominated waters, such as the pore waters.

In addition, in all cases, these waters contain nitrates, as we'll see.

You've seen this two or three times. I'm not going to dwell on it. I just put it up to show these natural analog lakes for the sodium, carbonate, sulfate, chloride kind of waters, and for these other more neutral pH carbonate free when they're evaporative waters.

An example of the high pH end is the concentrated J-13, and in that case, in our corrosion model, we assume that sodium nitrate is the most hygroscopic salt, and it deliquesces at about 50 per cent relative humidity. And, so once the humidity reaches that point, humid air corrosion is turned on. Also, the fluoride content of these waters can be on the order of 1000 ppm when they're concentrated.

In contrast to the high pH waters, these slightly acidic to neutral brines have the potential to form magnesium and calcium chlorides that are more hygroscopic than sodium nitrate. And in this case, we can deliquesce relative humidities down to maybe 15 or 20 per cent, and the boiling points can be up to maybe 160 centigrade.

This is an example of a concentrated, evaporatively concentrated pore water. It's a simulated UZ pore water concentrated to 1100 X, and you can see that it has magnesium, calcium, of course chloride and nitrate in it, and very little carbonate.

After evaporation to dryness and rewetting, these are concentration ratios, they're not parts per million. That may be confusing. So, this is 300 times this 11.8 to get to the ppm, for example. But you can see that both the rewetted waters and the 1100X concentrated waters are very similar, and they tend to be about pH, start out at a little over 7, and they end up about 5.

In addition to the waters that can form on the waste package and drip shield, we have to also consider deposits that can form either from entrained matter that comes in during ventilation, as well as drift dust. And so we're in the process of characterizing both of these categories, the drift dust and the entrained matter, in terms of their compositions, and soluble species, and so forth.

As you know, the drift dust tends to be primarily silica polymorphs or alkali feldspars. We know from the National Atmospheric Deposition Program, they've been collecting deposition of airborne dust, if you will, since 1985. There's a table in the backup slides of the annual analysis of those deposits from the Red Rock Canyon region here in Nevada. And there are waters that have magnesium, calcium, sodium, potassium, chlorine, there are particles that have those elements in them. And we'll see as we do the dissolved ion analyses from those various kinds of materials what the dissolved content is.

The issue of trace elements is an important one. And elements like lead, mercury and arsenic can have a significant effect on both localized corrosion and stress corrosion cracking. We are characterizing the natural systems with these species. There are several reports that are either in publication or now available on lead, mercury and arsenic, and we are doing evaporative concentration studies to determine the extent of these that remain in the brines after they precipitate out or absorb on precipitated particles.

We're also looking at these effects on stress corrosion cracking, and I mentioned to date, in a series of slow strain rate tests and crack growth tests, we've seen no effect of lead additions up to 6700 ppm in several different relevant concentrated environments.

In terms of localized corrosion, the molar ratio of chloride to nitrate plus sulfate is very important. We've talked about that previously in terms of providing margin against localized corrosion.

These are the molar ratios that I've calculated for the range of waters, the well water, perched water, pore waters and concentrated groundwaters and pore waters. And that range lies somewhere below about 2 1/2 molar ratio, concentrated or not.

These are some project-generated data at the University of Virginia done by Professor John Scully and his graduate students, where they have taken very tightly creviced Alloy 22 samples, and they've cyclically scanned them, cyclic polarization, measured the repassivation potential, which is the lowest potential at which crevice corrosion propagation is expected, and they developed a database as a function of this molar ratio. And these are primarily all done in 5 molar lithium chloride.

And they developed from this database through a multiple regression analysis, an equation, and I've plotted the lines for 80 and 95 centigrade versus molar ratio, and then I've put this band of molar ratios for the Yucca Mountain relevant waters, and these vertically are the corrosion potentials that the University of Virginia measures in these environments.

And you can see very graphically the increase in margin as this molar ratio goes down against localized corrosion.

In terms of characterization of the passive film, let me just show you some of the new data very quickly, since I'm running out of time.

These are data on Alloy 22 in 200 degree C. air. I briefly reviewed those at the last Board meeting. What we see is the passive film, and these are tunnelling atomic force microscopy data. We're measuring the film thicknesses as a function of time, and it levels off at a little under 34 angstroms, or starting to level off.

These are some very recent data done with x-ray photo electron spectroscopy in concentrated J-13 at 95 centigrade for both Alloy 22, Titanium Grade 7. And we also see in this case, that the passive film thickness is levelling off at about 50 to 55 angstroms, somewhat thicker than the air formed film. And, in fact, the real case will be that initially, we'll have an air formed film, and then it will ultimately potentially be contacted by water.

This is some of the first transmission electron microscopy to explore these passive films on Alloy 22, again, the exposure conditions. It's a complicated process to get at this very thin passive film, but in analyzing it, this is the matrix. We're looking through it with an electron beam after thinning it with a pretty sophisticated technique, and we find that oxygen and chromium are much higher, of course as you'd expect, in this thin oxide.

I know it's difficult to see here, but this technique now will allow us, using another technique called field emission transmission electron microscopy, which has a very fine 1 nanometer diameter electron beam, we'll be able to determine the structure and analyze these passive films. So, this is a very promising approach.

This is a similar case for Titanium. We're just getting started on that passive film.

The final subject I want to touch on briefly is microbial influenced or induced corrosion, and I've got much of the Path Forward effort in here in the backup slides, because we obviously don't have time to get into it. We know that the project is treating MIC based on linear polarization measurements, with and without Yucca Mountain microbes present. We established a maximum factor of 2, and that's being used in the corrosion models to accelerate the general corrosion by a distribution up to a factor of 2.

Can I switch two or three slides to the conclusion? So, to conclude then, for Alloy 22, and also Titanium Grade 7, based on a lot of experimental data, the resistance to localized corrosion is high in the expected range of repository relevant environments.

That's based on observations that the corrosion potential tends to plateau at values significantly below the critical potential, and also the low molar ratios of chloride to these buffer inhibitor ions mitigates or minimizes the propensity for localized corrosion.

The waste package/drip shield surface environments are boundable based both on laboratory concentration experiments, as well as the range of natural analogs in the vicinity of Yucca Mountain.

We do have a comprehensive experimental and modeling program underway to decrease remaining uncertainties in the area of passive film stability and growth, and I've reviewed some of the initial measurements with you.

Finally, although I didn't review it because of time, there is a comprehensive path forward in place, including focused effort to quantify any potential microbiological effects on corrosion.

Thank you.

CRAIG: Thank you, Gerry. Alberto Sagüés?

SAGÜÉS: Thanks for the presentation. I'm glad to see that indeed the Project was able to do open circuit potential measurements in the long exposure test facility. And I think that the results that you presented today go to show pretty much that surprises can indeed develop in these systems in the shape of pretty much of an expected open circuit potential, I would say, under those conditions. And, indeed, you are trying to find out a number of causes that may be responsible for those potentials.

I have a specific question. Are you going to try to do any measurements to see how open circuit potentials may develop when you have a very thin film of electrolyte on the surface of the package? Because that would seem to be the kind of condition that one might expect in many case; right?

GORDON: Right, where we have a deliquescent film, for example. We do intend to do that. In fact, one of the bullets in characterization of the environmental effect chart talks to that. We do intend to do that.

Also, the thin film, potentially the corrosion products could build up to a higher concentration than in the bulk solution, because of the surface to volume ratio. So, that's an important area to look at.

SAGÜÉS: When do you expect to have that kind of research in place?

GORDON: We now have a significant cadre of researches at Lawrence Livermore, as I think you know, and, so we're starting to make progress much more quickly. So, I would guess in the next three or four, five months. Alberto, is that a good guess? I'm sorry, I meant Raoul.

SAGÜÉS: Okay. You were looking at iron ions, and so on. Would you expect this to create some concern about the use of steel sets for tunnel stabilization, and so on, like maybe changing to something else if this looks like it might be a problem?

GORDON: If it turns out to be a problem, we're looking at thinks like clad carbon steel, for example, Alloy 22 clad carbon steel, if that should become a problem.

SAGÜÉS: Okay. And then I wanted to look at that Figure 30 that you have. In this case, it's a good graph because it summarizes a lot of the relevant information. The green rectangle are the tests at the University of--no, the green triangles are the expected environments; right?

GORDON: Well, the molar ratio range. The vertical are the corrosion potentials measured at the University of Virginia in their environments.

SAGÜÉS: Right.

GORDON: Which is a very concentrated sodium chloride. It's similar to the starting range of potentials in the environments described in the waste package degradation PMR, roughly from somewhere around zero to minus 250 millivolts.

SAGÜÉS: But, of course, if you now introduce within the realm of the possible, the kind of potentials that you observe in the long-term exposure times that might develop when you accumulate a lot of undesirable species in that electrolyte, then that green rectangle could become taller, for example, and it may get into what, 500 millivolts perhaps, or something of that order?

GORDON: Well, remember these are the repassivations in this potential in this concentrated environment. I would expect that the passive film heals and becomes more perfect. At these levels, at these thresholds, repassivation potentials may increase also with time.

SAGÜÉS: But the repassivation is for something--well, the film has already broken down; right? So, it would be a reconstitution of the film.

GORDON: Well, but it would reheal at a higher--presumably, it would be a more perfect film. It remains to be seen, but where we've ran the cyclic polarization on the four year old films, the passive current was significantly lower, and we did scan up to as high as 1000 millivolts. Those U-bends are creviced. Where the bolt goes through the holes, there's a Teflon very tight crevice, and we saw no post-test evidence of localized corrosion with stereo microscopy.

SAGÜÉS: Okay. I don't want to get into the details of that, but we may have to discuss that perhaps. But, at any rate, if you go up to 500 millivolts, you could already have some likelihood of this actually dissecting that 95 degrees centigrade line, isn't it?

GORDON: That's true if this line is representative of what I'll call the Yucca Mountain Project relevant environments, which isn't 5 molar lithium chloride. That's the only thing I'm saying. It's possible.

SAGÜÉS: I see. And now you have 80 degrees, 95 degrees C. We've been talking, or people have been talking recently about temperatures as high as 150 degrees, 160 even.

GORDON: Right.

SAGÜÉS: What would that do to those lines? Even with the present assumptions of open circuit potential, could we have a problem there?

GORDON: We potentially could have. Again, we haven't measured, and you get to those very high temperatures in magnesium, calcium, chloride dominated environments, and we haven't really measured the repassivation potentials in those particular environments as a function of this ratio. But, it could. It could at 160 C., it could cross over this region.

SAGÜÉS: And if that is the case, you will end up with localized corrosion, and that would be I think that even within--even allowing for all the other mitigating factors that may exist somewhere else in the repository, I think that, and that's again my personal opinion, it would be a fairly serious problem; right?

GORDON: If it were to occur.

SAGÜÉS: If it were to occur, yes.

GORDON: Right.

SAGÜÉS: And wouldn't that be then one of the areas of priority in trying to investigate what happens?

GORDON: It is an area of priority. Tests are in the Path Forward and they're getting initiated to look at these concentrated potential pore water kind of environments where you might have high magnesium, more likely a high calcium chloride, perhaps a small amount of magnesium chloride.

I showed you some initial data in pure calcium, saturated calcium chloride, that was a backup chart to the last talk, and it showed that there was a cyclic polarization plot, if you remember, I do have a copy of it, but I don't have it on a transparency, with nitrate. The cyclic polarization curve looked very similar to the typical Yucca Mountain water. Without the nitrate there, then the break-down potential dropped to maybe 150 millivolts.

SAGÜÉS: Okay. So, still, those are fairly critical issues that will need to be resolved. Because earlier this morning in the presentation of Dr. Dyer, that bullet on DOE believes that the bounds on waste package corrosion that would be used for the SR decision are adequate, and a confirmatory testing program is now in place. So, I mean, the latter part is okay, but it seems to me that we may have a few fairly important matters that need resolution quickly, I think.

GORDON: They do. As I mentioned, the initial results look promising in saturated calcium chloride, which is maybe a worst case. But, you're right, we do need to do more work.

SAGÜÉS: Okay, thank you.

CRAIG: Don Runnells, and Dan Bullen.

RUNNELLS: Runnells, Board.

I've had a hard time, Gerry, following sort of the line of logic for the water compositions. I know that J-13 water has been used a lot in it's various degrees of evaporation and acidification. Here, we have another matrix that's experimentally convenient.

You've also talked about pore water and evaporated pore water, but I don't think a lot of experimental work has been done with the pore water. I think the time has largely been invested in J-13 to this point.

GORDON: That's true. More recently, we've started to look at the pore waters.

RUNNELLS: Can you tell us what is going to be done, what is being done with the pore water? It seems like one more likely water to contact the canisters than J-13 water.

GORDON: Well, since Greg Gdowski is in the audience, maybe I should let him answer that question, since he's the principal investigator.

RUNNELLS: That's fine with me.

GORDON: I mean, I can answer it to one level. I think he can go into more depth.

GDOWSKI: Greg Gdowski of Livermore.

I can address the issue of what we're trying to do to understand the water chemistry that is being on the waste package. As Gerry mentioned, we're looking at several aspects of that. One is what's introduced during the ventilation period, what is entrained in the ventilation air, what particulate matter, what does that bring in.

We're also looking at, as Gerry mentioned, what the drift dust would be. I mean, you have rock there, you're going to have some decay of the rock. What sort of chemistry can evolve from water interacting with that dust, or deliquescing of the salts contained in that dust.

There is also a significant effort underway to look at scenarios for water seeping through the mountain, what path it takes through the mountain, what sort of ion exchange would occur as it flows through the mountain and flows down the fracture to try and understand what sort of chemistries are evolving from that also.

As Gerry mentioned, there are primarily two types of water. J-13 is a type of water that we're looking at. It's a carbonated based water. It's not necessarily that we're looking at a saturated zone water. It's a carbonated based water. We're looking at the characteristics of that, high pH, no calcium, no magnesium, a fluoride level in that type of water that occurs at high pH. Then we're looking at the near neutral waters that contain calcium and magnesium, but that puts restrictions on the water also. Calcium and magnesium in water tends to remove fluoride from the water, so that bounds our water that way also.

But, as Gerry mentioned, we're concerned about these near neutral waters, and we're trying to do our corrosion testing on them.

RUNNELLS: How far along that path are you on a scale of something, one to ten, or zero to 100 per cent, or something?

GDOWSKI: I think--well, one thing I forgot to mention, also, we're looking at minor constituents of a--concentrated in these waters. I think Gerry mentioned we have a fairly strong understanding of how J-13, or the carbonated type waters, evolve.

We have made significant progress I think in the evaporation or the evaporative evolution to the brine, the near neutral type waters. What we're lacking right now is some understanding of the contributions to the dust, the dust that may contribute to the water chemistry, and also the entrained matters, but we have significant programs underway for those in this fiscal year, and also the next fiscal year.

I'm pretty confident that sometime within the next five or six months, we can put--

RUNNELLS: Okay, good. Thank you very much.

I have one other quick question I think on Slide 17. Can you tell us the relevance of this to the repository environment? Here we're talking about different concentrations of iron, and more or less one to one ratios of ferrous to ferric iron changing the potential. Can you apply that for us to the repository environment, to the waste package?

GORDON: Right. I'm using this to illustrate the point that the corrosion product build-up in the water includes cations, such as iron, nickel, chrome and molybdenum, and perhaps other cations, and we're finding in the acidic water, a higher concentration of those metal ions, and we're finding a high electrochemical potential. And I'm trying to partially rationalize why we're finding that high potential.

One reason we think is this build-up in cations, which set redox potentials on the metal surface that can be hundreds of millivolts higher. So, we're seeing 350 millivolts in the case of a different mix of ions on Alloy 22 than here. This is just an example from the literature that shows that fairly small amounts of these ions can have a pretty powerful effect on raising the potential.

RUNNELLS: Now, I think Alberto Sagüés mentioned this. What does this tell us about the steel support system in the drifts? Is this saying something to us about whether or not steel should be used? For example, is that going to be a bad thing in terms of corrosion of the waste package, of the canisters?

GORDON: Well, remember the steel, if it degrades and falls, will fall onto the drip shield rather than the Alloy 22. If it were to fall on the Alloy 22, and if we had a low enough pH that we could form ferric chloride, then it potentially could lead to localized corrosion areas where that happened. With the drip shield there, that's very unlikely to happen. Also, you'd have to get to a fairly low pH to stabilize this ferric/ferrous couple, and that's unlikely to happen.

RUNNELLS: Okay, thank you.

CRAIG: Dan Bullen for one fast question.

BULLEN: Bullen, Board.

Could we go to Figure 4, please? And this is just a quick question maybe to help me do my homework, because I haven't completely read the 2000 page SSPA yet. But, I guess the thing that I'd like to ask, and maybe it's not Gerry that

I should ask, maybe it's Rob or somebody that's doing the PA, but how do you take the data that are generated by the corrosion in these kinds of waters, and then step it up into the Total System Performance Assessment, and identify, you know, what fraction of the packages would you expect to see with certain chemistries of water on the surface, and then carry out the calculations?

I mean, I know that you come up with essentially corrosion rates based on the suite of experiments that you're doing, and that gets plugged in. But is there any attempt to essentially differentiate between those and say, okay, if a certain fraction of the packages are basically in simulated dilute water with 67 ppm of chloride ion in it, and we let it go at that?

GORDON: If you remember from the descaled weight loss samples removed from these various environments in the tanks, we saw no effect of the environment or the limited range of temperatures, 60 and 90 centigrade. So, it's difficult to differentiate. That's the reason we're going to these potentiostatic tests and linear polarization and AC impedance, and other techniques, to get a better handle on whether there are subtle differences in the corrosion rate, or within the range that we've measured, we don't see an environmental effect.

BULLEN: Bullen, Board. I guess just a followup question then. So, essentially as you lay out the entire repository, you don't see any differences in water chemistry on the performance of the waste package?

GORDON: That's not taken into account--maybe I'll let Rob talk to that.

BULLEN: I guess it's just sort of counter-intuitive, because I would think if you're dropping acidic water on the surface of a material, it would perform a little bit differently than if you had neutral water.

GORDON: Well, this is acidic water, the SAW.

BULLEN: Right.

GORDON: And its corrosion rate is essentially the same as the simulated concentrated water, which is a basic water.

BULLEN: So maybe I can infer then that the C-22 is pretty good stuff?

GORDON: It's pretty good stuff over a broad range of pH.

BULLEN: Okay.

CRAIG: I'm going to have to break in here at this point. Gerry, thank you very, very much. This concludes the morning session. It's now time for public comments.

COHON: Yes, we'll turn now to the public comment period, just one question from the public on this last presentation, not to be responded to, but it sort of is a thought exercise. What's the chemical composition of water in a glacier? This is motivated by the fact that this stuff is going to be around for a long time, subjected during times of climate change, so there's something to think about.

Four people have signed up to make public comment at this time. Let me remind you that there are other opportunities to make comments, including this afternoon.

It's estimated to begin this evening at 5:15. We indicate on our agenda it will end at 5:45, but we're flexible on that, and I won't call it completely open ended, but a lot more flexible than just 30 minutes, if necessary.

So, with that, as I call your name, if you think you can wait until later today, or tomorrow, it would be appreciated, just so that we can stay close to our schedule.

Dr. Paz? And I'm going to limit everybody to five minutes, please. Would you identify yourself again for the record?

PAZ: My name is Dr. Jacob Paz. I'm self-employed by Metal Service, Incorporated. I used to work at the Yucca Mountain Project.

My comment is--actually, two comments. Number one, we forget with all the presentations, one of the issues we're facing is corrosion of metal and metal toxicity and carcinogenicity. And before you make the final recommendation to the Secretary of DOE, the issue of--is very unsettled. I'd just like to put into the record several publications.

Number 1 is the--carcinogenicity, and Number 2 is--antagonism. It's publications from NYU. It's very clearly stated.

Recently, we have reported Nickel Plus 2, with UV of chromium and x-ray interact--for--

Furthermore, metals affecting certain microstats in the process of DNA replication or repair may have a similar antagonistic effect. Further study is, therefore, recommended.

Another paper from Industrial Toxicology, carcinogenicity of nickel is enhanced by the presence of other carcinogens, such as visoperine (phonetic), arsenic, hexo-chromium.

Furthermore, for 3000 relevant articles indicated, at which they have found, 1000 of them reported evidence of chemical and radiation of--

To make them state of art is, in Canada, they have carried out research on drinking water contaminated, applicability of risk measurement assessments.

Finally, on this topic, is by Yang (phonetic) and others of his group, stated that 95 toxicological testing of single methods. The issue is here, we cannot predict what is the risk. The risk is uncertain. I'm saying that additional research should be done as soon as possible.

I have an argument with--the question should be on--it's also EPA and DOE who is responsible. My position is the following. DOE was mandated by U.S. Congress to carry out characterization of the site, and they're doing the risk assessment. EPA just writes the standards. They are not involved in the performance of the risk and the risk possibility of Yucca Mountain project.

And, finally, the President has called for the best available research to be conducted, and here, there is an opportunity to do the best research to get some data before you approve, because I'm very concerned about the risk.

And, finally, a paper by Oppert (phonetic), from UCLA, and he stated that according to EPA regulation and law, you cannot have a site in a seismic active region with a hundred years flood. How are you going to put a liner--and when we have to look about injection and water treatment remediation, we have to keep in mind that Yucca Mountain potentially will go through three stages, regular site, separate site and mixed waste site. Using EPA regulation, you cannot have water as a method of treatment.

Thank you. And I will submit to the Board all the necessary information and papers. Thank you.

COHON: Dr. Paz, Karyn Severson is standing over there. Could you give her those papers and references.

PAZ: I'm giving them to Dr. Wong.

COHON: Well, Dr. Wong will accept them. He'll guard them with his life. Jeff, don't lose those. Thank you.

Sally Devlin? Sally, it would be actually, if you don't mind--oh, okay.

DEVLIN: My name is Sally Devlin. I am the public, and I am so delighted that you all came here. I don't see any of our officials to welcome you, so I will officially welcome you. And, of course, I am from Pahrump, Nye County, where this mess is intended to be stored, and I'm so delighted to see my friend Abe here, because anybody who got the OCRWM bulletin, you see I just hit him over the head with my thing. So, that was kind of fun.

The other thing is I didn't come to excoriate--and that's your toastmaster's word for the day--but I came to praise Caesar. And everybody sends me everything, and as you know, I read it, and so I brought with me one that Bill from NRC sent me about the people that are going to be on the Board to review the licensing.

Now, I had them all checked, as you well know, and there is one missing. Now, this is how I'm going to help Abe do the licensing, and that is the fifth person that goes on that Board must be someone that has business turnaround experience, another Lee Iococa, because you know my opinion. You have nothing. You can prove nothing. And the more I get into everything, there is nothing.

Therefore, that person had better not be narrow, as some of these scientists are that will be serving, not consecutively, on the Board. So, that's enough. We want to influence whoever does the licensing to be a very well rounded business person with the scientific background who understands transportation, my bugs, health issues, and a few dozen other things.

Now, I'll really get into my meat and potatoes, and I want to thank you for sending me this book. The PEES has nothing in it except one paragraph on my bugs. This is the most wonderful book I have read, and I'm going to go down a few pages, and Jerry can cut me off when it's my five minutes, but the first A-1 appendix says, and I want this on the Page 1 of the licensing, Abe, and it says, "There will not be one repository, but two." And I have been saying this for years. In Lake Barrett's summary, it says not one, but two.

We have 126,000 metric tons besides the DOD thing. Now, the other thing it says about the DOD waste, and of course we all know they're going to put it in canisters. And as you all know, the bugs came to my attention from the mess at WIPP, when the bugs ate the canisters.

We've had the testing from Livermore where the bugs ate the rocks, and we'll get more into that when we get on canisterization. But the second point that I want to make, which is in this wonderful book, which is not in the PEES, and it says that they don't know the temperature of the tunnels. Will it be 90 degrees C., or 100 degrees or Fahrenheit, or whatever it is? And this is very important because I came here with instructions from Grant that it will blow up, and that's the Nelson limits. If it's 90 degrees, it will be a little better, if it's 100 degrees. So that's his comment. And I'm still trying to get this Board to find out about the Nelson limits. So that's your job for the day.

The other thing, and I will leave you with this, is this is the only book I have ever read that clearly, and I state this clearly, that it will take 200 to 225 years to fill these two repositories. The transportation will be 38 to 50 years. Then they have to leave it open for 50 years, and maybe they'll ever have to retrieve it.

So, I don't know about you all, but I know Abe and I are going to live another 200, 225 years, because we have a lovely adversarial relationship. But what about you all? Are you going to be here? Are you thinking of the future?

And then, of course, I have to talk money, as you know, I'm very fiscal, and we get into the numbers. And that is the numbers that I have seen recently were for the one repository, 58 billion. When I started with the Congressional Report in I think it was '93, it's our ninth anniversary, by the way, in August it was our ninth anniversary, and I got into this when you were going to bring it all Pahrump. And when we have more time, I will talk about transportation canisterization and health issues. But this is the most important thing, and I think everybody should realize it, and especially the rest of the nation.

The last number I saw for the first repository was around 60 billion. That's a nice number. And for the second repository, it was around 67, 75 billion. And from the first report to the Congress when John Cantlon was head of it, it said there will be two repositories, the first costing 25 billion, the second costing 35 billion. Now, Paul and I and all these guys, we go back a long way when it said those things.

So, the numbers have doubled and tripled and will continue. And since Abe and I are going to live 200, 225 more years, I rather worry about who's going to represent you on the stewardship, and that's a word never used. So, we must talk about that. But that's just what is in your writings, and I'm giving it back to you because the public doesn't know.

Thank you, Jerry.

COHON: Thank you, Ms. Devlin. Tom McGowan? Mr. McGowan, do you want us to bring the mike to you? You can sit down.

MCGOWAN: No, I'll be over there.

COHON: Okay.

(Mr. McGowan's complete written comments are attached hereto as an appendix.)

MCGOWAN: Where it's centrally located.

And so I revised my program. I was going to do a quick eight minutes, but that's out of the question, because you all appear to be famished.

COHON: You'll have another shot.

MCGOWAN: I'll have many shots. The doctors swore up and down, he said how are you, I said, "I was hoping you could tell me."

My name is Tom McGowan. I reside here on Mars. And as I said, I have some quickie questions here, but you don't want to hear this stuff. One was for Debra Knopman. Dr. Knopman, I just want to tease you about this. Another several for Dr. Wong, Dr. Craig, Dr. Bullen, Dr. Sagüés, whose first name I understand is Arturo, Alberto or Fernando, one or the other. Alberto. Half German, okay. And you. Are you anybody? Norm? Hi, Norm. I'm Tom. And, of course, we have our Chairman here.

So, I'm going to skip over that stuff and keep it lighthearted, uncharacteristically.

My public comment today will begin with reflections upon the broader historical perspective, and we'll transition the point of reference to the fundamental crux issue, with no deference to fragile sensitivities this time.

Throughout human history, mankind had attained to successfully greater levels of scientific discovery and technological achievement, all of which commendable advances have been equally potentially either beneficial or detrimental to the best public interest of humanity and our solar orbiting planetary home.

Evidence attests to the fact that the more advanced and sophisticated we become in terms of scientific knowledge and technological achievement, the less integrity we exhibit in terms of rational, responsible and conscionable compliance with the higher moral imperative to protect and preserve human and all other species of organic life, as well as the natural environment requisite to sustain life intergenerationally and for the rest of naturally ordered human and geologic time.

Currently, we're at a meteor in terms of the absence of human integrity, reasonable responsibility, sanity and conscience, and we obtain as--self impelled and precipitous decline toward the ultimate end state of self and mutually induced non-viability, ergo toward our own induced as immature extinction as a species.

Now, some of us are already on the outgoing ship, so this is really a word to the wise, some of the younger folks here like Dr. Craig. Be aware, 2010, or whatever that is. All of you present here are aware that Darwin's theory of evolution did not suggest the species continue to evolve and then we advance toward an ultimate ideal end state of perfection as a species, but rather, species evolve laterally, that way, and until they reach the point where the limits of adaptability and the natural selection make it impossible to evolve any further, at which point they promptly become extinct and cease to exist.

Now, anyone who doesn't agree with those last four assertions are free to identify yourself in the public record, feel free to leave the room. You will not be missed. You will be noted, however, in the public record as being absolutely incorrigible when it comes to reasoning.

Now, we have to know where we were, where we came from, where we've been in order to know where we're going. And that closely implies that experience of life is a seamless continuum, and as naturally ordered, reasoning, doubt, intellect, free will and conscience, we literally can and responsibly should control the direction, nature and extent of our progress forward of the time remaining in our own best interests and the interests of our progeny, if any.

The French artist, Paul Codan (phonetic), subtitled his metaphoric painting of the South Seas Island, he was en route to the subtropical jungle, with the inquiry, "Who Are We. Why Are We Here? Where Are We Going?" The metaphysical significance of that human--question is immediately self-evident.

(Pause.)

MCGOWAN: Would you like me to summarize?

COHON: No, no, I want you to--

MCGOWAN: I can. I'll continue this for the rest of time, but I'll just summarize now because he's here, and I don't want him to get away. Dr. Abe, very quick--okay.

COHON: I'd also like you to leave that.

MCGOWAN: Who are you, by the way? Okay.

(Pause.)

MCGOWAN: Let me just sum up for five seconds, please, with your indulgence.

Dr. Abe, my friend, with all the things going on in the world of stem cell research, et cetera, how does science intend to clone the soul? And does it take one to know one?

With that, I'll disappear.

(Pause.)

COHON: Don Shettel? Please identify yourself again when you get to the mike. Thanks.

SHETTEL: Don Shettel from the State of Nevada.

In the spirit of Dr. Craig's statement this morning about truth in advertising laws, I'd just like to point out a mistake, or a misstatement in Gerry Gordon, the last speaker's presentation in his backup slides. And, hopefully, it's not something worse.

On Page 45 of the backup slides, he makes the statement that lead species are not very soluble, are only soluble in very acidic solution.

All these calculations that I'm going to show you are based on, or calculated from the Geochemist Workbench, which uses EQ/36 database. On the left-hand side, we have an EH/pH diagram which most people, scientists at least, should be familiar with, oxidation potential, vertical axis, pH down here.

Dr. Gordon was referring to lead species are only soluble in the acidic region, which I presume he means here. These are for these conditions up here.

Anglesite is a lead sulfate mineral, precipitates at somewhat more basic conditions than lead chloride. These are aqueous species that predominate. These are mineral names that indicate fields for condensed or solid lead minerals.

But in the basic side, we have a very large range here for lead carbonate aqueous complex. This is at 95 degrees. I'll move over here to 160 degrees. The solubility of lead minerals increases with temperature. As you can see, the field for the anglesite here under similar conditions is shrinking. We have an expanded field in the acidic region for lead chloride complexes, and a larger field for lead carbonate species.

So, clearly, lead species are not soluble in just acidic, or under acidic conditions. A more rigorous calculation with the Geochemist Workbench involves the speciation calculation at a fixed pH, and scanning at pH range here from 4 to 10. The anglesite precipitates in this range from a lower pH up to about pH 4 1/2. Anhydite is stable across the entire pH range here, and calcite starts precipitating at about this pH, and accounts for these level lines here.

The main point I want to make here is this might be considered the acidic region, but even with a lead mineral, or condensed phase here, we have significant solubility of lead species. And then under more basic conditions, we have an even--we can have an even higher concentration of lead carbonate complexes.

And the conclusion here is that although aqueous lead two plus may not predominate as an aqueous species, it is present and available for reaction with Alloy 22.

That's all I have to say.

COHON: Thank you, Dr. Shettel. Dr. Shettel, if it would be possible, we would appreciate getting paper copies of those slides.

SHETTEL: These are in the backup slides for Dr.Pulvirenti's talk.

COHON: Oh, okay. Those are in the backup package. Okay. And we'll be seeing them, or we'll be aware of them again later.

Thank you all for those comments. We will adjourn now until 1 o'clock. Eat fast.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

COHON: Before we start the printed agenda, at the end of the public comment period before lunch, we had a brief presentation by Don Shettel, and I neglected to give Gerry Gordon a chance to respond to that, if he cares to. And I see Gerry has just reentered the room.

Are you ready, Gerry, or do you need Greg to do this?

GORDON: I'd prefer Greg--

COHON: Well, we could try to work that in--do we expect Greg back? Okay, why don't we just look for an opportunity later today, maybe at the public comment period you could talk to Greg and have him ready and we'll get it on the record that way.

Okay, with that then, I'll turn it back to Paul Craig.

CRAIG: Okay. We're now starting the afternoon session, where we have a number of technical talks, and the first of these is the report from Joe Payer, who has been doing a review of the waste package materials, a peer review, and their interim report is out, and here is Joe.

You have 30 minutes, and I will make noises at 25.

PAYER: Thanks, Paul. Thank you.

I appreciate the opportunity to come here and present this to the Board. The plan is, as Paul said, is to give you a 30 minute overview summary of what's in the interim report of the Peer Panel on Waste Package Materials Performance. There are copies of this report now out on the table out in front here, if people haven't gotten it. And my understanding is that it's also posted on the Yucca Mountain website.

I'm Joe Payer. I'm Chairman of the Peer Panel on Waste Package Materials Performance. And this is a summary of our interim report dated September 4th.

The Panel was put together by DOE's request to Bechtel SAIC to conduct a consensus peer panel in this area. The important aspects of what our job is, our charge is to review the current technical basis for the prediction of long-term performance of materials, waste package, both the Alloy 22 waste package, and the Titanium drip shield. And, in addition, to assess the planned experimental and modeling program that supports that long-term performance.

So, the focus on this is the technical basis for those two aspects, and our recommendations are provided to DOE by the interim report, and a final report that will be issued in February.

The panel is comprised of myself as Chair. I'm a professor at Case Western Reserve University. Dr. John Beavers is an executive vice-president of CC Technologies, which is a contract research organization in Columbus, Ohio that does work on corrosion, materials performance. Tom Devine is a professor of materials science and engineering and Chair of the Department of Materials Science and Engineering at Berkeley. Gerry Frankel is a professor at Ohio State of materials science and engineering at Ohio State, and Director of the Fontana Corrosion Center. Russ Jones is a senior scientist and technical group leader at Batelle-Northwest. Rob Kelly is a professor at the University of Virginia in materials science and engineering. Ron Latanision is a professor of materials science and engineering at MIT, and also Director of the ULIG Corrosion Labs.

All of us have background in materials performance, reliability, life prediction in different aspects of corrosion, corrosion science.

In addition, the panel has the availability and the use of what are being referred to as subject matter experts. There's on the order of 15 of these people that give the effort an international flavor. We have representatives from Japan, Sweden, Finland, and England, U.K., and also other North American participants who have expertise in particular areas, such as passivity, localized corrosion, geochemistry, hydrogeology, and so forth. These are people that are available. They're under contract to assist us with this in specific topic areas.

So, the overall efforts here are comprised of the Panel. Our Panel report is the consensus document of these people, and these folks are providing technical backup and support to the panel.

In addition, the Project staff has been very cooperative and working with the Panel, and also we've had representatives from other factions looking at the Yucca Mountain repository.

There's a couple of noteworthy, or several noteworthy events of technical exchanges that have occurred. on May 23rd here in Nevada, we had the official kickoff meeting of the Peer Panel.

On July 19th and 20th in Arlington, Virginia, the Board organized, primarily Alberto was the head of that, the guide of that, an international workshop on long-term extrapolation of passive behavior. Several of the Panel members were able to attend parts or all of that, and a couple of the subject matter experts on our Panel also participated in that workshop.

A significant event--I'll make a couple comments about that, and I believe following me on the agenda, Alberto has some time to give you more of a flavor of what occurred there.

Tied with that meeting, we had what we're calling a subgroup meeting. Several of our subject matter experts, Panel members and interested others attended, stayed over in Arlington, Virginia for another day to discuss localized corrosion issues. And some of the Project staff were able to join us at that as well.

On July 24th in Cleveland, we had representatives from the State of Nevada, and representatives from the Nuclear Regulatory Commission that came and made technical presentations to the Panel.

On August 10th, out at Livermore, we had a subgroup of Panel experts and subject matter experts and Project staff people discussing the issues of waste package fabrication.

So, we're working as a Panel as a whole, and also as subgroups within this area.

This just is the overall schedule. We're at September. We've delivered our September 4th report. There will be an open meeting for presentation, it will be a full day meeting September 25th here in Las Vegas. The Panel, Panel members will make presentations in the morning, and we'll have presentations from Project staff in the afternoon. So, that's scheduled for September 25th here in Las Vegas.

We will complete our analysis and evaluation over the October/January time frame, and we're on schedule to--are scheduled to issue our final report in February, and the response to that report by the Project is due in the April time frame. So that's where we're at.

I'd like to give you a flavor of some of the general findings, and then some findings and specific sub-issues that we're looking at.

The Panel has found no technical basis for concluding that the waste package materials being considered now are unsuitable for long-term containment. There are technical issues that remain to be resolved, remain to be settled. The likelihood of resolving those and removing uncertainty is great. This area of corrosion of materials, materials performance is an area that is amenable to experiment. It is amenable to modeling. And, so, further work and experiments and analysis has, in our opinion, a great chance of reducing that.

Much of the experimental and analytical work to support performance assessment is underway. A large amount of the necessary work is in Project plans, and remains to be done. So, our opinion is the Project is moving in the right direction. Their approach is sound, and there's work to be done yet.

The effective control of corrosion of waste package materials is essential for the long-term performance. The most viable realistic threat to waste package performance is corrosion in aqueous environments. So, that corrosion has to be managed, and that's been recognized for quite a while, and it continues to be a critical issue.

So, much of the performance, long-term performance projection revolves around how do you manage, how do you control corrosion in this particular application.

Corrosion has been a problem industrially for many years, and there's a common approach that has evolved over that time on how one addresses any corrosion problem, and that is determining the realistic range of aqueous environments that can cause corrosion, and then the selection, the use of materials that are resistant to corrosion in those environments.

When the packages are dry, in the absence of liquid water on the metal surfaces, corrosion is not a problem. Okay? So, we're only dealing with, we're concerned about corrosion in the aqueous state, when there's water on the metal surfaces.

Nickel-base Alloy 22 and Titanium Grade 7 have excellent corrosion resistance over a wide range of aqueous environments. The key issue becomes do they have adequate corrosion resistance over the realistic range of environments for the Yucca Mountain application. That's where the attention is being given. That's where the major issues are.

We looked at several sub-issues or categories of issues in this overall assessment. One of them was are the correct potential degradation modes, the processes that can degrade materials, has the Project identified and are they dealing with the right set of those degradation modes. So, we have a couple comments on that.

The second is are the environments being determined? Can they be determined? Is the approach taken by the Project, are they determining a realistic range of environments?

And then we have comments on three specific corrosion modes, long-term passive films, these alloys all depend on a thin oxide layer, self-forming oxide layer, for their corrosion resistance. If that oxide layer remains intact, the alloys have long, long life, very slow penetration rates. If that oxide layer breaks down, then the penetration rates can be more rapid.

So, the three corrosion issues we're involved in are the long-term behavior of passive films, localized corrosion where that passive film may break down locally, and stress corrosion cracking. And, so in the remainder of this, I'm going to talk about the headlines or the overviews of that from our interim report.

In the area of degradation modes, the Panel concludes that the Project has identified and is looking at the correct damage modes. Corrosion, as we said, is the most significant potential degradation mode. And the Panel has identified three metallurgical processes, three long-time processes, or processes that can occur over long times that could affect the corrosion behavior or the mechanical properties of the alloys.

These aren't degradation modes in themselves, but they could lead to changes in corrosion behavior or mechanical properties, and we identify those three in the report.

The Panel notes that design and fabrication details can have a great effect on corrosion and mechanical behavior. How the packages are welded, how the packages are fabricated, what sort of stresses remain, how those stresses are managed, all of the details of design, fabrication and assembly and emplacement can have a significant effect on the corrosion behavior and/or the mechanical properties of the packages. And, so, there's issues there that need to be addressed.

There's other degradation modes, for example, just a mechanical failure and/or hydrogen embrittlement, which is another important failure mode that needs to be addressed in the Project. And then there are other contributing factors that can affect those. Radiation effects, radiation from the, the flux of radiation from the spent fuel itself has the possibility of affecting the mechanical properties of the metals. The radiation can affect the types of composition in the aqueous film. And, so, these areas have to be addressed. They are being looked at by the Project, and we suggest how much more of that should be done.

Microbiological activity is an area that needs to be addressed. And, again, the fabrication issues can contribute to this.

The status of this is that the Panel is reviewing Project plans in each of these areas to see in our assessment where are they, how far should they go, and hopefully to suggest approaches to get to an answer on that, so we can compare what's being done to what we might suggest being done.

The view that's evolved for understanding the mountain and how it behaves from a corrosion standpoint and, again, coming back to the waste package materials, corrosion is the main issue. We see the world, corrosion folks, as having two distinct water types moving through the mountain. And the terminology we're using here is as the water moves through the mountain and the rock and is coming down approaching the tunnels and the drifts where the packages are, we refer to that as water. When it gets onto the metal surface, we refer to that as an aqueous environment.

That might seem like nit picking to you, but when you get seven corrosion folks in a room, that was an important distinction.

Waters, in our mind, are the waters that are moving through the rock, and changes that can occur to those waters. When they get onto a metal surface, we talk about the aqueous environment.

Essentially, there's two types of waters, and Gerry mentioned this in his presentation earlier, and it has to do with the relative amounts of calcium and carbonate in the waters. If the calcium is there in excess of carbonate, as the waters evaporate, as you remove water, calcium carbonate precipitates and you're left with an excess of carbonate. And, so you go to mildly alkaline, sodium-carbonate types of waters.

If the calcium is there in excess to the carbonate, as you remove water from evaporation, you precipitate calcium carbonate, all the carbonate is removed and you're left with the calcium magnesium type waters. And, so these are near neutral type waters, pH 6, plus or minus. These waters can go from 8 to 12 or 13.

All of these waters, either the mildly alkaline carbonate waters, or the near neutral calcium waters, have an ensemble, a mixture of chloride, sulfate, nitrate, many different anionic species there, and that's important from a corrosion standpoint, corrosion behavior.

The Project has focused on studying these waters, determining these waters by evaporating, removing water, which can occur because of the thermal effects, and has shown that these two general solutions described above are true. And the Panel basically finds that this is a consistent way to approach this problem, a technically sound way to approach the problem.

So, two waters are going to be coming into the drifts potentially to drip onto the waste packages.

When those waters come in contact with the metal surface then, or when the metal surfaces get wet, we see three different conditions that can pertain on that metal surface. The first one is a moist dust. Again, remember when it's above the dew point, when there's no moisture, no liquid water on it, corrosion is not an issue. Penetrate rates are extremely slow.

And, so the metal surface is sitting with a drip shield over it. There is some dust and particulate that can settle in on that, and Gerry had some comments about the makeup of that, being primarily silica based dust and deposits, but it can also have any other environmental species that settled in on it.

As the package cools down, you will get to a temperature at which moisture can start to form, and that's when corrosion issues have to be addressed. And in the absence of any dripping, if there's no seepage of water, then what will happen is that moist particulate will remain on the metal surface and the package will cool down until it gets to ambient temperature.

And, so that's the condition that pertains for the widest area of surface of the waste packages and drip shield. Moist dust on the surface. And the question is what kind of corrosion occurs under those conditions, and are these alloys resistant to that corrosion.

The second class of condition that has been identified is in the presence of droplets of water that can form and drop down onto the hot metal surface, when that occurs, you can lose the water and evaporate, and you have the possibility, the likelihood, of forming mineral scale and deposits on the metal surface.

So, in the area where droplets of water hit the metal surface, the condition is going to be scale and deposits on the metal surface, and the question then becomes how does corrosion--what's the corrosion resistance under those conditions.

The third important condition from a corrosion standpoint are in areas where there's tight metal to metal contact. And in a corrosion science and technology, we refer to those areas as crevices.

So, if you take two metal surfaces and place them together, moisture can get into that creviced area and the chemistry within the crevice can change from the bulk chemistry. And, so the waters that pertain, the environment that pertains in that creviced area can be significantly different than the bulk environment. The processes that go on are well known. The need is to apply what's well known to the specific applications of the Yucca Mountain project.

And, so we would suggest that the Project undertake comprehensive experimental and analytical programs, modeling programs, to address these three conditions with the waters that are coming in.

So, you've got two families of waters coming in, three different conditions, all of which should be and are amenable to experiment and analytical treatment.

The Panel recommends that in order to deal with those issues, that you need multi-disciplinary people. You need some corrosion people, you need some materials science people, you need some water chemistry people, and you need that cadre of people working together on designing the experiments and carrying out the experiments and the modeling. And, so we recommend that a task force be put together to address those problems.

Regarding the long-term behavior of passive films, these alloys, both Titanium and nickel-base Alloy 22, depend upon this thin metal oxide layer for their corrosion resistance. The question then becomes how sound is that, how protective is that over long periods of time. And that was one of the major topics, or the major topic of the international workshop conducted by the Board in July, was what is the long-term extrapolation of passive behavior.

Our perception, and we had to word this very carefully, because we had several Panel members who were there, we had subject matter experts that were there, the perception of the Panel members sitting through that workshop was, and I was one of them, that there is not such a great concern about the protective nature of the passive film itself, the oxide itself. The concern is will the environment change over time in some way to break down that film, or are there metallurgical processes that are occurring in the material below the film that could destabilize the film? Okay? So, that was our perception.

The Project has concluded, based on looking at the, or the Panel has concluded, based on looking at the Project's data from long-term test facility, from the long-term passive corrosion rates of these metals, electrochemical measurements, that the uniform penetration rate on these alloys is quite low.

The critical question is will that low penetration rate persist? And the Panel has identified in our report three areas that ought to be looked at, the intrinsic nature of the passive film itself, changes in the environment that could cause an instability of the film, and changes in the underlying alloy that could cause that instability.

And, so, experimental work and analysis over the possible range of environments is suggested, looking at these long-term metallurgical processes, and making projections about how rapidly they may go and what their effects might be. And, finally, in this area, the Panel recommends that a backup alloy, an alloy in addition to Alloy 22 nickel base, be carried along in the Program, not in each and every experiment, but certainly in enough of the experiments, because several of us on the Panel see Alloy 22 as a placeholder. It represents a highly corrosion resistant nickel based alloy.

Similarly, Titanium Grade 7 represents a highly resistant, corrosion resistant titanium alloy. Those aren't the only alloys in that class. And so we would suggest that a backup alloy be included.

We also suggested a comparison alloy, something like 825 perhaps, or pardon all this alphabet soup, but all these things suggest certain compositions of metal alloys, and essentially for the nickel base alloys, as the amount of chromium and molybdenum go up, the corrosion resistance goes up. There's a whole family of those alloys, and the benefit of including a comparative alloy which has less corrosion resistance than Alloy 22 is it can give you some feel for where you are and what your margin of safety is from the realistic environments or not.

As far a localized corrosion, the nickel-base alloy and Titanium Grade 7 are extremely resistant to localized corrosion over a wide range of environmental conditions. You can find environmental conditions, however, that will attack these materials. And, so the question becomes how do they behave in the realistic environments again.

The framework that the Project is taking is a valid approach, an appropriate approach. The Panel recommends a more rigorous way of looking at some of these critical potentials that are being measured, and the Panel again recommends that the conditions be looked at beyond the range of realistic conditions, so that some margin of safety or margin of behavior can be established.

In the localized corrosion areas, there's several issues to be addressed. It's been suggested here today, and we certainly support going to higher temperatures. The issue of once these degradation processes start, they don't necessarily continue to go forever. They will arrest in most cases, and restart in many cases. And, so that whole issue of arrest and re-initiation has to be addressed.

Again, we would suggest that the initiation, propagation and arrest behavior of these alloys be looked at for the three conditions that persist, or that pertain to Yucca Mountain, moist dust, scale and deposits that are west, and crevices between metal to metal contact.

Stress corrosion cracking, again, research to date, the experimental results to date, Project data would suggest that nickel base Alloy 22 and Titanium are resistant to stress corrosion cracking. The mitigation method, experimental approach being taken to this, is a rational, technically defensible, logical way to attack it.

The Panel makes some suggestions about deficiencies in the current program that certainly can be addressed, and the Panel recommends that, again, that alternative models be looked at in addition to the model that's being suggested now.

The purpose of looking at alternative models are, one, to validate and support the Project approach, and also to have an alternative in case this particular model is found to be deficient.

We make two comments regarding level of effort and organization. The Panel is concerned that adequate resources will not be allocated to complete the work. There's a lot of work that's necessary to be done. In Lake's opening comments today, he mentioned some of the budget pressures that are real. And, so much of the experimental and analytical work needs to be done. It's underway, but needs to be completed, a large amount of that necessary done, and so anyway, that's what we're saying, is it is important that the manpower, resources and level of effort be sufficient to meet the projected time frames, or something has got to give, either the decision points or the level of effort.

The Panel recommends a couple areas where we think a closer collaboration would be very well taken. One I've mentioned already in the area of determine the aqueous environments. We think that the corrosion, materials science, geochemistry, hydrology folks ought to be working in a combined official task group sort of method, or organizational method.

And we also suggest closer integration between design and fabrication engineers and the corrosion and materials science experts.

Again, industrial experience shows when the corrosion and materials science people participate early on in the design stages, that you can avoid some mistakes further down the road.

As far as going forward, the Panel has identified three categories that we're going to focus on in the remainder of our study, corrosion processes, environment and everything else. The last is just a curry of degradation modes, and so forth.

The hope is, and the intent is that the panel will not only identify issues that need to be addressed, but hopefully, we will also suggest approaches to be taken to address those issues.

Thanks, Paul.

CRAIG: Thanks, Joe. You're precisely on time, 30 seconds.

PAYER: Professors usually are wound up for 48 minutes, as you know, or the audience walks out.

CRAIG: I'm going to make a conjecture, which I'd like you to respond to, either affirmatively or negatively.

The conjecture is that if you were giving this talk ten years before a decision was to be made, and the resources were to be provided to do the work that you're describing, and when that work was done, the results came out as you expected, there were no negative surprises, positive surprise is okay, then everything would be dandy.

But, in fact, that's not the environment we're looking at. We're looking at an environment where what appears to be a go, no go decision is going to be made maybe this year, with no discernable retreat position, no credible back-out in case the future work doesn't get done, or it does yield surprises.

And you have these qualifiers, like you've not find a technical basis to conclude the material is unsuitable. That doesn't sound to me like a ringing endorsement. Or the

Board's workshop, the passive layer could, in principle, do the job for thousands of years. Again, it doesn't sound like a ringing endorsement. And then the resources are needed to complete the work, as you just said. So, there's a whole series of these which lead me to conclude that the program is on a good track, but it's got a time schedule mismatch, which is simply intractable.

PAYER: Well, the response is I guess that, two things, I think the decision point, as I understand it, and our Panel, you saw what the scope was, it was to look at the long-term performance material prediction, but obviously we recognize that what we say is going to hopefully have some relevance on things like site recommendation and work that needs to be done.

I guess I don't agree from my perception that the site recommendation doesn't have a retractable back-out. The decision is going to be is this a suitable site. It seems to me you could turn the question around and say is there a sound technical basis to show definitely that it's intractable, that it's an unsuitable site. And I think clearly, that's not the case. There's some important technical issues that have been pointed out that could, in fact, with further study, be found to be a show stopper, but they certainly aren't definitive now, and there's a lot of indications that they're going in the right direction. And there's a lot of resistance to this.

So, I think in my mind, the criteria for a site recommendation type of a decision is different than a license application. And that's all Joe Payer. Don't put that on the other six guys of this Panel. So, that's the point there.

So, I think maybe that's the difference in my distinction, is we're looking at work that certainly needs to be done before you're sure this is going to work. But it's going in the right direction. We think there's a lot of promising results.

CRAIG: That's a good way to characterize it, a very good way of doing it.

Questions? Jeff Wong? Jeff, you told me earlier you had already asked your question.

WONG: I know, I'm only allowed one question per meeting.

CRAIG: I just don't know what we're going to do with you.

WONG: This is Jeff Wong, Board. As a student of Alberto's classes on corrosion, I want to ask a very high level question.

These are--I think that Paul was getting at the fact that you are asking for studies or suggesting studies at a time when resources, money and time is getting tight. And to help me understand all the studies that you're asking for, are you asking for the studies to refine the projected performance of the system, or do you have a belief that performance can actually exist on the outside of the bounds of the current assumed or projected performance, or are you asking for these studies because you believe there's actual data gaps in the performance?

PAYER: Again, this is all my bias, or my perception and beliefs, I guess, here. My read of where the technical basis for long-term performance is that is showing promise, is showing good promise. I think it's not enough Is dotted and Ts crossed that I would feel comfortable marching in for a license application.

I think there's areas that have to be filled out. There's some legitimate issues that have been brought up that are worthy of study that currently the analysis and/or experiments aren't available to do it.

I tend to be perhaps an optimist about these things and think we could make a metal can that could live in a mountain of ambient waters at Yucca Mountain. But, the point is that doesn't matter. Is the technical basis there now or not? There's other people that aren't so optimistic perhaps on the overall scale. But, again, their opinion at this point I think is not so important either. You know, where's the technical basis? It shouldn't be Payer shouts louder than somebody else, or speaks better, or that sort of thing. It's what's the technical basis.

WONG: Well, if it's not good enough for a license application, is it good enough for a site recommendation?

PAYER: That I think is totally in the eye of the beholder. I really think it is. And that's everybody's got to do their own judgment on that. There's an awful lot of information that shows that these alloys behave very well under the conditions they've been tested under so far. Have they been tested under all the right conditions and all the conditions you'd like? Perhaps not.

WONG: So, as I take it, you believe that there's performance possibly outside of the bounds of the projected performance, but you really don't believe there's a data gap, any data gaps in what's been presented so far?

PAYER: Well, I'm not sure I said that. I think there are data gaps. I think there are data gaps. But there's some sound experiments that have been done. There's some sound corrosion behavior that's been done under what I think are relevant conditions. Have we looked at all the relevant conditions? I think probably not. But there's work in progress trying to determine where the realistic ranges are.

WONG: Thank you.

CRAIG: Debra?

KNOPMAN: Knopman, Board.

Let me follow up on that a little bit with some more specificity. This has to do with the composition of aqueous environments. And on your slide on that, Joe, you conclude at the bottom, "The Panel finds this treatment consistent with the current state of scientific understanding."

I want to make sure I understand what you're saying there, because you later seem to suggest that you want to look at a wider range of potential conditions. But what you're saying you find, the Panel finds acceptable here is the notion that there are roughly two types of water?

PAYER: What slide is that, Debra?

KNOPMAN: Do these have numbers? It's the one that says--

PAYER: Water composition, it's about 5, 6?

KNOPMAN: I don't know what number it is.

PAYER: That's it. Right there.

KNOPMAN: That one. No, the one before.

PAYER: Back one more. That's it. Okay, I think I know where you're going. Let me try to clarify.

This tries to capture what we said in three or four pages maybe in our report of the status of determining what the composition of water is at Yucca Mountain. And we think the approach that--the key spokesman for that has been Greg Gdowski. The approach they're taking with the chemical divide and the kinds of water chemistry that occurs when you remove water from these materials, that that is sound ways to deal with water chemistry in these materials.

So, we think that approach is correct, and we believe these types--this idea of two major categories of water, that makes technical sense to us.

KNOPMAN: To what extent did you delve into the issues of chemical composition at the high temperatures above, let's say, 120 degrees C., which we're now hearing might be--

PAYER: We recognize that, and we believe that. If you look at--I mean, again, it's pretty straightforward. If you start with a crystal of magnesium chloride on a metal surface, or on any surface, and you start at a high temperature with relative humidity and lower the temperature, at around 160, 170 Centigrade, you will get liquid water forming on that particular material.

If you have a mixture of salts, it will, depending on what that salt composition is, it will occur at different levels. One of the issues I think that hasn't been addressed, and while we're trying to focus the attention on this moist particulate and scale and deposits, and this is a question I have, if you've got a particulate a millimeter thick on silica and other things that have settled in on the waste package over time, and you start cooling this down and you start forming isolated thin films of moisture in that, is that conducive to causing corrosion, or not? It's not the same as having a specimen fully immersed in a tea cup of boiling magnesium chloride.

And, so, that's a technical issue I think that has to be addressed. Which way it comes out, I'm not real sure. But that hasn't been addressed yet.

KNOPMAN: Okay. So, let me just sum up so it's real clear.

The Panel is not saying that a full range, appropriate range of experimental conditions have already been--

PAYER: Absolutely not.

KNOPMAN: But what you are saying is that the approach to defining experimental conditions is on the right track, but it needs to be expanded and--

PAYER: And I think the other important part of that is we believe, based on what we've seen so far, that in fact you can put some realistic boundaries on this environment, that it's not everywhere all ways. It can't be the most oxidizing environment you can imagine, and the most reducing, and the highest acidity, and so forth.

The other clarification of why we think it's useful, and we would suggest that you go out of the normal what's expected to be possible at Yucca Mountain, are the very useful results that have come out of Scully's lab at

Virginia on looking at metal behavior in lithium chloride solutions. I don't think there's any likelihood of that environment existing on a waste package. But it's a very aggressive environment that allows us to examine the corrosion behavior of this material. So, it's useful, but that's the distinction I think we're saying.

CRAIG: We next have Priscilla. And who wants to talk over there? All three of you. Okay. This is so important that we're going to let our schedule run a little bit.

NELSON: Nelson, Board.

In your consideration of the environments, there's two issues that have come up in the past, and I want to know whether the Panel is going to consider them. One is what the environment really is going to be, or might be like under the drip shield, and the evolution of that environment as separate from the one in the overall tunnel.

And the second one is the influence or possible influence of the steel sets, or other ground support that may be there. Will your Panel be considering that, or have they already started to consider either of those two?

PAYER: I think in detail, we will not be going into analysis. I think we cull those out as issues that need to be addressed. We're not going to do--we don't do anything. I mean, we suggest ways of doing analysis. We, in fact, will be doing some things. We'll be trying to do enough analysis to maybe convince ourselves that that's an appropriate approach or not.

The environment underneath the drip shield I think falls into this moist dust type of a situation, again, where you're going to get a condensation in that area. If it gets to the point where you would form droplets of water, if that's a feasible thing to occur or not, then you have to deal with the liquid droplets of water.

NELSON: Well, regarding that evolution of the environment underneath the drip shield, do you know what the Project is doing to try to characterize it? Would you expect you, with your expert support, would be commenting on or addressing the issues of the evolution of the environment under the drip shield?

PAYER: We have met as a subgroup, a couple Panel members and a couple subject matter experts, with Project staff working in this area. We've done that in the past, have discussions just to try to get a feel of what approach they are taking, where they are in their studies, and I anticipate that we would get together with that group again to discuss some of these. If we can get to that specific issue, I'm not really sure.

CRAIG: Okay. Among the three of you, who--Bullen?

BULLEN: Bullen, Board. Just a couple of quick questions.

First, I was intrigued by your comment of Alloy 22 and Titanium Grade 7 as placeholders for the evolution of whatever waste package material and drip shield material is selected. But, I guess the following question is do you foresee a change in alloy family, or do you think it's going to be a nickel, chrome, moly kind of alloy that will probably come to the forefront?

PAYER: My, again, personal opinion is we are at the most corrosion resistant metals that we have, the Titanium class, and for passive metals in an oxidizing environment, this is where you go.

The kinds of things that may happen, though, is you just look at the evolution of the C-series of alloys, it started with C-4, and all these are jargon names that Raoul might be responsible for some of these from his past life at Haynes, but there are alloys that have been developed where you're tweaking the composition, and one of the important things that they're tweaking is trying to maintain the corrosion resistance and offset any of these long-term metallurgical processes or processes that occur with welding.

And it may well be that, and I don't know this, but it may well be that a tweak of the composition, either within the current specification of C-22, to tighten it up somewhat, or to move slightly away from that, could avoid a long-range ordering problems. I mean, I don't know. That's just pure speculation. But that sort of thing happens, and that's been the natural sort of evolution of those kinds of alloys.

So, to think that--I personally don't think we're locked in to C-22 for evermore, if a better alloy comes along and there's time to substantiate it. At someplace, you've got to draw a line in the sand and do all your fabrication testing and all that sort of thing. But we're just suggesting that a backup alloy be brought along with this.

The Titanium Grade 7 has a palladium addition to it, and already there's a lot of work of trying to substitute ruthenium for that, and showing that it has the same corrosion resistance. Well, if that becomes a commercial alloy five years from now, why lock yourself in for evermore to Grade 7 if Grade XYZ has the kind of performance. That's all I'm saying, is keep an open mind of it.

BULLEN: Bullen, Board.

I think you're right, and I think that in the nuclear industry, there's been a history of this when you start with just looking at the evolution of the 304, 304-L, 316, 316-NG stuff. I mean, these are the evolution of alloys that are used in the nuclear industry, and so there's a parallel there.

PAYER: But in the licensing mode, you have to qualify each of those changes, but it's a doable, amenable thing.

BULLEN: I guess I have just one more follow-on question, because you talked about the environment and the dust and the deliquescence issues associated with the formation of a water film. Did your Panel address the issue if you had a cooler operating mode and you didn't go there, would it make a problem go away? I mean, if you never went above 80 degrees C. on the waste package surface, does that make a difference in your expected performance?

PAYER: It would take all those processes above 80 off. But to a certain point, and I'm glad you brought it up, because a comment was that I was involved in the TSPA-VA, the viability assessment, and it seems to me that one of the very significant things that's happened in the Project since that time frame is, you know, now we're talking, the Project is talking about it, you're talking about it, we're all talking about high temperature operating mode and low temperature operating mode. In that time frame, we're talking about a really hot operating mode, and where the entire footprint was going to get above boiling. And to my mind, a significant amount of uncertainty has been taken out of the program when the decision was made not to have overlapping dry-out zones from drift to drift. Okay? I think that made a significant difference in just understanding how the process behaves. It's still complicated.

How low do you go? It's sort of, you know, my colleagues and I were sitting around the other day and said universities would be a lot less complicated if we didn't have all these damned students. You know, the heat comes with this stuff, and so at some point, you get to a trade-off. But the straight question is what do you have to do to get there? And is a trade-off there or not? So, I'm open on the--and, again, this is Joe Payer--but high temperature operating, low temperature operating, you start to get in a mode where there are some trade-offs. Looking strictly from corrosion standpoint, sure. Lower temperature is better.

CRAIG: Thank you. Don?

RUNNELLS: Runnells, Board.

You answered one of my questions just now. You defined, I think, what a backup alloy is. It's an alloy that is sort of on the horizon as being potentially even better, and you're recommending that one of those be brought along through the testing program?

PAYER: That's a possibility.

RUNNELLS: Okay. Would you choose one if you had to? Do you have an alloy--

PAYER: If I had to, I would. That's not on my peer panel charge.

RUNNELLS: Is there something that you would say, oh, this is likely to be as good as C-24, so we'll bring it along?

PAYER: I think to a certain extent, I mentioned, and these are not new, we mentioned three long-term metallurgical processes that can affect these alloys, and those three are you could precipitate phases that take certain things out of solution. And so you could have localized corrosion around that. That has to be looked at.

These alloys in some circumstances are prone to long-range ordering. All these are detailed metallurgical processes. But that can greatly affect the mechanical properties of the material. And you can segregate things like sulfur and phosphorous to grain boundaries and surfaces. That can have an effect.

The work we don't believe has been done enough on those to see if they are problems or not. There's strong indications we're on the borderline, perhaps we're below it, where there won't be a problem. But there might be a problem. If that was the case, then I think tweaking the alloy could be a way out of that.

RUNNELLS: Is your Panel going to recommend a backup alloy?

PAYER: No.

RUNNELLS: Okay. On the slide that's on the board here, you talked about mildly alkaline sodium carbonate waters moving down through the mountain as a second family of waters. I may misunderstand, but I thought that kind of water came out of the J-13 well, which is a saturated zone

1000 feet below the repository.

PAYER: Yeah, I'm a materials scientist explaining to a geologist how water moves through it. The distinction I have is this. Not necessarily waters moving down through the mountain. From a corrosion standpoint, we're interested in what's the composition of water that could drop on a hot metal surface, and all the changes that could occur to that water before it gets there. And what we see, and what we understand is the case, is there are two distinct classes of those waters based on the calcium to carbonate ratio.

After it's been thermally affected by driving water out of it, concentrated, you know, the non-volatile species concentrate in it, we think that water could either wind up being a near neutral carbonate water, or a--excuse me, these two--either mildly alkaline carbonate, or the near neutral calcium, magnesium based.

RUNNELLS: It doubles the experimental load. It doubles the number of experiments to carry along those two waters.

PAYER: It does.

RUNNELLS: And I'm not sure that it can be defended that both of them are appropriate for the repository horizon.

PAYER: My understand, again, of what I hear now is you'll be very hard pressed to eliminate either one of them. I think they both are potentially realistic and could occur under some circumstances. And, also, while those are the two classes, there could be many subclasses within each class.

CRAIG: Richard Parizek?

PARIZEK: Parizek, Board.

Priscilla sort of asked about this under the drip shield, is there a third water? That is, if we have condensation of a vapor phase that somehow gets in the repository environment eventually, things cool down and we see condensation, and, again, Mark Peters may give us some information on what we see in the cross-drift in terms of, you know, the water, either the dripping water or its condensation water, is that a different chemistry than what's listed here of the two types of water that you have. Condensation on cool packages, it's like my toilet boil in the summer always gets wet, sweats, will that happen in the repository?

PAYER: Again, my understanding is, and my feeling is, that it would fall within these two classes. What it could be as a condensate water, typically, the condensate water is much more dilute. It could be a much more dilute issue. Well, it better or worse gets into, you know, how much and how many, and all that sort of stuff.

PARIZEK: It's not more aggressive than either of these two, perhaps? I mean, it's almost distilled water.

PAYER: It depends on what gets solubilized and how much it builds up. But, in general, yes, less concentrated solutions are less aggressive in a general sense. That's true. But I don't think there's any water in that that doesn't fall into this type of class of materials and types of study.

CRAIG: Last question from Carl Di Bella.

DIBELLA: Thank you. I wasn't going to ask a question, but something you said, Joe, in response to Dr. Knopman's question I think really needs clarification. And what you said was something like John Scully's lithium chloride experiments, we're never going to have that kind of environment in the mountain, but you need to do these experiments to get extremes.

And did you say that because you're not going to get a high build-up of lithium in the mountain, or did you say that because you think, your Panel thinks that you're not going to get a high chloride to beneficial ion ratio happening in the mountain? With regard to the latter, since you talk very specifically about scales forming due to episodic or dripping processes, and since you're going to have changes in temperatures and humidities in the mountain, just because of the different decay of each of the waste packages in the different drifts, you've got all the forces there for selective enrichment or depletion of anions, and you can have some sort of separation. So, could you clarify what it was you meant?

PAYER: Two quick issues. You're talking it to a level more detailed than I certainly intended with the response here. It's not a wrong way to go with it, but I was looking for an example basically of a useful accelerated test environment that could allow us to probe the corrosion resistance of these materials and where those boundaries might be from a temperature and composition standpoint.

Our Panel certainly as a panel has not addressed the issue of how aggressive and where boundaries, you know, where can you go to those boundaries. My personal opinion is it's still an open question, but I find it hard to believe you're going to get a 6 molar lithium chloride environment as an aqueous phase in contact with a metal surface and it will stay there very long. I haven't seen the analysis to disprove it.

The issue of segregation and what kind of--how do the anions segregate, the chlorides, the nitrates and the sulfates, it's an open issue. I think it's amenable to experiment as well.

CRAIG: Joe, thank you very, very much.

The Board ran a Panel last spring on passive materials, and Alberto Sagüés is going to report on it.

SAGÜÉS: Well, very good. This is sort of an uncommon situation in which we have a Board member addressing the other Board members. I guess you'll see how we treat each other in a minute.

By the way, I didn't make any comments on the presentation, Joe's presentation, because I have read parts of the preliminary report and I think there's a number of very valuable issues there. And this just came out about-- looking at the performance of materials, not just in the environments in which they are likely to perform, but also try to go into areas which are more challenging. It's certainly something I agree very much with.

Okay, very good. In the last months, we have--in July, we had this symposium. We are going to be talking about the background of why we have that, some of the results, and a very quick indication of what's next.

The main concern that we have was this unprecedented extrapolation job that we have to fulfill, experience for passive alloys like stainless, aluminum, and some of the materials like Alloy 22 that span maybe a century or so, and now we're talking about extrapolating these materials over something which is, say, two or three orders of magnitude beyond that. That's something that really has not been done in the past in the area of materials science.

And we do have, like everyone else, a few concerns about this, and we have a couple of those concerns specifically that we really wanted to look at in great detail. What we wanted to see is if we could find out any plausible mechanisms by which Alloy 22, the passive layer specifically, could degrade in the long term, leading to increased corrosion rates. And that's item number one of what I'm going to do in a second. The deal being that if the rate of dissolution of this alloy is maybe a hundredth of a micrometer per year, or a tenth of a micrometer per year, well, then yes, you would expect to have 100,000 years before you chewed through about 2 centimeters thick of that material.

However, if that rate goes up to as much as maybe, say, a micrometer per year, or so, then we're in trouble, because we are getting close to the time in which we will have penetration, widespread penetration, over many of these packages in a period which is comparable to the performance period, say, 10,000 years, or so. Then we really will be in trouble. And we're talking about a difference between something which is almost nothing, to something which is next to nothing. In other words, we are talking about those things in which we're looking at a very, very tiny change making a big different. And that would be number one. Are there any ways that maybe--maybe hasn't thought about very much whereby that could go wrong.

The other issue has to do, of course, with localized corrosion. So far, most of the project assumptions for performance evaluation purposes use a criterion in which they compare the episodic potential of the material with some kind of a critical potential. And then there are a couple of things that could happen, making one of those potentials may start creeping in the wrong direction as a function of time. Or maybe, the other possibility, the other process is maybe the whole concept is really not a very valuable concept anyway for long-term applications. Maybe there is not such a thing as a critical versus a potential. Maybe the concept of a threshold is not really very appropriate.

And then, of course, if you can identify things which haven't been so far identified, then what can be done to test for those specific issues.

Finally, one last objective of this workshop was to broaden awareness within the corrosion community of the Yucca Mountain corrosion issues. A lot of the corrosion engineers, corrosion scientists in the world are really not very much aware of the issues that we have to deal with.

This was held on July 19th, and the morning of July the 20th of this year, and there are a number of things available on our web page. We have the workshop participants, the background material, the kind of questions that were given to these participants to trigger those issues, and finally, there are complete transcripts of the workshop.

The people involved in this read like a Who's Who of corrosion science and passivity, and I'm not going to go through this list. Some of them have made presentations to us of the Board on these issues. Some of them are consultants retained by the Department of Energy, and so on, and you can look at this list at your leisure.

The printout for this presentation is available outside, I believe, at this moment. Yes, it is.

Let's go to the next slide here, and let's look at some of the ideas that the symposium participants brought up. Now, some of these ideas of course have already been known and acted upon by the Project. But I'm going to highlight some of the things which were considered to be particularly of interest.

One of the things is that the passive corrosion may proceed at different rates for the various alloy components. C-22 has chromium, nickel, moly, tungsten, and so on. And, if that happens, you may end up with a situation in which you may end up with defects called vacancies. For example, at the metal-film interface, if that accumulates over long periods of time, it could create a problem, and the passive rate of dissolution may become greater than otherwise anticipated, maybe going over about a micrometer per year, or so. That could really spell trouble.

And, indeed, the Center for Nuclear Waste Laboratory, I believe is the exact name, of NRC is addressing some of these things from a theoretical standpoint.

Another issue of course is what happens if something like a transpassive dissolution were to take place of potentials which are moderate potentials as opposed to very high potentials in which transpassivity is usually observed.

Another thing that was brought up at the workshop was what would happen with local changes in solution composition, again, near the metal surface passive layer interface. And in each one of these cases, you may end up with what could considerably be a substantial increase in the rate of passive dissolution.

Something that was brought up that really hasn't been brought up very much was that because of the nature of the repository, although the system could be mentioned to be a very steady state type of system, it might not be so. There could be situations over the long term, and perhaps even over the short term, depending on the way in which the thermal processes operate during the warmer phases of operation, in which you may end up with cyclic conditions, some packages becoming wet for a certain amount of time, maybe dry off, and maybe wet again, and so on.

Under those conditions, the passive layer may respond in ways which are not well known at this time. Indeed, the passive layer is speculated to possess an inner film with a relatively low density, and an outer hydroxide film. And it has been pointed out by symposium participants that indeed the interaction between the outer film and the inner film could be adverse under these conditions. And in terms of this, by the way, are things which are assumed to be--by some over the symposium participants which are preparing brief write-ups on these particular issues which will be disseminated afterwards.

Another issue is roughness of the passive film. So far, conceptually, one tends to think about the passive film as a relatively thin, a few nanometers thick kind of layer. But, in reality, whenever you have any of these processes, you end up with a certain amount of surface roughness, as evidenced by all kinds of evidence of some electrochemical--and in that case, you're going to be dealing with a larger surface area than otherwise experienced, other ways in which the layer may evolve that are not yet anticipated.

Now, this is really a very general kind of statement, but I include it to emphasize some issues which are really important. Specifically, there's very little known about the nature of the passive film on most materials, and specifically on C-22. We don't even know whether the film is crystalline or is amorphous. We don't know exactly what is the thickness of the film. We don't have even direct evidence that the film on C-22 is a double layer film--double layer is not a proper name--but a two layer film, with indeed an inner protective barrier and an outer less protective barrier. Those things are speculated. Those things are expected. But it's not quite known.

We know very little about the way the thickness of either portions of the film changes with time, and we don't know about whether it's a morphological structure, crystalline versus amorphous, will itself change with time. So, there's a lot--we're basing quite a bit on that.

Let's go now to the next one to talk a little bit about issues that concern the second question, specifically whether there may be ways in which the expectations for localized corrosion may be different from what could be thought otherwise.

Let me highlight quickly a couple of these things. One of the participants, Norio Sato, brought up the issue that under radiation, and I'm talking about likely the low level gamma radiation, you could have, whenever you have barriers which are of the semi-conductor p-type, in those cases, you may have increases in open-circuit potential, even under--this is because of the process happening intrinsically in the passive layer, not on the water around the passive layer.

The role of sulfur or other impurities may be forceful, even in the system, can have some interesting effects. You may have accumulation of sulfur at the passive film metal interface as the layer as a function of time, or you may have interactions through segregation phenomena that have not been until now pretty much considered.

And then the other issue that was brought up by several participants was possible synergistic effects of chloride and fluoride ions.

I want to highly the particular issues in here. What will happen, or what will be the behavior of this--of whatever is on the surface of C-22 after this long stretch of relatively dry regime, after several hundred years of being relatively dry, for example, depending on the final design of the repository. Now, finally, moisture will begin to develop, and now we're dealing with a very long aged dry film all of a sudden being faced with a wet environment.

And the other issue is that there's a lot that needs to be known about what is happening when you have very deep crevices.

Okay, let me quickly finish by showing some of the research analysis ideas mentioned by the participants. These are not, I want to emphasize, these are not--this is not research that the Board is proposing to be done. This is what the participants of the symposium felt that might be of interest.

A couple of highlights on things that maybe have not been done by the Project to a great extent, or maybe they're being done, but they're just beginning. Structure of the passive film as a function of potential time and anion concentrations, specifically chloride and fluoride. What happens to the relatively dry films when they are put in with environments, which is this one over here.

Another thing that I want to highlight, and you can look at the rest of them in the printout, this is something that the Project is doing to a certain extent, but it may be the key to a much deeper understanding of what is going on, specifically to look at the kinetics of the redox reactions on the passive layer and the transport processes in the passive layer itself, and looking at this from a fundamental standpoint. And, of course, investigate the effect of surface roughness and how that may affect the evaluation of corrosion rates which are being presently conducted.

Here is one that appears to be quite interesting, that is looking at the behavior of Alloy 22 when there are increasing amounts of impurities, specifically different sulfur, different levels of sulfur in the alloy. And then this is something that of course the Project is addressing to a certain extent at this moment, but it may be particularly fruitful if this is continued, ironing out some of the issues that Gerry was mentioning this morning.

Okay, I'm going to then finish by going to the next one, and the symposium participants are preparing the summaries. The Board will decide how to disseminate this. One possibility is that they may appear in the form of symposium proceedings. In that case, an attempt would be made to do it in as standard a manner as possible.

Then, of course, the Board is going to evaluate what we have seen in this symposium, and that accordingly. Anyway, this is a very quick run-through to what happened at the symposium, and I will be glad to answer any questions from my colleagues and staff as well.

CRAIG: Okay. Dan Bullen?

BULLEN: Bullen, Board.

I guess I should have my own perspective because I was at the workshop. But I guess I'd like to ask you were there any surprises? Were there any things that you saw or that the workshop Panel saw that were surprising in your eyes?

SAGÜÉS: Well, what is a surprise depends on kind of what you are expecting beforehand. Let me talk for myself first, and I heard the participants say a good number of things that they expected to be mentioned. For me, more than surprises, I would like to see new angles on things that I haven't thought about before. For me, one of the most interesting areas was the mechanism proposed by Phil Marcus on the possible role of sulfur, namely as the material corrodes passively, the passive layer begins to bore deeper and deeper into the metal, and it may encounter a number of defects, but in particular, it may encounter small amounts of residual trace sulfur in the material. Then they have nowhere to go, and then it may end up accumulating at the interface, and that's something which I believe was picked up a little bit by the Peer Review Panel people as well.

That was something that for me was quite interesting because it's something that we haven't talked about. Mechanism of possible--a potential open-circuit potential changes in the passive layer itself, because of interactions between either the gamma radiation or by--produced by processes triggered by the gamma radiation. That was interesting as well, as an example of the type of potential mechanism, think a little bit--of regularly addressing phenomena of passive radiation, which came out of the meeting.

BULLEN: Bullen, Board. One more quick follow-up. A previous distinguished speaker summarized this same Panel meeting by saying that the perception of his Panel was that those who attended the workshop think that passive corrosion rates of the alloys of Titanium Grade 7 and Alloy 22 in principle remain low enough for waste packages to survive for several thousand years. Did you have that same perception, or do you think the Panel had that same perception?

SAGÜÉS: I'm sorry, but Joe said, in principle, and I guess that depending upon what principle, the principle being that the passive layer is stable, then it's going to remain stable by definition. I didn't detect anyone saying that passivity is impossible over a period of many thousands of years. Indeed, I didn't hear any concern of that type.

I did detect in several of the participants a sense of caution about making a very long extrapolation. And I did detect, and these are the ones that I highlighted earlier, that very little is known about how passive films are and how they operate. We are relatively early in the history of corrosion science to have what I would say deepens our knowledge about how these things operate. And these morphological questions that I mentioned earlier are an example of that. We don't even know at this moment whether we're dealing with a crystalline layer or not.

BULLEN: Bullen, Board. Just one final comment.

I think there's a career in politics for you, because that semantics is really a wonderful thing.

SAGÜÉS: Thank you.

CRAIG: On that note, let's--Alberto, thank you very much. I'll refrain from giving my reactions to the workshop, except to say that it was a wonderful, wonderful workshop, and we all thank you, are indebted to you, Alberto, for arranging it.

Our next presentation is April Pulvirenti. And did I get your name correctly?

PULVIRENTI: Yes, you did.

CRAIG: I did? My goodness. Wonderful. And, April has just recently received her Ph.D. in chemistry from Purdue, and she's going to talk about experimental work on Titanium Grade 7, and the famous Alloy 22. I'll warn you when you've got about five minutes to go.

PULVIRENTI: Okay. I just want to start off by saying that there's not a whole lot of theory or horsetails in this presentation. This is entirely laboratory results. I'm not going to offer any speculations. I'm just presenting you results as we saw them in my lab.

These are going to be laboratory results on the corrosion of Alloy 22 and Titanium 7. We've done both, or we've done a little bit on both, I should say.

I divided this talk into three different parts. The first part is going to deal with Alloy 22. The other two parts are going to deal primarily with Titanium.

The first part is we've tested Alloy 22 under accelerated conditions. What I mean by accelerated, and admittedly, there's been a lot of talk about what will or will not exist in Yucca Mountain. But since we only have about a month to test these things, and 10,000 years in the mountain, we do have to make our conditions a little bit harsher than we expect. That's what I mean by accelerated conditions.

We have tested both disks and U-bends, and we have not been on this project for very long, so we've done primarily screening. We've screened for aggressive elements, mostly trace metals, that could possibly be within Yucca Mountain, and we've tested them with aggressive elements and we've done a little bit of extrapolation type studies under milder pH and milder temperatures. This is on disks, and we've been able to, through both weight loss and by solution analysis, determine as best we can a corrosion rate in micron per year by both those methods.

The other type, here's the U-bend testing, so we're doing both disks and U-bends. This is sort of a yes/no proposition with the U-bends. We did dry-out testing. I know that there's a low probability of full submersion, so we have done testing in which we have submerged our U-bend in something. We've allowed it to dry out, and we have essentially used the same type of accelerated conditions, and we've used mostly lead. We have done a little bit of varying pH and temperature and, again, a little yes/no, just to open it up and look at it and see if there's any signs of corrosion.

Let me wander back and give the results of the disk tests. Now, all of these numbers are corrosion rates that we have calculated in terms of microns per year. We've done it both by weight and by solution on the same disk. For the most part, there's fairly good agreement between the two.

The conditions, we've used these simulated 1000X, J-13 in this particular matrix. We have accelerated our concentrations up to 5000 ppm, although there is going to be some concentration. We have used an aggressive pH. 160 was originally thought to be an accelerated temperature, but from previous meetings, it seems to be not as accelerated as we thought. These are all 14 day tests.

The elements we're going to pick out, of course lead is giving us more than our control. Mercury is a funny beast. We're seeing very high results with cadmium and tin, some with selenium, and this is thiosulfate. This is S3 plus. We're actually seeing it with a sulfur complex as well.

Let me go back to this. Because there's been so much talk about lead and about mercury, and I've done a little bit with tin as well, we decided to use--we've gone to progressively milder conditions for some of these elements, specifically lead, mercury, tin and thiosulfate, just to get an idea of what would happen at a more mild pH, and a milder temperature or lower temperature.

This is a temperature extrapolation on lead and mercury. These are 30 day tests. And what we're seeing is as the temperature is reduced, we are seeing a reduction in our corrosion rate. However, it's still active, and given more tests, you could easily extrapolate this down, and if you had a linear line, you could extrapolate to whichever temperature could be expected. All of these values, again, are in microns per year. These are all based on solution, and they're based on the nickel. We have the solution results normalized for the other elements in C-22. Nickel is very close to our weight loss, so we have chosen to use the nickel solution. So, lead, as you can see, is still active even at 120, as is mercury.

We have gone to milder pHs on this, and we can see that even if we have no aggressive species, there's still a little bit of activation up to a pH of around, I think, 2.5. Lead is still active at a pH of 2.5. Mercury is active even up to a Ph of 5, as is tin. Tin seems to be quite aggressive in the higher pHs.

At a pH of 14, sodium thiosulfate is also aggressive. So, we have aggressive species at both ends of the pH spectrum. Now, the sodium thiosulfate is kind of funny. I think that's a weight loss, and the reason for that is that, and I'll show this in a second, the sodium thiosulfate number, we didn't trust the solution data on that because the corrosion that we saw with this came in the form of flaking, and we had insoluble material at the bottom. We weren't able to dissolve it, and we didn't trust our solution data.

To show you what I mean, this might not be all that easy to see on this particular overhead, this was a disk that was put again into a high concentration of sodium thiosulfate, and here is our high pH. And this is only 14 days. This is under an oxygen blanket. And it's very hard to see, and I have the original printout if anyone else wants to see it, but you can see sort of a flaking, bubbling wing right around this, if you can see where the red goes into the gray, you can see sort of a bubbling. That was before we cleaned it. We cleaned it in a cleaning solution and brushed it off and sonicated it, and you can see parts where it looks as if the metal has flaked off.

In this cleaning process, we had a .7 per cent weight loss, and this is due to sodium thiosulfate. I will also mention that we saw very nearly identical results, although not so dramatic, with sodium sulfide, which is S2 minus. So, we've seen it with both--with two types of sulfur we've seen this kind of thing.

The sodium thiosulfate we felt was enough--we did a little bit--we went into detail on the testing with that, and we've extrapolated or gone under more mild pHs, milder temperatures, and even less concentration to try to take some of the aggressiveness out of these tests to see if it's still active. And you can see here's the sodium thiosulfate at a pH of 14. These are all weight losses again because of that flaking. We're still at 49 microns per year, and as our pH was mildly down to 6.5, which I believe was within the range I've seen talked about, it's still slightly active.

We've seen the same type of thing with temperature. As we reduce our temperature, it's still active. And these are all 30 day tests. So, we're still active after 30 days. Even when we reduce our concentration all the way down to 500 ppm, we still see some activation.

So, the sodium thiosulfate still causes corrosion by weight loss over a fairly wide range of temperature, pH and concentration of the element, which is what we're seeing.

Those were the disk tests. The dry-out tests, we did quite a few of them. We, again, started off with our saturated J-13 water. This is the standard solution. We added our aggressive lead to it. We heated it at either 250 or 230 in a what I would call a partially sealed vessel, allowed it to dry out at that temperature. We opened it up to look for it, and what we saw, and of course in the most aggressive, we saw some pits on the edges. We saw pits on the edges, and we saw a couple lines on the apex we still don't understand.

We had a .1 per cent weight loss. But, again, weight loss is usually good for general corrosion, but localized corrosion, even a small weight loss could still mean a lot of local corrosion, and you'll see that in a second.

Again, we saw even more weight loss at 230 degrees and a pH of 1. We saw some pitting at the crevices underneath our Teflon spacer. And we even saw it at a pH of 3, not so much weight loss, but we still saw some crevice pitting. I'm not going to leave you in the dark. I am going to show pictures of these particular U-bends.

This U-bend is of the first one, the first entry I had up there. This is a pH of 1 at 250 Celsius. Within six hours, this dried out, and you can see that there's this little pit here right on the crevice. You can see there's lines kind of around the edge that we don't quite understand. There's a pit here on the crevice. And this is a close-up to give you an idea, and you can see this in the handout, that's a bubble from our mounting technique. This whole pit together is 80 to 90 microns deep, this particular pit. That's a pH of 1 and 250.

If I go to a milder temperature, which is at 230, you can see that we have a great deal of pitting at this edge and at this edge. We have a little at the corner. And in some of these we had pitting on the bottom. This is the most dramatic pit.

Here's another picture of it. This is another 70 to 80 micron pit. It's one of these. And this is at 230 degrees Celsius, and this one didn't dry out completely, and that would be with the vessel that we were using, just to show that even when we go to less temperature, we're still seeing pitting, almost the same amount.

This is at a pH of 3, and here we see it pitting, and this is a great deal of pitting. On this picture, we can see that's our action number, that's our identifier, and you can see there's a pit right on it. That's quite a pit. There's also some pitting here on the bottom, and it looks like some beginnings of some pitting even up here.

This is another--this is probably another 70 to 80. All these pits seem to be about the same depth, and this was at a pH of 3, or 210. And, so, when we get up in that high range of aggressive temperature and mildly aggressive pH, we couldn't tell. We needed to do more testing. But it seems that we were getting pitting, even if we make our conditions a little milder. And this is under conditions of dry-out, so this has not been submerged, and this was in the oven for 24 hours. We suspect it dried out within six. So, this is 24 hours worth of test. That's the C-22 parts.

I'm going to move into Part 2 now. We did some preliminary, and this is extremely preliminary, tests using the gamma radiation source, Cobalt source at the University of Maryland. We chose a solution that was a simulated 1000X, J-13. We excluded carbonate in light of the pore water results. As a value, we put in 1000 ppm aggressive species.

Now, in this case, our aggressive species were hydrogen peroxide, which is an expected product of radiation in water, and zinc 2 plus, which sounds like an odd aggressive species to put in. We chose zinc because it's known to be an electron scavenger. The main point of that is that electron scavengers tend to extend the lifetime of a corrosive hydroxyl radical, which is also a known corrosive species that is formed in radiation. We were trying to extend the lifetime of that hydroxyl radical.

My pHs were all at 7, and the temperature is only 40 degrees or less. So, we're talking extremely mild pH and temperature. This was irradiated over three or four weeks with a total of 89 megarads of gamma radiation. This particular picture is the before, this is just an untreated bend. We chose to use titanium foil for this.

This is a piece of titanium in the zinc. Even without any gamma radiation, we're still seeing a slight tarnish. It's hard to see. This particular roughening is about what the surface looks like without any treatment at all. However, if we add to it our gamma radiation, you can see even right here that there's a considerable spot on this particular disk, and you can see that there's a real roughening of this surface. And this, it's hard to see the scale here, but this is about a 10 micron roughening of this surface.

We see something very similar with the hydrogen peroxide. In this case, it almost looks as if hydrogen peroxide is aggressive in itself. This particular sample wasn't anywhere near the radiation. In the radiation, you can see that there's considerable tarnishing and some corrosion in the middle, and this is the surface. This is a metallographic cross-section, and you can see that there's another roughening of the surface around the 10 micron range. These, again, are very preliminary results.

Part 3, the tests on Titanium 7 using levels of elevated fluoride and elevated chloride. We chose to use elevated chloride and fluoride in light of both the Cragnolino and Abrasio electrochemical results from a few papers. However, for these two tests, we used disks and we did U-bends as well, and these are both untreated.

For the disks, we chose two environments. We used a calculated 10,000X J-13, and in these cases, we've chosen to use mild temperatures and mild pHs. There's no acceleration in the temperature and pH for any of the tests I'm about to show you.

The 10,000X J-13, we did disks. We also used a 1X EJ-13, and we added to it fluoride and chloride to simulate this is where the acceleration was, was added chloride and fluoride. Again, mild temperature and pH. We used this identical environment on the U-bends. We're not able to get numbers in terms of corrosion rate because the corrosion products adhere too well to our samples.

In the 10,000X J-13 environment, here's the disk right here, and you can see that this is dotted all over with pits. And on the edges, there is also some pitting. These are not defects from fabrication. We take this pit down here at the end, the metallographic cross-section, that's almost a perfectly hemispherical pit, and it's about--it looks to be about 40 or 50 microns deep, but this whole disk is covered with little pits like that, and it's in the 10,000 X J-13.

In the other environment with the added chloride and fluoride, just to show you, here's our conditions for this. Here's our 1X J-13. I added to it 35,500 ppm, that corresponds to 1 molar, 1900 ppm of fluoride, which, depending on which water system you're using, that's a conceivable level of fluoride. We have very mild pH. We're only at 100 degrees Celsius. After 30 days, we saw this roughening right here with what looks like some corrosion products inside. This is the cut line for megallography, and I'll show you a section of the metallography.

These two are identical. This, we had the background in the focus on the optical microscope. This is the foreground in focus. I'll show you the background first.

You can see that there's roughening even though it's out of focus right here. We can see the individual grains. And this is a corrosion product up on top we believe to be Titanium oxide. This is a big pit right here. We're also looking at a pit, I'd like to draw your attention to this line which follows between the grain. So, this is an intergranular attack of some sort, and it's extremely rapid. This particular, at its deepest, this is an 80 to 90 micron pit.

If we look at the metal being in focus, we can see that there is attack, you can see it's sort of digging down deeper, this white part is the metal, digging down deeper into the metal. I have a close-up to show it. We can see that there is some more intergranular attack here. We can see that there's intergranular attack around here. We can see there is a section of metal that is somehow either dissolved or come out. We can see attack everywhere, and you can see on our scale that this is still a rather deep pit.

We saw these disks and we decided to subject a couple of U-bends to this same environment, almost the exact same solution, and almost the exact same conditions, the same EJ-13 with the chloride and the fluoride, almost the same pH, and the same mild temperature. This is a 35 day test. Here is the U-bend, and you can see, around here you can see that there's a pitting here. That's a very, very deep pit.

That's in the hundreds of microns range. You can see that there's one here. There seems to be a surface preparation phenomenon that seems to cause initiation of the localized corrosion, not on these large bases, but for some reason, on the edges and in the crevices.

This back here, this is a close-up. This is the under side. We can see there's pitting here. We can see, this is not in focus, but you can see that there's a great deal of roughening here. I have a closer shot of that.

This is a metallographic picture of the bottom of that U-bend that we saw, and you can see that there's a great deal of roughening. This is around the hundreds of microns, as you can see, range.

I put two U-bends in under the exact same conditions. I used the same solution. That was one that I took out. I left this one in a little bit longer to see what was going on. We can see here, now this one, there's a great deal of corrosion on the apex of this particular U-bend. It's still together. You can see we haven't disassembled it yet. There's a great deal of powder here, and there seems to be some pitting here on the apex. That's at 67 days.

At 107 days, you can see something funny. There's kind of a line here, and it looks as if that pitting has extended somewhat. But after 164 days, we took it out. This has only been subject to a toothbrush here. We see that we have a crack. It goes all the way across, and all the way through, and it seems to start, you can see it started on the apex, and it developed, and at 164 days, it's gone most of the way through, and you can also see that even on this side, there's a great deal of pitting here. There's a great deal of pitting there, probably around 100 microns.

To give you a better close-up of the pit, this is better shots for the Board if they want a better close-up. We can see here this is that same U-bend. This is just the side-on view. You can see that here's the crack, and you can see the crack has gone through the metal and it's about halfway down the inside. If we had left this in for another couple weeks, we're guessing it would have just broken entirely. It's a fairly quick crack propagation.

But at this same time, we have a great deal of either dissolution or another mode of localized corrosion here, that's a great deal of corrosion on this side. So, we're seeing--and also, we saw evidence of pitting corrosion and crevice corrosion on the edges. So, this particular U-bend has three modes of corrosion on it. It's got stress corrosion cracking, it's got localized corrosion on this surface, and it also has crevice corrosion as well.

Just to get a better close-up of the crack right here, you can see it's quite big. This is an SEM shot. This is deposits of corrosion products. These we believe are corrosion products. We need further study on this sample. You can see these are the lines of 600 grid. And you can see it's just eating through. We're even starting to see some secondary cracks down this way. And you'll see that there's a--the Board will see there's a whitish yellowish type deposit. We think we have it characterized, but we're not entirely sure.

In addition to showing these three modes of corrosion, the solution itself had a white precipitate, and this particular precipitate, there were 100 milligrams of it that we know came off of this titanium. An the test is showing that it is certainly--it is titanium oxide and it's Anatase, which is not the rutile. It's an Anatase, we believe it's only kinetically stable, formed very quickly, I should say, the Titanium Anatase phase of titanium oxide.

Okay, so I'll conclude. At 160 Celsius, Alloy 22, under accelerated conditions, high levels of lead, tin, cadmium, selenium, sulfate at a low pH, there are high levels of general corrosion within somewhere between 2 and 500 microns per year.

If we lower the temperature and raise the pH and make the conditions milder, we still see general corrosion in the presence of lead, mercury and tin. It's reduced, but we can still see it, and it's still active.

At 160 Celsius, Alloy 22 is susceptible to flaking in solutions that contain either thiosulfate or sulfide at high pH. At lower concentrations, and more mild conditions, we still see some flaking and some weight loss.

Under dry-out conditions, Alloy 22 is susceptible to crevice corrosion, and you saw that very, very localized pitting underneath the Teflon spaces. Now, that is in a high concentration of lead. This was all found within 24 hours.

I have conclusions for Titanium 7. Titanium 7 is extremely susceptible to environments which contain fluoride. The pits that we see that are filled with those deposits, they happen almost every time we put it into a solution that contains fluoride. We see it on both disks. We see it on U-bends. We see marked pitting under those very mild conditions.

Titanium 7 is susceptible to stress corrosion cracking under the same conditions, again, very mild conditions, and also quite fast. This was five months, but the cracking itself I think started within about three months.

And in the presence of gamma radiation, not as much fluoride here, Titanium 7 is also susceptible to localized corrosion roughening in solutions that have either zinc, an electron scavenger, or hydrogen peroxide.

Thank you.

CRAIG: Thank you very much, April. A lot of information.

Questions from the Board? Alberto Sagüés?

SAGÜÉS: Thanks for the presentation. I would like for you to sort of put yourself in my position. You know, if I'm say in the business of evaluating, I don't know, a bullet proof vest of some sort, and then someone shoots at it with a little air pistol, then nothing happens. And then the next thing is someone shoots at it with a gun, you know, just makes a big hole through it. And, in a way, the presentation you just gave is the latter, and some of the other presentations that we have seen in the past are more like the first.

The point is, of course, if you expose the material to harsh enough conditions, then things will happen, and we see it in the pictures, and so on. So, the impression that I get is that yes, if you have an environment that's aggressive enough, C-22 is going to fail in a number of dramatic ways, as shown by your pictures.

PULVIRENTI: Okay, that picture is Titanium. It's not C-22.

SAGÜÉS: I'm sorry?

PULVIRENTI: The picture is Titanium.

SAGÜÉS: Is Titanium, okay, great. Anyway, what is the point then of your presentation?

PULVIRENTI: Oh, you mean of the aggressive species point of the presentation?

SAGÜÉS: Yeah, right. In other words, what is the meaning of this information and what pertains to the performance of Alloy 22 in the repository?

PULVIRENTI: Okay, Alloy 22, we needed to use conditions that are more aggressive than what we expect from a site characterization, very simply because I'm not going to live 10,000 years to take one of these out of the oven. So, we did have to use aggressive species.

I think the point of the aggressive on the C-22, now, they were mainly screening studies, and so we were just looking for elements that may be aggressive under harsh conditions, and if we found any elements that were aggressive under harsh conditions, then you go and you progressively make your conditions milder and milder to hopefully get a linear or a log scale line to where you can extrapolate to service conditions under that amount of time. And you can see that we are nearing the end of our, at least the end of our planned screening, and I was showing in the milder conditions, that some of these elements, these trace elements, in particular, lead, mercury and tin, are still active even under the more mild conditions.

So, yes, it's true if you make your environment aggressive enough, you can dissolve away, or you can corrode these metals under these conditions. But if you make the conditions more and more mild, and there's still corrosion, observable corrosion, then it's a sign that there should be probably more study on it to make sure that it can hold up under our expected service conditions. That's the C-22.

SAGÜÉS: But of course it's becoming milder, but it's still not what I could call terribly mild conditions; right? I mean, they're still operating at--

PULVIRENTI: Uh-huh.

SAGÜÉS: So, do you intend to now go all the way down to the other extreme? Because the interest in these things is not just going to something which is extremely mild, as some may envision what may have been done, say, perhaps even by the Project--that's not my statement--or you can make them extremely aggressive. But until we get to some point in between and we span the entire range, I don't think that we're furthering knowledge very much. Do you all intend to continue--

PULVIRENTI: Oh, I have the plan written out. We received our initial funding in November, and so we have not been on this project very long. And, so, we certainly do intend more mild conditions and more testing under these. As I said, these are all in a relative sense fairly preliminary, and they are all, as I said, screening for possible aggressive species, at least for C-22.

SAGÜÉS: Okay, thank you.

CRAIG: Debra?

KNOPMAN: Knopman, Board.

I appreciate the fact that you're in the early stages of your experimentation. But I'd just like to ask a couple of questions here.

When you were doing these experiments on C-22 and Titanium 7, did you also have some reference materials that you were running alongside?

PULVIRENTI: When you say--

KNOPMAN: I mean either some other metal or some other alloys for which there are known, fairly well known corrosion properties under at least some of these conditions?

PULVIRENTI: For example, have I tested Inconel 600, or something, under these conditions? We did do a comparison test of Inconel 600 versus C-22. It was a very small one. Our focus is primarily Titanium 7 and Alloy 22, and I would have done a backup metal, but Joe Payer only told us about it a half an hour ago, and I don't work that fast.

So, we did do a little bit of a study on Inconel 600, and in the presence of lead, Inconel 600 did perform. There was more corrosion on the Inconel 600, but we have thought of comparison studies, but we had to focus on something.

KNOPMAN: That's not an uncommon experimental technique to be running similar--running other kinds of materials under similar conditions to get some sense of perspective.

A related question is whether at least for these, for C-22 and Titanium 7, you were able to match your experimental results in milder conditions with what is considered standard results of behavior for these alloys under more, or let's say less extreme pH. I mean, I'm just trying to find out whether you baselined or ground truthed your experimental techniques, so that under these more extreme conditions, you have confidence that you're in the ballpark.

PULVIRENTI: When you say baseline, you mean--

KNOPMAN: Again, you know, if you're running any kind of experiment--yeah, I mean, there's published information both from the project, but also from industry, I'd imagine, on these alloys and the metal's performance and corrosion properties under less aggressive conditions. And the question is can you replicate, with your techniques, do you replicate those same results so that--I'm just try to put in perspective--you presented results under very extreme conditions. I'm trying to get an anchor on whether your methods of experimentation are consistent with what is used in another context for study of these metals.

PULVIRENTI: For background and for information on other metals and other techniques, I'll need to defer to Dr. Roger Staehle, who has much, much more experience in that. But from what I've seen, many of the tests that have been done have been electrochemical, and part of our plan is to do the tests what we call the old fashioned way, as to actually test the metal rather than to go through a--rather than to do the test electrochemically. That's part of our idea.

KNOPMAN: I'm essentially asking whether you calibrated your methods, and it sounds like you haven't quite yet.

The third question is has any of your work been through any kind of peer review external to your lab?

PULVIRENTI: I don't think so, not yet. These are very new results.

KNOPMAN: So, nothing has been published, and you haven't had anyone just come in to doublecheck on how you're doing, conducting experiments?

PULVIRENTI: Not yet.

KNOPMAN: Okay, thank you.

CRAIG: Okay. Dan Bullen?

BULLEN: Bullen, Board.

I'm assuming that your high temperature experiments were all done in an autoclave, so there was a high pressure. Do you know what the pressure was inside that container? Did you do a PV equals RT calculation and determine that?

PULVIRENTI: The pressure inside the dryout--well, no, there wasn't pressure inside the dryout. The pressure in the 250 is up around 1200 psi. At 160, I don't have that value.

BULLEN: Okay. And the 103 and 105 temperatures for the titanium disks, were they at ambient? Were they in open air, or were they still in an autoclave?

PULVIRENTI: They were in a Teflon autoclave, so it wasn't pressure, it wasn't under pressure at all. There was a little bit of evaporation under those. Butt it wasn't in a really sealed tight.

BULLEN: And as your follow-on performance plan, are you going to back down to temperatures like 85 and 95 and 60 degrees C. and see if you can take a look at the effect of addition of aggressive species into the same types of environments that the people at Livermore are doing?

PULVIRENTI: For the Titanium 7, we have started a program. We have gone down to 80, and we've gone from 80 to 160, and we seem to see the same sort of, at least in Titanium, we're seeing the same sort of corrosion. It's just taking longer.

BULLEN: Okay. Bullen, Board, again.

Changing a little bit to your radiolysis experiments, your dose rates are somewhere between two and three orders of magnitude higher than the expected dose rates, and I know you wanted to do that so you could actually see the results. But have you done any experiments to see if there's actually a dose rate effect backed out by an order of magnitude with respect to the radiation dose rate to see if you can actually see some kinetics or a threshold for the initiation of the radiolysis effects?

PULVIRENTI: The radiolysis experiments are hot off the press. This was--again, they're very, very preliminary. It was sort of a put them in just to see. So, no, we haven't done additional ones. Additional ones are planned.

BULLEN: Okay. Well, I guess the suggestion I'd make is that you try and take a look at the rate effects, because we know radiation goes away as the packages age. And sort of the peak dose rate that you hear in the Program is about 1700 rads per hour.

PULVIRENTI: Right.

BULLEN: Which is about two orders of magnitude less, and it can drop off to as low as, you know, a couple hundred rads per hour. So you're three orders of magnitude less than where you are.

PULVIRENTI: This is true.

BULLEN: So, it would be nice to know where those rates are.

PULVIRENTI: Uh-huh.

BULLEN: Okay.

CRAIG: April, thank you very much.

We now have our last talk before the break, and this is Roger Staehle, who has been before the Board. Roger, you're listed for 15 minutes, and I'll warn you after, how about 12?

STAEHLE: You've no doubt had my pass-out, so I won't-- I'm going to leave out quite a bit of the pass-out because I think it's imminently readable.

Let me start, first of all, by saying something about how one goes about predicting, because that's relevant here. The issues in prediction in this case are the following. First of all, you need to define the conditions at the surface, temperature, chemistry, stress and metallurgy. That has to be defined in order to make a prediction.

Then you have to know something about these deposits. And, finally, you have to know something about the modes by which failure can occur. So, those are the main ideas that are required in order to make some kind of a prediction.

Now, the framework that we're all in here is essentially the same problem that the commercial nuclear group has in barriers. In commercial nuclear, there are four barriers. And, furthermore, these devices are relatively extensively inspected and monitored. Here, there are nominally also four barriers, the mountain, the container, the cladding and the fuel. And the reason you think about four barriers is because you're not really sure of how this is all going to work. I mean, despite the best efforts of people in here, many of whom are very good friends of mine, and are wonderful scientists, you still can't make really precise predictions. And, so you develop sets of barriers to deal with these complex problems.

Now, I'm going to talk then mainly about the issues of temperature and chemistry and deposits in thinking about how one goes about making predictions, and where we seem to stand.

Now, this slide that's in your pass-out, Page 14, simply gives you an agenda for thinking, but I'm not going to talk about it because we don't really have a lot of time here. But I would like to start temperature by recognizing that if you have no ventilation, that you have a temperature picture which starts off relatively high, and over certainly a period of 50, maybe to 400 years, the temperature on the outside, the container OD, stays relatively high. And, of course, the temperature of the fuel is higher.

Now, why do I show this? I show this because it's my opinion that nobody will stand for ventilating this site. I think this site, when you think about ventilation and you think about the politics of ventilation, that nobody will accept ventilation. So, I think we need to rig ourselves to deal with a non-ventilated case.

Now, I mentioned this matter of temperature and the possibility of deposits. There are basically three cases we have to think about. One is a clean surface, which is essentially the present design. The second is naturally settling deposits, which will give you higher surface temperatures, but still have low resistance out the side. Then the case of complete coverage.

I think the most likely design condition is probably this condition, where we're probably looking at a design temperature at the surface on the order of about 150 degrees Centigrade, give or take a few degrees.

Now, the problem that we're dealing with on the surface is very much like a problem that the commercial nuclear industry has had at heat transfer crevices. I've talked to the Board about this before. Within these heat transfer crevices, there's built up a deposit, there's concentration, there's corrosion, and that same set of conditions occurs on the top of a container where there are deposits. And, so, we can learn a great deal from reviewing what's already known about the deposits and the build-up of material in nuclear steam generators.

This is simply a picture of how these things may occur. So, we're looking at concentrated chemistries, i.e. boiling point elevation, heat moving through. We're looking at formation of precipitates, surface environments change with time. We have long-range electrochemical cells which either may be a deep crevice or a lateral crevice, radiolytic things that Dr. Bullen was just mentioning, deposits, water evaporation, human intrusion chemicals, mountain chemicals, and so on. So, that's the picture of the problem.

Now, let me deal with the problem in terms of how you actually think about it. How do you design experiments, how do you do design? Well, I think there's essentially two inputs here. One input is the chemistry to the surface, essentially the flux to the surface, the mountain average chemistry, the pore water, the water down the fracture surfaces, the microbial/fungal, and human intrusion. That's the flux to the surface.

Now, at the surface, you have a set of dynamic processes which are these: boiling and evaporation, evaporation of volatile species, long-range cells, precipitation, including retrograde processes, radiolytic processes, and then gradients in concentration, wetness, temperature and synergy.

Now, out of that process of an incoming flux, and this set of modification processes, I think ultimately gives you an unboundable chemical situation at the surface. I think when you consider the fact, for example, that in steam generators, people still haven't figured out what the steam generator chemistry is, and with a system this complex, it's not clear to me how it can do better than that, at least in the 30 years people have been working on steam generators.

So, I'd like to exemplify this problem, this conundrum that we've got, by identifying the fact that in this system we're discussing, we have a wide range of chemical species, a range of electrochemical potentials, ranges of concentrations, ranges of wetness, and ranges of synergy. But the domains for the occurrence of pitting, the occurrence of cracking, and the occurrence of other species are relatively small.

For example, most stress corrosion domains are on the order of 200 to 300 millivolts wide, or several pH units wide. What that means is that with this array of conditions, the possibility of hitting a domain of destruction is very high. And I think we need to think about this broader picture of the domains that are in front of us.

Now, I'd like to say a word about the metallurgy, not much more than a word, but a word or two. Metallurgy of C-22, we have this molybdenum and tungsten problem that Professor Sagüés mentioned. It's a multiphase alloy. We have a stress relief heat treatment that will put us in the middle of a range that causes major metallurgical change. We have a giant heat to heat variability problem, and arrays of chemistries at grain boundaries which have yet to have been identified, or thought about--maybe thought about.

Now, I want to point out very quickly some results on Alloy 600 from the primary and secondary side of steam generators. These are data which you'll have to look at in detail because I don't have time to talk about it. But in a given steam generator, this is a French steam generator, 41 heats, and what these data tell you is that some heats don't crack at all, and some heats crack a lot. So, you can imagine that in this repository, there are going to be "N" number of heats, where "N" is a very large number. And the variability of the response of the heats to corrosive environments will look just like this.

There are other data for other heats that have the same problem. You will not have a simple--well, there won't be a mono heat here. We're looking at enormous problems in variability.

Now, the next step I mentioned in thinking about design was the problem of knowing how the corrosion is going to occur, that is, the modes. And Joe, Dr. Payer, he's a good friend of mind, I think still is, has mentioned a number of these and I won't reiterate this in detail. But when we think about identifying the domains of corrosion, we need to think about the domains and the range of design, and those domains are certainly in the range of 150 minimum, up to maybe 250 maximum, where you have the central influence of a larger coverage.

The chemistries are affected by inputs of mountain average, as I showed the flux coming down, modifications, as

I showed the problem, the lateral problem, and of course the metallurgy. And, so, when we think about identifying modes, let's look at history. In 1962, Alloy 600 had only one identified mode of corrosion, and that was done in an obscure French laboratory by somebody who nobody knew, and showed that in pure water, Alloy 600 cracked.

Well, it bamboozled the entire technical public at the time. It doesn't bamboozle them anymore because they've had to shut down a lot of steam generators for that reason. In 35 years, or 39 years, this array of modes of corrosion on that alloy have developed.

In addition to these--and I won't discuss the details of how we've developed this. In addition to those major submodes of cracking, there are another seven minor submodes of cracking which have been identified and quantitatively studied.

Now, let's look at C-22 and see where it is. C-22 is sort of where Inconel 600 was 38 years ago. So far, and this is, again, the nickel diagram, C-22 is about where Alloy 600 was. There are, so far as I can tell, about three identified occurrences of cracking in this alloy in the general range of the deaerated condition, which is along the hydrogen line. And, so, will this same array of modes in 30 years look like Inconel 600? And I think we have to think about that.

Now, one of the reasons for specifying temperatures and specifying chemistries is to conduct accelerated tests so we can move out to 100 years and 200 years and 1000 years. How do you do that? You have to start from a base point. Well, what's your base point? We don't have a base point. We don't have a base temperature, we don't have a base chemistry, we don't even have a base metallurgy, aside from C-22, but that's not saying very much.

How do you conduct accelerated tests? In general or frequently use temperature? What I have here are a set of lines for surface temperature of 100 C., 160 C. to be predicted at 1000 years. As a function of activation energy, this is the time required for the test.

Now, simply what this tells you, it's just a calculation made several weeks ago before Professor Payer's meeting, if you're at a high activation energy system like stress corrosion cracking, the times you need for testing are on the order of a year. However, if you're in the range of maybe five to 20 kilocalories, like what, pitting and general corrosion, you may never run a test long enough to check it out. That's because of the low activation energy.

Now, another problem with accelerated testing I want to point out, and this is, again, just a little bit too quick, this is a Weibull plot, probability versus time. This is, in general, what a field behavior situation looks like at a Weibull beta of 1. Accelerated testing generally has much steeper Weibull slopes, and so whereas this gives you an accelerated test by a factor of 100 of this field experiment essentially at the mean, at 10 to the minus 3 probability, there's no acceleration at all.

And, so, when you run accelerated tests, it's terribly important to understand the fact that the nature of the inherent dispersion of the data has a big effect on whether you can predict anything or not.

Now, let's suppose we're conducting a set of tests to model heat to heat variation. Here's a set of tests on crack growth rate for BWR type applications. These are, what, five orders of magnitude of variability, of crack growth rate heat to heat. Now, these data down here have been censored a little bit. Censoring in a sense is taking out the data you don't like. That's not totally fair to how this was done. And the point is if we were to run this many tests on C-22 for any corrosion situation, we would see a similar dispersion. And I have another slide that you can look at having to do with a magnesium chloride environment. You can look at it for yourselves.

Now, another issue in this program in my mind has to do with the assumptions of design. What do we assume intuitively, innately, somehow we assume. Now, I'm not going to talk about these in detail, but my point here is I've read about 2500 documents in the last months on this problem. I've got a few more to do yet. But when I read this, I say what are the assumptions people are making about design. So, I made a list of these assumptions, and I think at some point, the Board has to come to grips with the idea that there are assumptions here that need challenging.

First of all, what are the assumptions? And second of all, shouldn't they be challenged? So, I made a list of these, and you can look at them perhaps later.

Now, to conclude, first of all, with respect to temperatures, most of the work today has been done around 95 centigrade, some a little bit higher, as Dr. Gordon mentioned. But I think that the nominal design temperature for this system in a non-ventilated case, and even with a ventilated case with a deposit over the surface, has got to be around 150 centigrade.

If you take all the data that exists today at 95 centigrade, it doesn't help you at all at this point. In fact, if you forgot about all the data at 95 centigrade, it wouldn't make any difference, because it doesn't tell you anything about 150 centigrade.

Second, the present chemistries that are being considered for corrosion testing are essentially tied up in pore water and J-13, and there's nothing wrong with that, but in view of this flux of chemistries and the array of things that can change at the surface, it's my opinion that there are no chemistries being used in testing that are relevant to container performance.

Now, how do you prove that? Well, we've got three days to discuss this. We could do that. But I think it's a serious problem. It's not even clear to me that the surface chemistries can be bounded. I think you can take good shots at them. There's a lot of smart people here, and as I say, I know most of them, and I know they're really quite intelligent people, but it's still, nonetheless, a formidable problem.

People have assumed that stress is negligible. That's for good reason. I think that when you start thinking about how do you heat treat to make negligible stresses, you will make structures with metal that you can't stand.

Next, the present C-22 metallurgy is not easily defined because of it's multiphase nature. And I think we haven't paid enough attention to the metallurgy problem.

Now, none of the modes of corrosion have been defined in the general range of temperatures and chemistry expected on heated surfaces. There are no data.

Now, since there are presently no defined temperatures nor chemistries that are relevant, there have been no accelerated tests, and the time required for such tests hasn't been allocated.

Now, with respect to the barriers, we know that the mountain doesn't provide a significant barrier. That's why we're using C-22. We just discussed the problem with C-22. Wherever chemistry moves through C-22 has not been exposed on the fuel cladding, nor has been tried out on the fuel.

I mentioned a few points on the assumptions, and I won't repeat that. And, finally, on my conclusions, I'll just say the following: that there are no apparent design bases for the corrosion related design of the containers. Such bases should consider surface temperatures, coverages by deposits, bounds of surface chemistry, stress and metallurgies. Without such design bases, it's not possible to conduct accelerated tests, nor is it possible to develop meaningful statistical distributions for failure.

CRAIG: Okay. Well, I think you gained us a couple of days worth of questions, but given our time pressure, who wants to ask the one question?

BULLEN: Bullen, Board.

Actually, Roger, I've got a couple of quick questions. Invariably, we run into the heat to heat variability issue of the French steam generators, and I wanted to ask just two quick questions about that. First off, part of this was a problem of not complete through-wall rolling; is that not correct? If you do away with the crevice, part of the problem goes away?

STAEHLE: That's all heat transfer crevice stuff.

BULLEN: Right. And I guess the question that I have is in subsequent data to this, have they been able to tighten up the characteristics of the heat to heat variability? Are the problems still as prevalent as you expect to see here?

STAEHLE: That's the same problem in stainless steels and BWRs in the early days. It's the same problem for stainless steel and magnesium chloride. The heat to heat problem is a big issue, even on homogeneous alloys.

BULLEN: Okay, thank you.

CRAIG: Alberto Sagüés?

SAGÜÉS: Actually, this is a little bit of a more general nature. You have a number of statements in the printout, there is no basis for this, there is no basis for that, and so on. Well, in a sense that is correct if one refers to it by saying there is uncertainty about this, there's uncertainty about the other, because you're never going to have a complete basis to bound anything, even for systems--because you're going to run into surprises. So, I would say that those statements are a little bit hyperbolic. Am I right in that?

STAEHLE: No, I think that, you know, my duty to you and to other people is to tell you first of all, I think, and to put it in a perspective that you can deal with. And I do the same thing for all the companies I consult for. They need to know as close to the truth as I can get. And when someone says can you predict this, there's no basis--there's a lot of difference between 150 centigrade, especially an activation energy of 40 kilocalories, and 95 centigrade. And a lot of things happen in that domain.

Further, on this chemistry issue, I've studied this heat transfer chemistry a lot, and one of the things we know, even a well defined system like a steam generator, where the water and the ambient is well defined, the reproducability and what happens in a crevice still defies the efforts of the best people I know to figure out what it is. And so how can we, in a thing like this mountain, be better than that, especially only after 30 years?

I'm concerned that we have not faced up to the realities of the problem. I think that we are talking about things which we would like to have happen, but not things that are real engineering serious problems. I don't think this is overstated at all. In fact, if I wanted to overstate it, I know how to overstate things, but that's not my obligation to you or anybody else. But I think you need to pay attention to these things.

SAGÜÉS: Thanks for clarifying your statements.

CRAIG: You did make reference to the Payer list of research needs. You made reference to the Payer list of research needs that are emerging from his review. Do you think that's a comprehensive list?

STAEHLE: Well, I think it's an interesting list. And Joe didn't answer some things directly either, but I think that there's some issues here that need to be thought about. One issue certainly is the problem of the deposit surface and the build-up of chemistry and temperature, and the processes at that surface. That is probably the single biggest issue in this design.

And I know there's some interesting work. I know that Greg Gdowski, I don't know whether he's here or not, but is aware of this and thinking about it, and that's not to say that people aren't thinking about it. But I think that's the single biggest issue.

The second biggest issue is the lack of information on the dependencies of the corrosion modes over the array of chemistries and temperatures that are reasonably definable.

CRAIG: Roger, thank you very, very much.

We now take a 15 minute break. It's now 3:20. Please return by 3:35.

(Whereupon, a brief recess was taken.)

CRAIG: Okay, we are now going to hear from a person I'm absolutely overjoyed to introduce. Jim Blink is actually a graduate of my very own department at U.C. Davis, although we never knew each other, but he was a student there.

But, nevertheless--nevertheless, I think it's truly wonderful. It's a good department, and we're proud to have you as an alum, and you are now allocated 30 minutes, and I'll warn you after about 25, or so.

BLINK: Thanks, Paul. We'll see if I really was from the same school when we get to the Q and A; right?

CRAIG: Yeah. Yeah.

BLINK: I did have the fortune at lunch, the fortune cookie, and it said my fondest dream will be realized. So, I guess that's being here right now.

What I've been asked to do is to compare the results of the high temperature operating mode and the low temperature operating mode at the process model level. So, I'll walk through the various process models, starting with the UZ, and working my way all the way up to EBS transport.

This is an adaptation of a talk that I gave to a panel of the TRB a month or so ago. But there have been a few updates to the slides.

So, those are the subsystems that I'll try to work my way through. There's one more at the end that I don't have a slide for, and that's the UZ transport, and I may do that if there's time at the end.

The first one is thermal seepage. There's two aspects of thermal seepage that are important. One is how much--what fraction of the repository footprint experiences seepage, and that's shown by the pie charts, and you can see that the high temperature operating mode and the low temperature operating mode both have about the same fraction, about half, thus, considerably different than in TSPA-SR, which had about 15 per cent.

The other aspect is where it seeps. What is the volume of the seepage? And that's shown in the left chart, and you can see that other than for the very early times, that it almost identical. The difference at the very early times is because we close one repository operating mode at 50 years, and the other one at 300 years, and so you're just really seeing the difference in the closure time there.

The low temperature operating mode seepage is based on the TSPA model, or it's similar to the TSPA model, an ambient model. Whereas, in the high temperature operating mode, the process model gave a lower result than the TSPA model, and that gave a lower result still than the ambient model.

Thermal hydrology will be the area that I spend most of my time in, because I'm trying to compare thermal operating modes. This is a chart that was inspired by Rick Craun of the DOE, who kept asking us are the models that we use for the high temperature operating mode suitable as is to analyze the lower temperature operating mode. And, so, what we did is we took the lower temperature operating mode temperatures, and we just started to shift them in time arbitrarily to see if they would overlap the temperature history of the high temperature operating mode.

And this is a little complicated now. The red is the high temperature operating mode unshifted, and these three curves that are nearly next to each other are three different shifts of the low temperature operating mode. The green curves are what I showed to the last Board meeting, the Panel meeting, which was the low temperature operating mode results at that time.

In one case, I shifted to match the peak to fall onto the high temperature, and in the other case, I shifted to match the majority and let the peak over shoot a little bit.

After that discussion with the Board, we found in our normal quality assurance process, we found an error in the implementation of the thermal hydrologic model. And when we went back and assessed the impact of the error, we generated the blue curve. So, the blue curve is our current understanding, and we published that in ICN-1 of the SSPA document. So, the control document that you have has the current results for thermal hydrology in it.

So, just focusing on the blue curve, you can see that with a shift of about 2600 years, the temperature profiles are almost identical for a typical waste package and location in the repository.

What does that mean? Well, in this portion of the high temperature operating mode, if there are no permanent changes in either the natural system or the engineered system, then we are starting off with the low temperature mode at about where we would be at 2600 years in the high temperature mode.

So, the question to keep in mind is are there permanent changes? And some of the slides that I'll go through later will try to address that question.

The other point is out in here, these models are the same, the same model for the high temperature operating mode beyond a couple thousand years is perfectly suitable for the low temperature operating mode starting from day one.

So, to get to some of the details, I'm going to follow the same practice that I did in the previous slide. High temperature operating mode is red. Low temperature operating mode, the original charts that duplicate what I showed you a few months ago are green, and blue is our current understanding. That will let you contrast visually the difference between the operating modes, and also the small shift that was caused by the correction of the error in the calculation.

There's a band here, and the band is the variability within the repository. And I'm showing the variability in two respects. The variability here is the variability across the footprint, with the highest temperatures being near the center of the repository, which is thermally insulated from the edges, and can only have one dimensional heat transfer for quite some time, vertical heat transfer, and the lowest temperatures are near the corners of the repository. And, actually, if you look at the specific past, the temperature histories rather than the band, there are really three sets of curves. There's the corner, the center, and then there's ones near the edge where you basically have one, two or three dimensional heat transfer.

This variability is the variability in one location of the repository amongst the various types of waste packages. Waste packages range from 11.8 kilowatts of peak power, peak thermal power, down to just a few hundred watts of peak power when they're put into the repository. So, there's a variation amongst those packages at any given location.

You can see that the difference between the two modes is of the order of 90 degrees, and the range of variability for either mode is around 20 degrees, depending on what time you look at the curves.

This shows the exact same information, but now I'm plotting the relative humidity histories rather than the temperature histories. The high temperature gives you a depression in the relative humidity at those early times, followed by an increase back towards the ambient, which is very near 100 per cent underground.

A couple of interesting things about this. We were very curious as to why the original low temperature operating mode results had a humidity depression that persisted for so long compared to the high temperature operating mode. And that question was asked I think three times at the last Board meeting, and the hypothesis that we had was at lower temperatures, the radiation heat transport is less effective.

That hypothesis is true, but it wasn't the major effect. The major effect was actually the error in the calculation that left out a significant part of the radiation. And it was a very simple typographical error in an input file that caused it.

Now you can see that the humidity bands overlie each other, and most of that question has gone away. But it's interesting if you look at this band closely, right there in the blue, there's a cross-over point. And at earlier times for the low temperature operating mode, you have more humidity depression at the center of the repository, where the temperatures are higher, and at the later times, you have more humidity depression at the corners of the repository, due to that hypothesis that we gave you the last time that the overall efficiency of radiation heat transfer is less at the lower temperatures.

Another interesting thing about this is when we look at the variability amongst the waste package types, the variability is much stronger at those early times after closure for the lower temperature operating mode. And this is basically the--this high part here with the lower depression of relative humidity, those are the defense high-level waste packages, the ones that only put out a few hundred watts, up to a few kilowatts, at emplacement.

This shows the same information, but in a different way. What I've done is plotted the footprint of the repository at four different times, near closure, near the time of peak temperature, at 2000 years and at 10,000 years, and I've used the same temperature scales for each of those points so that you could directly compare colors between the high temperature and low temperature operating modes.

And you can see that, of course, at the early times, the high temperature operating mode is much warmer than the low. That's obvious. But as time goes on, these things converge back to the same sorts of distributions. So, not only is the temperature history for a typical waste package very similar at later times, the spatial distribution and variability across the footprint also becomes similar.

This is the same information, or the same format, but this is for the relative humidity distributions. And, again, you can see that we converged at the same sorts of distributions, and also you can see the strong influence of some of the natural systems variability on the results. In this case, it's the influence of the infiltration maps that are produced by the UZ process model.

This chart shows you in CCDF format, a fraction of waste packages that are above or below a certain temperature or humidity or time which a temperature/humidity combination occurs. For a moment let's ignore the green curves which were the low temperature operating mode ones that are superseded, and just compare the red and the blue.

You can see that the distributions, the range from this end to this end, are similar for the two, and this little notch here for the low temperature operating mode is the one I told you about earlier, the defense high-level waste packages, which don't have as much humidity depression.

This shows you the time at which that relative humidity of 80 per cent is attained, or the temperature at that time, and there is a little bit more difference now between the high temperature and the low temperature operating mode, but that's to be expected, the high temperature operating mode being able to dry things out a little longer.

This is the overall result of the sensitivity runs that we did, and all of these were, in essence, one off runs. The zero on all of these scales is the temperature that we calculated, minus that for the base case. So, that is the temperature recalculated, minus the base case. So, zero is just simply the base case.

So, if you look at these, the base case for lithophysal porosity was 12 1/2 per cent for the lower lithophysal unit. So, there's a zero there for both the high temperature and the low temperature operating modes.

Then we varied the lithophysal porosity from zero per cent to 25 per cent, which is the range over which the mapping of the ECRB has produced. So that's the variability that we've seen in the limited area that we've characterized, and we assumed that the entire unit had those values, which wasn't a fair way to do it, but it was okay to get a general idea of the sensitivity.

And you can see for the high temperature operating mode, the sensitivity is very large, near 100 degrees, 90 or 100 degrees of potential variation if the entire unit had those properties. Now, it's actually going to be variable across some spatial scale, and due to the significance of the result, we will have to do more detailed sensitivity studies.

You can see the low temperature operating mode isn't nearly as sensitive, about 25 degrees C. spread. You can also see that a low lithophysal porosity doesn't gain you as much temperature reduction as a high lithophysal porosity costs you in temperature increase. So, that means it's important for us to find out where there is high lithophysal porosity. We may be able to make some engineering adjustments to waste package spacing, for example, to compensate for that if we mapped as we went.

Thermal conductivity is largely--is the largest factor that's sensitive to the lithophysal porosity, so its results are almost the same. The other thing that's sensitive to the lithophysal porosity is the specific heat. But that's not as big of a factor.

Most of the other factors had quite a small impact. It didn't really matter much when we varied things over the extremes that we've observed, or believe are possible.

There's a couple of things that I'd like to call to your attention, though. One is the ventilation efficiency. We varied that efficiency plus or minus 10 per cent from our base case, and we didn't see all that much effect, of the order of 20 to 20 degrees, depending on which operating mode we were in.

The other thing on ventilation that's important is the ventilation efficiency that we calculate is a time dependent function, with a low efficiency at early times, and then an increase in efficiency as time goes on.

The way that we do our standard calculations for the process model, which is then abstracted for PA, is we integrate that time dependent function, and then we reduce the energy in the waste packages by that fraction. And we do that uniformly in time. We lose that time dependence in the process.

So, we decided for the sensitivity study to go back and look at that assumption and see what effect it had. And this is the only bar on the chart that's not the post-closure temperature, peak temperature, but rather this one is the pre-closure peak temperature as the figure of merit. And you can see that for both the high and the low temperature operating mode, it's quite significant, 60 or 70 degrees of additional temperature in the pre-closure if you put in the time dependence. So, that was a non-conservative assumption we had with respect to temperature, and now we'll have to go back and probably in our next round of calculations, start doing things with some time dependence, or at least we'll have to look very hard at whether we've got the time dependence proper.

Now, for the high temperature operating mode, it didn't matter in that the peak pre-closure temperature was still less than the peak post-closure temperature, except if we started out with a high temperature operating mode and wanted to preserve the capability to go to low and never go above boiling, of course it could put that option at risk. But for the low temperature operating mode it was very important, because the pre-closure peak in a few decades after emplacement was above boiling. So, we have to go back and look at this some more and work it some more.

One last thing on this slide is I've shown three ways here to get to a lower temperature operating mode. The base case for SSPA was to spread out the waste packages. But you could just as easily spread out the drifts if you decided ahead of time, or you could just as easily reduce the capacity of some of the hotter waste packages, drop them down to, say, 16 PWR assemblies, while the cooler packages, the BWR packages and the glass packages, could stay at full capacity. Those three methods gave this range of peak temperatures, not a very big difference. And what that tells me is it probably doesn't matter as much which way we choose to get to low temperature operating mode, we can use other factors, such as worker safety or even cost to influence that decision.

Moving on now to thermal hydrological mechanical coupling, this is an example of a continuum calculation that was done by the UZ Department, and it shows you the change in permeability of the rock due to thermal mechanical processes for the high and the low temperature operating mode. And you can see visually these are very similar. They don't have much difference.

This shows you the THC, the thermal hydrological chemical processes, and I'm showing you the carbon dioxide, the pH, the chloride and the fluoride in the water as a function of time, in the water or the gas. And you can see that things are fairly similar, except for the high temperature operating mode has this period of either a big depression or no data because there's no water there.

This is an abstraction of the previous slide for the CO2 and the pH, where in order to make the PA calculations more tractable, we divide our time into specific periods, and we average within those periods. It's showing you the same information, but in a more grainy fashion, and that's the way that it's carried forward into the PA, an example of simplification.

This one shows you the evaporation rate out of the invert. The invert gets water into it by seepage, by condensation on various interior parts of the drift, but mostly it gets it by imbibition, by pulling it out of the host rock. In this case, we have a crushed tuff invert, and that crushed tuff will pull water from the host rock.

And what you can see, this one is a little hard, but the blue bands and the red bands are fairly similar. The evaporation rate gets to a reasonably high level, but it's about the same for both operating modes. So, the additional ventilation and the delay of closure for the low temperature operating mode compensated for the smaller footprint.

The other thing that I'd like to call to your attention is in the red curve, it looks like it's a double hump peak. That's not actually the case. If you plot all of the curves for all of the waste package locations, those curves crisscross and overlap each other. Those peaks are not the same history. There's an early set of peaks for some waste packages, and a later set of peaks for other waste packages.

This shows you what happens to the pH as you evaporate or condense water. This scale on the X axis is the degree of condensation or dilution, and the degree of evaporation or concentration. The red curves are the high temperature operating mode, the blue curves are the low temperature operating mode at different points in time. Remember, we abstracted that previous slide on the chemistry, so those are just the curves as a function of concentration for those various times.

And the point to be taken from this slide is in a couple of the time periods, ranging from about 50 years to 4000 years, the high temperature operating mode can run down to much lower pHs, around 5, than the low temperature operating mode. So, there's more variability in time for the high temperature operating mode, depending again on how much evaporation you've got, which in turn depends on the specific local conditions where it's taking place.

Moving on now to corrosion, and June just told me that this work should be potentiodynamic, not potentiostatic. So if any of you want to make that correction to your handouts, please do.

This shows you the temperature dependent general corrosion rate for the Alloy 22. Now, it's based on a pinning to the 60 degree data from the Long Term Corrosion Test Facility at Livermore, and then the temperature dependence is put on top of that from the potentiodynamic polarization measurements, which were done quickly in an accelerated environment in order to get enough data to be able to discern the temperature dependence.

We think that ultimately, the Long Term Corrosion Test Facility experiments will also give us the temperature dependence and the chemistry dependence, but right now, the corrosion rate and the total amount of corrosion to date is so low that the scatter in the data overwhelms those dependencies, and you can't see them because of the scatter. But as time goes on and the total numbers increase, there's a chance we'll be able to pick some of that out.

In the meantime, we went to an accelerated approach to get the temperature dependence. Now, when we did this, I think a lot of us thought intuitively that the high temperature operating mode would suffer from that comparison because of these much higher numbers at the higher temperatures. But what it actually turned out is the overall corrosion in the problem got much less, because the problem is dominated by the very long periods where you're at temperatures lower than 60 degrees.

So, when you look at the total dose curves, a lot of that shift to the right, that delay in time, is due to the influence of this temperature dependent corrosion rate.

But now let's look at it from a comparison viewpoint. Let's take that curve and multiply that curve by the temperature versus time curves that I showed you earlier, and then integrate them. And that's what this shows you. The red and the blue are using the median, and the dots are the ranges shown there, which are 25 and 75th percentile. I could draw bigger extremes if I choose to, but this is good enough for illustration.

And what you see is in the first tic mark of this integral, the first 25,000 years, you have a jump, and you corrode a few millimeters of the 20 millimeters total. This 20 millimeter scale is the total thickness of the Alloy 22. And the difference between the two modes is about 1 millimeter at the end of that thermal period.

After that, they're corroding at the same rate, because the temperature history is the same. Remember that first slide I showed you. And the difference is about 1 millimeter. So, remember the slide when I said 2600 years of time shift, it's valid to think about comparing the modes if there weren't any permanent changes. This is the permanent change in the engineered system, is we've had about 5 per cent of the thickness of the Alloy 22 has been compromised by the higher temperature mode compared to the lower temperature mode, not a very large difference.

I already showed you the chemistries and the THM effects, and they weren't very large on the natural system. I didn't go into it, but in the THC, the thermal hydrological chemistry, I believe that the calculations that Eric Sonenthal has done in the UZ Department show about 1 per cent of the 1 per cent fracture porosity, that is, 10 to the minus 4 of the total volume, being filled with precipitants. And, so, that's not a very large effect for either of the modes. It probably wouldn't change the water flow very much in the mountain.

This shows the result of applying those general corrosion rates with the sampling and the spreads, and this shows the failure of the waste packages. The early foot was an early failure. We took another look at the reliability data, and came up with about one-fourth of our realizations had one waste package fail early due to the combination of unlikely effects of human error and computer error. Then this rise here is due to the general corrosion of the other waste packages.

I should also point out that the general corrosion rates that's inherent in this also has some sampling and some weighting factors for microbiologically induced corrosion, Mrs. Devlin. And also for aging at the corrosion weld.

We've heard a lot about localized corrosion, and I've tried to synthesize the localized corrosion into one graph. This one is a little tough to follow, but I'll try to get through it in the couple minutes that I've got left.

Here is the temperature on the X axis, and the humidity on the right axis. And when you close the repository, the temperature rapidly increases in the red for the high temperature operating mode, or to the blue for the low temperature operating mode. And the range on those is due to the range across the waste packages and across the footprint. In this case, just across the footprint is shown.

Then it progresses back up this pathway in time until eventually it gets back to ambient temperature, around 25, 26 degrees C., and ambient humidity, around 100 per cent humidity. I've given you a couple of weigh stations along here, 600 years and 3000 years, for two different locations in the footprint for the high temperature operating mode.

This band here shows the window of susceptibility for initiation of crevice corrosion. The bottom of the bank is the deliquescence curve for magnesium chloride, and the bottom of this smaller window is the deliquescence curve for the sodium chloride. The right side is the thermodynamic condition that we can't pressure the drift, and the left side is the lower temperature limit for crevice corrosion initiation, which is in the 85 to 90 degrees C. band, based on experiments and some limited modeling.

The point here is the high temperature operating mode goes through the window of susceptibility, yet in the TSPA, we don't have any crevice corrosion, and the reason is there's actually one more axis for this graph, which I haven't been able to figure out a way to plot it. It's already complicated enough. And that axis is chemistry, and it turns out that we never, over the entire pH range, we never quite get to an aggressive enough condition to start crevice corrosion.

So, for the high temperature operating mode, we didn't see it, and for the low temperature operating mode, we didn't see it. But we can make the argument that it didn't occur for the low temperature operating mode on pure thermal considerations, the blue never getting into that window. The argument is a little more difficult to make for the high temperature operating mode.

Waste form. This shows you the solubility of Neptunium as a function of temperature. I think Dr. Boyle showed you that one earlier with datapoints on it. And this one shows you the clad unzipping rate for the two modes.

Basically, there's some difference in the Neptunium solubility. There's enough of a temperature difference in the neptunium solubility to make a small amount of difference between the operating modes. But the actual difference between the base case, the old TSPA, and the SSPA in the reevaluation of the solubility data, was a bigger effect than the difference between the operating modes.

All these other factors on the side are factors that we looked at but didn't have a significant effect on the performance.

Engineered barrier system transport. For the SSPA, we put sorption coefficients for the ballast material in the invert to see if there was a significant delay caused by the sorption of some of the radionuclides in the invert.

First of all, you can see that the delay is only a matter of several centuries, so maybe it's not enough to worry about. And these diffusion coefficients that we used also were temperature dependent, and you can see that there was a fairly large difference near the peak temperature time. But when you put that together, you still don't have a whole lot of difference between the two cases.

So, although there is a temperature effect here, it's probably lost in the noise of the overall TSPA.

This goes a little further. This is the sorption now rather than the diffusion in the invert, and this is the result of a one-off study that was in Chapter 3 of Volume 2 of the SSPA, where they just made that one effect, sorption or no sorption. So, they didn't make any of the other changes in this model that are shown in Volume 1 of the SSPA, and you can see the difference in the dose curves. There is a difference, but it's not a large difference. It's mainly a shift in time.

So, the conclusions of looking at this at the process level is, first of all, the process level uncertainty is reduced for some of the subsystems for the low temperature operating mode. And we have propagated some of those uncertainty reductions into the TSPA, but not all of them. So, when you look at the TSPA differences between the two, you're only seeing part of what we discovered at the process level, and that's largely because we proceeded in parallel with the two volumes.

The thermal conductivity uncertainty and variability, and those are two separate things. There's an uncertainty about the thermal conductivity at any one location, and then there's also a variability amongst the locations. That's the most important effect for temperature, so that's the one that we need to work on.

Our results suggested the differences between the operating modes are really only significant for the first few thousand years. And beyond that, the differences are not large. Dr. Boyle showed you the horsetail curves and the ranges between the threads and the horsetail curves were much larger than the differences between the means or the bounds between the high temperature and low temperature operating modes.

So, the overall conclusion is those analyses that are documented in Volume 1 of the SSPA are a reasonable comparison of the subsystem behavior, or basis for decisions by the DOE on how to proceed towards the SR and potential LA.

CRAIG: Okay, thank you, Jim.

Let's see, I've got Alberto and Priscilla, and I want to throw in my own, if I'm allowed to, or even if I'm not.

If you'd turn to Number 16, this is the data that relates to the activation determination for the temperature dependence of the corrosion, and that's driven by a rather small number of datapoints taken by Scully at 80, 85 and 95 degrees, and a small range, and at 95 degrees, there are only eight datapoints. One was an outlier, it was an outlier, but over an order of magnitude, so it really matters. And these are short-term measurements, they're only one hour measurements, and they were all normalized to the dunk tank results, which is a reasonable thing to do.

But the fact that there's an outlier in QA data does at least suggest that maybe one should ask what happens to the activation energy if it's included. And that increases it a lot, from roughly 36 up to about 65 or so kilocalories per mole, which has big implications for the corrosion rates at higher temperatures.

Now, I have no idea whether that outlier is or is not real, and as far as I know, there's no way to decide at this point. But there certainly is a question there as to what happens at the higher temperatures.

What are you planning to do about this kind of issue?

BLINK: I guess I'm going to ask Tammy Summers if she wants to answer that, or Gerry Gordon. But the one thing I would say is that when we did the TSPA-SR, with no temperature dependence of corrosion rate, we all knew that mechanistically there must be some sort of temperature dependence, and we just couldn't see it from the data. So, we sought to find a way to get at that dependence, and we may not have it perfectly, but we know it's a curve something like that, and maybe it's a steeper curve. And obviously if you have the higher activation energy, you're going to see a bigger difference between the operating modes. But it at least gave us a chance to look at the sensitivity.

LONG: I'm Kevin Long from Waste Package Science and Analysis. Indeed, we actually in the Volume 1, we presented I believe two or three different corrosion models. Corrosion Model 1 was using all of the data, including the outlier, and it did result in a slope of about 66 or so. And we ran waste package degradation curves with that. They were not run all the way to dose in Volume 2.

The corrosion at earlier times is much higher at higher temperatures, but also much lower at lower temperatures. And, therefore, the waste package lifetimes--well, very few realizations failed, on the order of 2 or 3 per cent, I believe. And, you know, it resulted in a very--it would result in a very low dose.

So, we had significant--if you read Section, I forget which, of our 7335, I believe, you'll see arguments for why this is an outlier point. It's P value is something around .04, or something. So, it is indeed an outlier. And the fact that the data is based on eight replicate samples even further--one of which was an outlier, even further convinces us that that was indeed an outlier.

CRAIG: Thank you. Alberto?

SAGÜÉS: Okay, very good. I'm glad you brought this transparency up, Paul, with that question. And it's sort of like addressing another issue that may be more general. When one looks at those graphs, and let's forget about the issue of the outlier for a moment and look at the 75th percentile, 25th percentile. I think it's important for all of us to remember where this is coming from. That's not an estimation of the natural scatter that may occur in the system. It is an indication of what the calculations return when you enter a certain number of assumptions into it, one of the assumptions being those eight samples which were not actually exact replicates, because they address different solutions, and so on.

When you take those samples and you make an assumption that that's the same as what you would get when you have an infinite number of samples, and so on, that's what you would get, but, however, if you take say, for example, a different heat of the same model, and maybe you get a different number, or maybe if after some time, one of the many mechanisms that were mentioned at our workshop shows up and causes something to change, and so on, then the curve would be different.

And I find it bothersome to look at the curve on the right where we have an extrapolation to a million years, which is fine if we keep in mind that that curve has the assumption that let's pretend that this and this and this and the other. Otherwise, when you look at the real uncertainty of that, namely how little we know about the way these things are, we would have a trumpet of uncertainty if you will. We will have a curve that will have the horizontal axis, and it will go up eventually orders of magnitude over what it is right now, maybe an order of magnitude, orders of magnitude would be a possibility. Which means that in the context of the specific numerical extrapolation question we have right now, the curve is fine, but only within that context.

I mean, I'm saying all this to see if you view it in the same way, or maybe you think that it has more of a meaning than what I just was mentioning.

BLINK: I guess I have to agree with your point, and maybe add one sentence to it, and that's if the uncertainties of the passive layer surviving for very long times are thermally dependent, then that larger trumpet that you have might be different for the higher temperature than the lower temperature. Since we don't have any idea as to the size of that trumpet for any operating mode, our assumptions in these calculations are that the passive layer persists, and that's clearly an assumption on this chart.

If the passive layer doesn't persist, then a lot of charts change, not just this one.

SAGÜÉS: Right. But if one goes after the question of is it good to go to a lower temperature than to a higher temperature, if you're using this instrument to try to answer that question, then a few things come up that will not show up in that curve. For example, yes, maybe the uncertainty will be lower at lower temperatures.

BLINK: Yes.

SAGÜÉS: And then that could change the message dramatically. But that is not, of course, is not asked in that particular question. And maybe what would happen is that you may reduce the number of unexpected events that could happen, which is unfortunately not quantified in the context of this. Right? That's not quantified?

Okay, I just have one quick question on Figure 11, Transparency 11, and that is what kind of experiments were used to get the data that allowed predicting this decrease in permeability around the drift due to thermal stress induced by decay heat? How was the input to that developed?

BLINK: There are two sets of THM models that have been run on the project. This is the continuum model approach, and there's also been one with discrete blocks, or it's called distinct elements, which are blocks of rock that are bounded by fractures. And the latter model has been used to calculate the repository situation, and has also been used to look at the results in the drift scale test and the large block test, thermal tests that we've done. And the model does a reasonable job at explaining the data from those tests.

So, I guess the way I'd answer the question is we've applied the model for a validation test, and then we've applied the same model to the repository situation.

SAGÜÉS: I see. Is the effect of seismic activity taken into account in this?

BLINK: Not in this one. This is THM. The key block analysis includes normal seismic and temperature effects, and one of our intentions is to take the results of what we've learned from this part, and move it back into the key block analysis. I suspect that some of what we've learned from the probabilistic seismic studies could also be moved back. But that's future work in support of a potential LA.

SAGÜÉS: Okay. All right, thank you.

CRAIG: All right. Priscilla Nelson?

NELSON: Nelson, Board.

I wasn't going to ask this, but since this is up, when you talk about the lower lith. here, are you using continuum matrix properties? I'm confused throughout this where lithophysal effects are included and where they are not. So, let's just look here for thermal mechanical. In this model for the properties and the response of the lower lith., did this include a mass, a matrix based property, somehow was a bulk property evaluated for this case?

BLINK: I don't know the answer to that one.

NELSON: Okay.

BLINK: Jim Houseworth, do you happen to know?

HOUSEWORTH: Jim Houseworth, Lawrence Berkeley. I believe it did include the lithophysal properties.

NELSON: Okay. You might want to hang around then if you're the person who knows the answers to these.

Can you go to Slide Number 10? I think all along we have known that there was these first two variables, variations that you demonstrate there on lithophysal porosity and thermal conductivity were to be anticipated, both being very important effects that need to be modelled very well. And I look here, and first of all, I want to understand the lithophysal porosity surely has something to do with a bulk thermal conductivity, so they're not independent, unless that thermal conductivity is matrix thermal conductivity. Can you tell me what that is there?

BLINK: That is matrix thermal conductivity, and the lithophysal porosity affects both the thermal conductivity and the specific heat. The first set of bars use both effects. The second set of bars only vary the thermal conductivity.

NELSON: What second set of bars? What are you talking about here?

BLINK: The lithophysal porosity bars is this set. When we varied the lithophysal porosity, we automatically changed the thermal conductivity and the heat capacity. In the second set of bars, we only changed the thermal conductivity.

NELSON: Right. And only using matrix thermal conductivity data, or using bulk equivalent thermal conductivity data that includes lithophysal effects?

BLINK: We used the lithophysal porosity to reduce the bulk thermal conductivity.

NELSON: Okay. Which is why you have such a huge spread. Okay. Now, I see that you've looked at this for the base case, or variation from a base case, getting peak temperature. But the plots that were done before about temperature resulting from low temperature and high temperature, they were not done investigating this. They were done only using this base case?

BLINK: They were done with these base numbers, the 12 1/2 per cent, the 1.87, 1.27.

NELSON: Okay. So, it seems pretty clear to me that with these big ranges, that the proper representation of lithophysal porosity and thermal conductivity in the temperature models can really have an awful lot to say about what the temperatures are, and the rest of the study that you report on may be strongly affected by putting in these bulk properties, which has not been done yet.

BLINK: Other than spreading the properties out across the whole stratigraphic unit. The next step would be to take this variability and put it at different spatial scales, with the base case for that being the spatial scale that we saw when we mapped the ECRB, because at small scales, it will tend to average itself out, but at the larger scales, it won't. But one of the things to note from that is the center of mass of that bar is not at the base case.

NELSON: That is right, yes.

BLINK: So, it's clearly an area to be looked at.

NELSON: Okay, we need to move on, but, I mean, I think that this could be a really important impact on the rest of what you said.

BLINK: Agreed.

NELSON: So, it's very important.

CRAIG: Okay, we have three more questions, and we're pushing on time, but we'll get through them if everybody will be brief. Debra, Leon and John Pye.

KNOPMAN: Knopman, Board.

On Slide 4, I want to make sure I get the implication of what you've put down here, because it's I think pretty important. What you're showing here is that all things being equal, to some extent, the low temperature operating mode--rather, the high temperature operating mode at best can equal performance--well, let's not talk about performance. But, at best, replicates the conditions of the low temperature operating mode, but you've got this 25, or how many years, 1000, 1500 years.

BLINK: 2600.

KNOPMAN: I'm sorry. 2600 years when something may happen in your high temperature operating mode that's simply not going to happen in the low temperature operating mode. That is, you've got--it's almost impossible to imagine how you have better performance in a high temperature operating mode when you've got 2600 years of something going on that's not happening in the lower temperature operating mode.

I just want to make sure that that's in a sense what you said, because your models now are matching up. You're matching up your low temperature operating mode to what you had before, starting at 2600 years. So, you have your first 2600 years of things happening, of changes being made. You lose something of your C-22.

Then you're saying--you seem to be concluding that you do also have reductions in uncertainty with the low temperature operating mode, apart from the performance issue. Is that correct?

BLINK: I agree with the second half about the reductions of some of the subsystems uncertainties. But in the first half, the reason for doing the shift was to show you how the performance would be similar at certain times, but it's not like you delayed the low temperature operating mode for 2600 years before anything happens. It's happening in both from the day of emplacement, and then the day of closure. It just may be happening at different rates during those times.

The purpose of showing you the time shift was to show you that the models that we use for the high temperature operating mode represent the low temperature operating mode as well. It's not like we've done all of this work that doesn't support both modes.

NELSON: I understand that. On the subject of, just real quickly, you may want to answer this at another time, but let me just put it on the table. There is a paper that I think is unpublished, but written by a bunch of Lawrence Livermore folks, as well as some from Sandia, entitled Uncertainty, a Discriminator for Above and Below Boiling Repository Design Decisions. And the thrust of the paper is that low temperature designs, a low temperature operating mode in fact could increase uncertainties for certain subsystems.

Have you tested the assertions or hypotheses that are presented here, primarily related to kinetics, and the reduction of uncertainty if you're in high temperature modes and essentially bypassing kinetic modes of chemical change, and just going right into equilibrium conditions? Because I'm just wondering how this fits in with your analysis that you showed us that shows lower--some reductions in uncertainty in some subsystems. So, would the points made in this paper offset the uncertainty reductions in the systems you looked at?

BLINK: I think before I answer that one, I'd better take a close look at that paper to look at the assertions. I'm familiar with some of them, but I'd want to go point by point through it.

KNOPMAN: It's Dale Wilder--

CRAIG: Let's do this one off line. I think it looks like it requires some serious time. Leon Reiter?

REITER: Leon Reiter, Staff.

Jim, I hope that sometime you'd carry through the different temperatures into the TSPA analysis. The reason being is that sometimes there are subtle differences that are important, and sometimes we want to see whether these kind of calculations get rid of those subtle differences, make them larger, or bring in new subtle differences. So, I hope to do that sometime because I think it's important.

BLINK: We did have a method to what we did. We ran the one-offs in Chapter 3 to try to discern which factors had the biggest effect. And then as we did our work in Volume 1, we also attempted to look for things that were important enough to carry forward. But, obviously, we didn't have time to do all of the effects, and there may be some factors or synergistic factors I should look at.

REITER: Right. Just following through on that, one-offs don't tell you everything. The existing SSPA results, there's an interesting pattern there, and I wonder if you'd explain it. If you look at the nominal case, we see essentially low temperature performs better than the high temperature at periods below 10,000 and above 100,000 years. And that can be up to an order of magnitude.

However, when you get to the period between 10 and 100,000 years, for a lot of that time, they're about the same. The dominant radionuclides are Carbon 14, and I think Technetium 99. And when you go to the igneous case, you see the same sort of thing, only this case, before 10,000 years, they're about the same. But after 10,000 years, where the dominant radionuclide is plutonium, the cold has worse performance.

What's going on here? What's the systemmatics? What's causing this.

BLINK: I wouldn't read too much in the differences between those mean curves. I think some of the early effects are probably due to the closure date. 50 years for the high temperature and 300 years for the low temperature just gives the radionuclides a head start in some cases for the early failure packages.

For the longer term ones, there may be some effect of this general corrosion rate during that high temperature pulse for the high temperature operating mode. We had about 1 millimeter, or so, of the Alloy 22 being corroded deeper during the thermal pulse for the high than the low. And I suspect you will see those. But people like Peter Swift tell me never look at the mean curve and try to just judge on that. Look at the individual realizations to try to do that.

So, I guess I'd refer that to Rob or somebody else.

CRAIG: Okay. We have to move on now. Last question is John Pye.

PYE: Pye, Staff.

Can we look at Slide 18, please? Jim, how should we interpret relative humidity at the waste package if it's boiling? You also mentioned briefly in your presentation over pressurization in the drift.

Did you hear my question?

BLINK: Yes, I heard it.

PYE: Okay, I'll repeat it. What is the meaning of relative humidity on the waste package above boiling point? And you mentioned over pressurization, trying to avoid it. Could you explain that in this graph, please?

BLINK: Well, first, let me do the over pressurization. This curve along the right-hand side is the thermodynamic curve at the elevation of the repository horizon, showing you the combinations. If the gas that's above the liquid is 100 per cent water vapor, that's the relative humidity.

So, for example, at this temperature of 130, or so, if you had .89 bars of water vapor, which is the ambient pressure at that elevation, that would be 40 per cent of the saturation pressure of water if you put it into a pressurized system.

Does that help?

PYE: Not really.

CRAIG: Okay, this is one where you two should get together at the end of the session, I think.

Jim, thank you very, very much. A lot of material. We now move into two presentations, both relating to peer reviews. One is on the biosphere, and the second is a TSPA peer review. And the first of these is Eric Smistad, who is the DOE technical lead for biosphere, igneous activity, unsaturated zone and performance confirmation. And you've got 20 minutes, and I'll warn you after about 15.

SMISTAD: Thank you.

The first thing I'd like to say is I brought copies of this report for Board members who don't happen to have this report, for Staff members who don't happen to have it, and anybody else in the audience who's interested in this report, I do have about 350 copies, so don't feel bad about asking me for the report.

I'll go over the review objective we started with, go over the members of the team, terms of review, what we asked the Panel to do for us, how the review was implemented, give a quick one pager on the summary of the results, and then I'll walk through selected recommendations. I'm not going to walk through all of them, but just enough I feel to give you a flavor of what's in the report, and then I'll summarize.

Objective, high-level objective was to provide, on the basis of available international standards and guidance, an independent evaluation of our biosphere methodology on Yucca Mountain.

Peer Review Team. It was a team that was assembled by the International Atomic Energy Agency. It consisted of six members mainly from waste management organizations, national advisory boards, and regulatory bodies. It included an agency secretary and an agency technical person, scientific secretary.

I won't walk through each individual member here, except to say this was a seasoned group. There is a pretty good bio on each person within the report itself. So, if there's any questions out of that, I can field those perhaps later.

Terms of review, what we asked them to do, we initially asked them to review all the documentation we had on the street at the time, and we asked them to consider looking at the characteristics of the biosphere system, the FEPs and how that played into our conceptual models, the appropriateness of the GENII-S code, the code we use in the biosphere, methodology of choosing our receptor, and then looking at data and parameters.

Implementation. They did review all the documentation we had at the time. It was the PMR, 16 AMRs. They looked at both the EPA and the NRC regs, which were draft at the time. They also looked at the TSPA we had out, and the TSPA-VA peer review we had, along with some other miscellaneous documents they asked for.

We had a question and answer exchange, mainly through e-mail based on what they'd read in the documents we sent them, and this took place over August through November of 2000. They then came to the site in November. It was a one week visit. We had interactive presentations and discussions with them for a couple days. We took them out to Yucca Mountain and spent most of the time out in the Valley itself to get them a feel for what is out there. They held some closed discussions themselves.

And then at the end of the week, they summarized orally their results to us. They submitted a draft in January. We reviewed that draft for factual accuracy, dates and names and that sort of thing. And then they put out a final report in April.

Just a quick one page summary. We felt that the review was generally favorable. We thought it focused on efficiencies and enhancements for the most part. There were 23 recommendations. They did a couple different groupings here. The first grouping they did was recommendations they thought that could be carried out within the regulatory context, and ones they felt that were outside of the regulatory context, but still may provide some bolstering or increase in stakeholders' confidence, as they put it. And these numbers, 14 and 9, are just a breakout of the 23.

Then as far as the report goes itself, they did more of a classical chapter grouping. These are the three main groupings that they had in the report: assessment, definition and then data and model itself. And those are just the breakouts of the 23.

As far as the assessment approach went, there's subtopics here, and I'll walk through an example out of each one of these. Again, I'm not going over each recommendation out of each category. Assessment context, regulatory requirements, and then the integration of the biosphere into the TSPA itself.

Assessment context. They really didn't have recommendation in this category itself. They had more comments. They did recognize how we kind of got to where we got to in our biosphere modeling. They understood the regulatory basis that we were working towards.

They felt that the biosphere modeling was a little less mature than the rest of the models that we've been working on through time, and that it appeared to be more of an accessory to the modeling, although they didn't say it was not functioning properly, they just felt it was a little disaggregated.

And then they thought that this separation was enhanced by the regulations themselves. They thought they were a bit prescriptive, and I'll talk a little bit about that in the next few slides.

Regulatory requirements. This was an example that they categorized as outside of the regulatory framework, although you could make an argument that it could indeed be within the regulatory framework. And it went in pathways. They felt that we could have done additional looks on possible pathways that, in their words, could lead to logical extensions for the compliance case.

And we are, the response here, we are looking at additional pathways right now. We'll look at some more next year. Two pathways that we're concentrating on at this point are inhalation and ingestion by animals, cows for instance, and the pathway that occurs there, either milk or perhaps liver.

And the other one we're looking at is a pathway that swamp coolers may introduce, and this is also something that the NRC had brought up in one of our meetings as well.

Integration of the biosphere into the TSPA. Again, this is something I'd mentioned just a few slides ago. They felt that there might should be something done to better integrate TSPA and the biosphere together. And we're looking into this. The first thing that we're doing, and we're starting it now, is looking at a meshing of the GENII code we use in the biosphere with the Total System Performance code, which is the GoldSim, and we've done a similar thing with the flow and transport with GoldSim as well. So, that's one of the things we're looking at to try to integrate better and make it more of kind of a seamless integration.

This is something that would also help in sensitivity analyses, and with the analysts ease of performing these.

Definition of the biosphere. I'll walk through an example of recommendation for biosphere characterization, and then exposed groups. And if you have questions on these, I can answer those later.

Biosphere characterization. They thought that we should consider a program, a limited program perhaps in their words, of characterizing the biosphere. We don't have specific characterization data on the Valley that we have taken. There is information out there that we have used.

Our plan right now is to look inside of our models and do sensitivity analysis that may tell us whether or not we want to go out and actually characterize the biosphere. Is there a sensitivity to a high degree on some of the parameters or models that we've got in the system now?

In particular, they were concerned with soil related parameters, Kd specifically. And this is the subject of a TSPA KTI agreement that we're looking into, trying to provide better justification of the Kds that we are using in our modeling.

Exposed group. They thought that the survey, the food consumption survey we did in 1997, which we're using as our baseline right now, they thought we perhaps provided too much reliance, put too much reliance on that particular study. They thought that we should consider all human activities that could occur out there. An example that they gave was this fish farm that went belly up out there, I guess, and they were saying, you know, it's gone now, but, you know, it could conceivably return, since it existed once in the past.

And then, again, they thought that the survey, food consumption survey, was a snapshot, and that we should look at updating that in time.

We did perform a sensitivity analysis in regards to this, and that was with the receptor of interest, and it was essentially a consumption rate, sensitivity analysis where we took our baseline receptor, which is the residential farmer, and looked at subsistence farmers, someone who's growing 100 per cent of their food in contaminated soil. And essentially that worked out to be about a factor of three effect to dose, so that would be a bounding look in this regard, although that is not what we're providing for a baseline at this point.

We may look in time at going after another food consumption survey. We don't know of anything right now that's going to lead us out to the field to do that quite yet. It may be more of a confirmation activity, performance confirmation activity, if you will.

Model development, data, results. Again, I'm not going to walk through all these. I'll take a look, an example they provided on--or a recommendation they had on FEPs and conceptual models, and then analysis methods and results.

In terms of FEPs, they thought that we could do perhaps a better job on explaining how FEPs make it into the modeling, justification, if you will, and a tracking through the conceptual model. They felt it was a little difficult to follow, and they thought that we could be helped by looking at the international effort on BIOMASS that has a method involved that may help us out. And, in fact, in response, that's one of the things we are doing, is we're going back through the BIOMASS material to see if there's anything that might help us out in that regard.

The flow of FEPs into a conceptual model is a TSPAI KTI agreement we've got right now. So, the NRC is interested in this as well.

Analysis methods and results. This essentially went at uncertainties. They felt that we should provide a better explanation of the uncertainties we do have, and perhaps perform some more uncertainty analysis to bolster our modeling.

We plan on doing that this next FY, revisiting our uncertainty analysis and perhaps doing some more. We had done quite a bit of uncertainty analysis in Chapter 13 of SSPA, Volume 1. That information was not available at the time of this Peer Review, so they didn't have that to consider in their write-up.

There were some things that they did recommend that we continue, and that was to carry out analysis out beyond the regulatory requirement, 10,000 years. The food consumption survey, although they thought we should do more, they applauded the fact that we did go out in the field and gathered information on this. And then they thought that we should continue to report conditional doses for the volcanic event.

In summary, we did feel that it was a favorable review. There was nothing in our minds that came out of it that called into question our biosphere modeling. And we feel that we have a model we can move forward with. But they did provide 23 recommendations, as I said. These were suggestions to aid in the future development of our model.

A lot of the recommendations they have we are looking at in the planning next year. In fact, we've got them in there now, and we have done some work, as I said, in SSPA, Volume 1.

And I might mention, I don't have it on the slide here, that we are planning to put out a White Paper next year that sort of statuses where we are in these 23 recommendations so we can keep track internally, and also for folks on the outside to see where we are on these recommendations.

That's all I have.

CRAIG: Okay, thank you, Eric. Questions?

COHON: Cohon. You indicated that the Peer Review Panel submitted its report, its final report, in April 2000. I was curious why now 17 months later, it seems like--

SMISTAD: That was a typo. 2001.

COHON: Oh, I'm sorry. Never mind.

CRAIG: Dr. Bullen?

BULLEN: Bullen, Board.

Actually, maybe that will clarify this question, because the Peer Review was completed using TSPA-SR, or was it using the LTOM and HTOM supplements that were from SSPA?

SMISTAD: Those weren't available.

BULLEN: Okay. So, were there zero waste package failures in TSPA-SR, so less than 10,000 years, there was zero dose?

SMISTAD: There was very little dose, yeah.

BULLEN: I'm mistaken. You failed one or two, and had the diffusion release?

HOWARD: Let's keep in mind that there are the base case results that have zero dose for nominal scenario. You do have cases in that document that would give you an indication of what the doses would be if you have waste package fails. You have the human intrusion event. You have sensitivity cases for juvenile scenarios, and then my favorite, the disruptive events.

BULLEN: Okay. Thank you. That was Rob Howard.

CRAIG: Jeff Wong?

WONG: Jeff Wong of the Board. This is my third question.

You said that you're going to add swamp coolers to the pathway analysis, or you're thinking about it. If you had swamp coolers, is it because of the presence of air spalls from the swamp coolers? What's the--

SMISTAD: Contaminated water in the swamp cooler itself.

WONG: Okay. So, does that mean that you're going to include the exposure to showering also?

SMISTAD: I think it would be more of an inhalation thing.

WONG: Showering represents significant inhalation pathway.

SMISTAD: Yeah. It would include showering? Marilyn says yes.

WONG: Okay. And another question. When you say ingestion by animals, ingestion of what? Water? Fodder? Alfalfa from the farm nearby?

SMISTAD: Yes. All pathways that could get into an animal.

WONG: So are you going to then consider the recycling of manure used as fertilizer?

SMISTAD: We haven't thought about that, I don't believe. Recycling of manure? Fertilizer? No, we haven't. I'll make note of that.

WONG: I have one more questions, Paul. But I must be getting old because I'm forgetting it.

COHON: Jeff, could I--oh, are you ready?

WONG: Yes, go ahead.

COHON: No, if you're ready, go ahead.

WONG: In your view, you know, we heard from Dr. Payer about the uncertainties with the data gap TCs and corrosion. Which do you think contributes more to uncertainty, uncertainties in the biosphere or uncertainties in corrosion?

SMISTAD: Oh, definitely corrosion.

WONG: That was a baited question.

SMISTAD: I don't know, Jeff. I guess we've never done that.

WONG: I mean, it's whether or not to pursue the influence of various soil types on the BCDS versus pursuit of corrosion factors. I mean, that's a priority for funding.

SMISTAD: This is a priority call, and, you know, part of the aim of doing sensitivity analysis on these things is to try and figure out where you perhaps spend the resources.

WONG: Did the Peer Review Group at all talk about dust tracking models?

SMISTAD: Dust tracking--

WONG: Yes, I mean tracking, where you, you know, if you have a residential farmer, he's working outside, his family is working outside, he's bringing the dust inside. The dust becomes entrained in the indoor air, and he starts to breathe it.

SMISTAD: I'm having a real hard time hearing you, Jeff. I'm sorry.

WONG: I'm talking about dust tracking models. And actually, there was some work at Lawrence Livermore National Labs about tracking of dust indoors, and then having that, you know, as an exposure reservoir, a constant exposure reservoir.

SMISTAD: I don't recall them talking about that. It certainly wasn't in the report, and I don't remember it coming up in discussions or in the orals. I'll check on that, but I don't recall it.

WONG: I think if you're going to expand your exposure pathways, you would want to be more comprehensive.

Thank you.

CRAIG: Jerry?

COHON: Cohon. Slide 23. Did you say whether you were going to do these three things? I didn't hear you.

SMISTAD: I didn't say, I guess. Yeah, we intend to continue to carry our analysis out beyond 10,000 years. We don't know whether or not we're going to step into a, prior to LA, step into another survey, food consumption survey. Again, this is part of the sensitivity analysis we'll be performing the first half of next year to tell us whether or not that's something that we should spend money on. And I see us still reporting conditional doses for the volcanic event.

COHON: You say you see you doing it? I didn't hear you.

SMISTAD: We're not going to be regulated on it, but we produced it in just about every document recently that we've got.

COHON: So, yes, you will be reporting conditional doses?

SMISTAD: I would expect we would continue to show conditional doses.

COHON: Thanks.

CRAIG: Richard?

PARIZEK: Parizek, Board. Of the 23 recommendations that were made, several you picked on as being ones you're going to address. Will you explain in the White Paper those that you will not address and why? For instance, the million year time frame of interest was one you just commented you probably will consider that. The other one was basically the species, other than humans, ecological concerns, you know, in terms of animals and--

SMISTAD: As a pathway, yes.

PARIZEK: No, not to eat them, but those endangered species have their own value. I think that was one of the international recommendations made. Are we going to throw that one out?

SMISTAD: This Panel didn't recommend that, and I don't see us taking on that in LA space right now.

PARIZEK: But of the ones you'll not include, you'll explain perhaps why they might have been made?

SMISTAD: Yeah. You know, I'm envisioning this White Paper to talk about, you know, what we have done, and the results, what we plan to do, and things that we might just not get to, and the reasons why we won't get to them, yes.

CRAIG: Eric, thank you very, very much.

We now move on to our last speaker of the day, Abe Van Luik, well known to everybody here, not only for his technical work, but also for his website and his photography.

VAN LUIK: A little abuse is good for a person. This is Abe Van Luik, DOE. This is an unauthorized update of the Peer Review, and the Peer Review Team will not be bound by anything that I say that they mean. It was made very clear to me by the Chairman that I can say what I want, but they're going to pay no attention to what I say.

I want to give you a very similar outline to what Eric gave.

The thing that we asked for is for the Nuclear Energy Agency, part of the OECD, Organization for Economic Cooperation and Development in Paris, and the International Atomic Energy Agency, part of the United Nations, to put together an expert team and look over our TSPA, Total System Performance Assessment, for the SR, the Site Recommendation.

These two statements here I took out of the statement of agreement that we came to with those agencies. We went ten iterations on it until I got it right. But, basically, they needed to have stated in there under what authority they carried out this type of review. And the IAEA is supposed to do services useful in either research on, or development or application of, atomic energy, international standards, and in fact they are working on a generic international standard for repositories even as we speak.

And the NEA has a mandatee for improving and harmonizing the technical basis for dealing with nuclear waste related issues among its member countries, and we are a member country.

The primary subject of the review was TSPA-SR. We made all of the supporting documents available to them that they could handle. In fact, I think we spent a lot of money Fed Exing documents over there. It's very expensive. Over there meaning Australia, Japan and Europe.

Documentation of subsequent work was also made available. So, we were in time to have them look at the SSPA and the Uncertainty Evaluation that we did this year. However, their charter was to look at TSPA-SR and to consider these other items as they had an interest or felt a need to.

This review, as the previous one, was not under the Quality Assurance Program, which means it cannot be used for either product acceptance or validation. But the comments that we get from them are just as useful as anything else that we get from the Board, the NRC or anything else.

Two meetings were held in Las Vegas in June and August. Three exchanges of questions and responses by e-mail between the meetings, and those are records in our record system. However, the review team will decide what they want to do with that material, whether they want to use it or not.

August meeting, the one that we just finished a couple of weeks ago, served to clarify questions as well as responses. And the one thing that we learned, to my dismay, especially in all the questions that I answered on behalf of the program, is that I often misunderstood their question, and in fact I generated some considerable consternation because I was answering the wrong question, and they thought that I was purposely answering the wrong question.

But this all came together in the August face to face meetings. It's amazing when somebody explains it to you three times, you actually get it.

They also, after a whole week of meetings, and they worked almost until 9:30 every night, they presented their preliminary observations to us on August 31st. The executive summary is due by the end of October. The final report is due by the end of January, 2002.

These were the members of the team. I know these people. Perhaps some of you don't. Materials sciences, we had Emmanuel Smailos, who is a Greek working in Germany on the German repository program, and he had some very interesting insights. All of these other people also had interesting insights, and it was a pleasure to work with them, because they caught onto what was important and what was unimportant right away.

These are very unofficial highlights, and the point that Tonis made to me is you can say what you want, but only the international review team can report its findings prior to the final report.

Now, these are the recollections of one person, me, in the audience where the team gave an overview of their very preliminary impressions.

They basically said TSPA methodology that we are using conforms to international practice. You know, it's not outlandish.

TSPA-SR is appropriate for addressing the regulatory compliance requirements that are the basis for the site recommendation decision. They read that language very carefully, and it says "likely to show compliance with regulations." They said, you know, we'll give you that one.

TSPA and process models, and this is the big "however," the big "but," if it is to provide to the regulator reasonable assurance in the next step of safety, or is to become part of a comprehensive safety case with considerations that go beyond regulatory compliance, these things need more work. So, these things were the pat on the back, and then here comes the big slap in the face.

They said that good choices were made for the waste package and drip shield materials, but more experimental work is needed to provide firmer basis for modeling. And Smailos provided his own list for what he thought we needed to do, in addition to what we were doing. And it will be interesting to compare that with Joe Payer's Committee's recommendations.

There was much questioning of the cladding model, and I think here is a case where we actually caused them to think about the differences between Yucca Mountain and their repositories. Their repositories are all under very high hydrostatic pressure, and in most cases, they are surrounded by an envelope about, well, up to a meter thick of bentonite that swells.

So, naturally, they expect deformation of the waste packages over time, and cladding is not a consideration. And, so, they thought, gee, ambient pressure, maybe so. So, they were I think very much questioning the cladding model, and came away thinking maybe they have a point here. But it will be interesting to see what they actually say about it.

Movement of radionuclides out of the waste packages through continuous films of water is incredible, not just conservative. And what one of the gentlemen said to me was this is how we--the way you model it here is how we model it in a saturated repository, is diffusion through free water. What's wrong with you guys? You have a dry site. Okay, so this will be an interesting observation, how they fill it out.

They liked the unsaturated flow and transport modeling basically, but the active fracture model they said needs validation. They liked the model, but they didn't see that we had a good enough basis to really have confidence in it.

The saturated zone flow and transport modeling needs additional site specific work, and it needs a new regional model. As a matter of fact, Ghislain de Marsily was on the review committee for the 1997 model, and he called it substandard and just would not let go of that. He says when you take the boundary conditions from that model, you are misinforming your model, which is a much better model. So, we're very well aware of that, and told them we would fix it. And he said, well, until I see the proof of the fix, this is going to be my comment.

This is one that's near and dear to the Board's heart. We showed them the uncertainty report. They said yeah, we agree with this. It says you need an overall strategy for evaluating and reducing. They insisted that we reduce some of our uncertainties. And part of the reason for that was some of the large uncertainty ranges we are using may be conservative at the process level, but they may be non-conservative at the system level. This is dose dilution, needs evaluation.

And in close questioning of what they exactly meant by this, they said we're not accusing you of purposeful dose dilution, but there are standard procedures, like three tests, on which you base an evaluation of whether or not you're artificially diluting dose.

For example, if you have, you know, this is a real trivial example, we're very well aware of this one, but if you had datapoints and you bring in an outlier and cover them all with a distribution that captures that outlier, depending on which direction that lies, it may be conservative or non-conservative. Said basically, we're not questioning what you did. It's just you didn't give us enough information to feel good about the distributions that you selected.

So, they said volcanism, although it's very conservative in your assumptions, seems to be handled appropriately, if you want to stand by those assumptions.

More could be done to evaluate human intrusion. They did not like the regulatory truncation of that analysis. They wanted something more to be done there.

They actually read through our FEPs literature, and said we have a couple here that need evaluation because we couldn't find them. And they happened to be ones that are very important. For example, when it comes to cladding, the metal degradation of their waste packages causes an increase in volume, and that crushes their cladding also. We tried to tell them that we evaluated that, and they said, well, you can tell us all you want. We looked through your FEPs evaluations, could not find it. And then we went back and looked and said, yes, we couldn't find it, even though the person that did the evaluation was sitting right there.

So, these are the kinds of things that they picked up on right away. And in some cases, in fact in a lot of cases, they said only after talking to you did we understand really what you meant in the documentation. It is not yet sufficiently transparent, which was crushing to me, because I thought we had come such a long way from, you know, where we were a few years ago.

Impressions. These are strictly my impressions. The review was technically critical, but I feel it was balanced. The experience level of the reviewers was apparent very early in the process. Even the first set of questions they asked, they honed right into the important issues of, you know, what were the meaningful areas of weakness. And, in fact, I asked some of them if they were intimately familiar with the proceedings of the Board and the KTI meetings, and they hadn't really, you know, been.

And what they said was, well, you know we do this for a living ourselves, so we know what the weaknesses are, and we just hope you have answers.

Instances of critical observations were based on known problems faced in other nations' evaluations of long-term safety. That's what I mean. These people were experienced, and went right into where the difficulties are.

A very important issue for the review team was the ability to compare safety evaluations of different nations' potential repositories. And this is where they thought that the performance measure prescribed by the EPA and the NRC really was a hindrance, and we needed to go beyond it.

They said that the time constraint, combined with the locational specificity of the target individual is rather unique. No one else prescribes things quite in that way. And, so, they suggest alternative performance measures for greater insight. For example, one of the questions they asked, and this was in response partly to the idea that in the future, we're going to be looking at non-human impacts on the environment, and they said at least don't preclude having the information available to answer those kinds of questions. Look at the fate of the radionuclides beyond 10,000 years and beyond 20 kilometers.

Now, luckily, in the EIS process, we are looking at those things. But they said, you know, one of the great benefits of your site is you're not going to poison the whole world because you're in a closed basin. But at least tell us where your radionuclides are going to go in a closed basin.

Kind of a humorous aside is one of our analysts, and you know how analysts are, but we assume that they will all be taken up at 18 kilometers. And one of the gentlemen said that is a typical modeler's answer. He says we are looking for some semblance or some indication that you have a grip on the realities of your situation.

So, it will be interesting to see what this comes out, but I think this is very similar to what came out of the Biosphere Peer Review, is you look like you're ready to address your regulatory compliance issues, but convince us that you really understand this system. And I think in the review, that's going to have a lot of play on a lot of issues.

CRAIG: Abe, thank you.

VAN LUIK: I didn't hear the bell.

CRAIG: You're fantastic. I was counting on you to meet our Chairman's requirement for getting finished with the session, and you delivered.

Questions for Abe? Hey, if no one has any questions for abe, we're actually early.

VAN LUIK: How can you ask me questions when it is an unauthorized status report?

CRAIG: Okay, Alberto is going to ask a question.

NELSON: Just a clarification. Will this group be meeting again before their final report? Nelson, Board.

VAN LUIK: In October, at the end of October, they're going to give us their Executive Summary, which will be their high level impressions. And in January, the end of January, they will give us their detailed report with all of its recommendations.

NELSON: And will the people be here for both those occasions?

VAN LUIK: They will not. They will send us probably by e-mail the report for fact checking in both instances, and, you know, that's the protocols we've established. We cannot comment on the substance of the report, but if they say 23 kilometers and it should be 18 or 20, we'll make that clear to them.

We will receive those reports, and they will tell us when they're good enough to send out. You know, we'll make them public, in other words, as soon as we can. They will publish the final report, just like they did the biosphere report, and I'm getting 500 copies. So, I hope there's a lot of interest.

SAGÜÉS: I was intrigued by their statement, or what you reported to be their statement, that movement of radionuclides out of the waste packages through continuous films of water is incredible, not just conservative. Did they give any suggestion as to what would be a credible model to address how radionuclides make it out of the package?

VAN LUIK: It will be interesting to see if they go to that level of detail. The comment they were making was that, in fact it sounded like a Board comment, is that the mixture of conservatism with what they thought wasn't very conservative in some cases, and then this ludicrous assumption which they thought they were talking about here, where diffusion has a direct pathway with a film of water all the way from one place to the other, that that really made for a less credible analysis than it deserved.

In fact, they kept recommending that we do what do you really think is going to happen type of analysis, and then use that as a comparison point. Does that sound familiar?

SAGÜÉS: Is it unfair to ask what do you think it will be, what would happen to get radionuclides out of the package, or maybe--you don't need to answer this question.

VAN LUIK: Yeah, my gut feeling is that it would basically take away 90 per cent of the diffusion controlled releases. You would only have a contribution from the advection controlled releases, and probably it would lower all of those curves for a long time.

Since you asked the question, I thought it was interesting that something that I did not know, but you may, is that two European countries are now looking at Alloy 22. One of them is Germany, and one of the reasons that he's making recommendations to us on what should be done is to basically bolster their case for using Alloy 22. And he says, "I'm not surprised at all by the results you've gotten so far, because we're trying to dissolve Alloy 22 in salt brine, and it just doesn't seem to happen. It's always below measurability." So, it will be interesting to see if two other programs now are looking at Alloy 22, we may accelerate the credible database that we need.

CRAIG: Don Runnells?

RUNNELLS: Runnells, Board.

I'm just curious, Abe, on Page 10, and you really emphasized it, how quickly they went to meaningful areas of weakness.

VAN LUIK: Yes.

RUNNELLS: Could you give us a couple of examples?

VAN LUIK: One of the areas that they went to very quickly was the lack of a coherent evaluation of uncertainty at every step of the process, that it looked to them like there was a real mismatch between the level of detail of the uncertainty discussions, for example in the UZ work and the SZ work, and I think those were the two examples that came out right away, is one looks like a competent model, the other one looks like a nice patch over a large unknown.

These people had a sense of humor, which was a little humiliating at times.

CRAIG: Abe, thank you very, very much. And we've come to the end of the session.

On behalf of the Board, I'd like to thank all of the speakers for their many excellent talks. It's been a good day, and we now turn to the second public session.

COHON: And on behalf of the Board, I'd like to thank Paul Craig for his wonderful job of Chairing. Let the record show that we ended exactly on time, which is a wonderful accomplishment. And they were very good sessions.

We have three people who signed up for public comment. Before we get into them, you may recall that the public comment period in the middle of the day ended with a statement by Dr. Shettel, who also showed some slides from the backup material from Dr. Pulvirenti's presentation.

I want to give the DOE a chance to respond to that, and I meant to do it then, but did not, but we'll do it now. And I believe Greg is ready to do that. Yes?

GDOWSKI: Greg Gdowski, Livermore.

COHON: Maybe you can remind us what Dr. Shettel showed us.

GDOWSKI: What was shown was an EH/PH diagram, or Pourbaix diagram for lead solubility, and I wanted to comment that a lot of valuable information can be obtained from that, but as I show on the slide, there are other things that need to be considered.

Lead aqueous concentrations are controlled by numerous things, lead precipitates, lead sulfate, lead carbonate precipitates, but also lead substitution into minerals, primarily calcite. Lead substitutes very easily for calcium and calcite.

You also get a lot of lead absorption onto mineral phases such as quartz, calcite, smectite, and one I can't pronounce. And also there has been a large database put together by the USGS of lead concentrations in the Southern Great Basins, and it includes Yucca Mountain.

The mean concentration of lead in all those waters is about 9 ppb. So, it's a fairly low level.

COHON: Greg, from which presentation is this taken?

GDOWSKI: From Jerry Gordon's presentation.

COHON: Okay.

GDOWSKI: Slide 44.

COHON: Number 44 in Gordon's presentation. Thank you very much.

Now, we turn to those who signed up to make public comment, and we start with Judy Treichel.

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force.

The question was asked earlier in the day supposing you were ten years out and you were looking at some of the materials that were being presented today, and it would seem more appropriate that it be ten years out when we've heard about all of the work that's left to do, not just on data gathering and on understanding the site, but also on regulations. And even with all of the work on the Peer Review for the TSPA-SR, it seems crazy that that would now be being applied and that we're on such a race track.

And a friend of mine earlier today talked about some analogies for what we're doing, and one of them was that if someone came to the FDA and said I've got a great product, and let's just in order to talk about it say that it's a vaccine that would prevent AIDS, so you'd vaccinate everybody, and the FDA would say, well, show us what you have, and then we may or may not allow you to sell this. And the answer is, well, we've got something we think is pretty good, but we don't want just an approval on that. We want to be able to sell it to get it going, and then we're just going to keep working on it. And if we find out that there's problems then, you know, we can back off and whatever.

And this very much sounds like that, just sort of trust us and let us go.

And the other analogy when it came to Titanium, and the fact that that's supposed to be added in 300 years, if you look at this part of the country, 300 years ago, it was probably under the rule of the Spanish, and they may have made some assurance to the residents at that time, and I doubt that anybody would pay the price to carry through on some of the Spaniards assurances that could have been given then.

So, it all seems very, very crazy. And there has been some talk about the hearing that was held on Wednesday. I'm still furious about it, and that sort of colors the way I'm able to talk, but it was the chance at that point, when everybody got a chance to check their assumptions, to see how valid they had been, because the Department of Energy had made some assumptions about the level of opposition and the sort of concern that Nevadans had about this project. And they had also made some assurances to us that they really cared.

And last Wednesday night, we were able to check all of those things, and I suppose many of you know about how it went. But it was very clear to those of us who were here for that that public opinion and public comment is really just a little series of triangles on the timeline, and that needs to get out of the way, and the Department did not care, and they gave us very, very short notice. The hearings were absolutely a travesty. They were held in a dreadful place.

I was surprised that as many people showed up as did, because the whole thing was televised so that people could watch it at home. And, in fact, my oldest, my son and his wife watched it at home because on the news, it said don't try to go out there. You'll be turned away. You won't be able to get in. The place is full.

So, even with all of that, we had a meeting that was absolutely a nightmare. And it wound up finally about 2:30 in the morning, the next morning, everybody was worn out and had left, but I would say that two-thirds of the people that intended to say something were not able to do so. And that's just going to go on the timeline as a checked off box, and I think that's dreadful.

There was also a lot of confusion, and partly it was fueled by the letter that Lake sent out suggesting topics that people could talk about, or that he thought it would be nice to talk about. Because anybody coming to that hearing knew what they wanted to talk about, and we were never allowed to talk about alternatives to Yucca Mountain. That was always not the law, and it was never even allowed in the EIS. And for somebody to have gotten ahold of the PSSE and read it through in order to give DOE their comments, was insane.

And that's what led to the question that I asked earlier, because there was a lot of confusion, and people still are calling me to say if I came out there and I said as a resident, I oppose this project, I'm raising my children, I don't believe that there's enough assurance, you know, for whatever reason was given, it was our belief that they were supposed to either support the project, oppose the project, but at least let their opinions be known so that they would weigh in with the Secretary. And there's a lot of confusion about whether those kinds of comments will be thrown out, as they are in every other meeting that we've had where you had a very narrow scope that you had to talk about, and that was why I asked to see what sort of guidance was given.

And, apparently, you just got 30 to 40 people with absolutely no direction that are sorting through the mail, and that doesn't make sense to me, and I don't think I believe it. And for us to be told now by Lake when he was answering a question that came from the Board that DOE is working on ways to increase public involvement, and here we are screaming through the hearings and heading, you know, full force toward the site recommendation, it seems just a little late, and it seems very disingenuine, and I would say that it's just sort of adding insult to injury.

Thank you.

COHON: Thank you, Judy. I'm interested by that metaphor you, or the parallel you drew with an AIDS vaccine. And, actually, that's worth thinking about, because, you know, new drugs go through different phases of trials, including clinical trials, which in fact carry risks to the volunteers in the trials, and some people die during trials. And the whole question--

TREICHEL: They usually start with people who are dying anyway.

COHON: No, actually that's not true. I'm not arguing with you. I'm just saying that that's a very interesting parallel to think about. Just to demonstrate the point, in response to your question that they're usually very sick anyhow, you may have read about the difficulty that Johns

Hopkins University went through, and they lost their NIH funding for several days. That was the death of a completely healthy volunteer, who had no symptoms of any kind, who died in their clinical trial.

Now, the point of all this is that is a very well developed system, based on a phased approach where you try to learn, and you learn, and you learn, you actually try it out. Now, I'm not saying that's what ought to be done here. I'm just saying that, in fact, there is some precedent for that.

TREICHEL: Do you want a volunteer?

COHON: Judy, if you want anybody else to hear you, you have to talk--

TREICHEL: Oh, never mind.

COHON: The key word was volunteer. That's a good point.

Tom McGowan. Now, before, Mr. McGowan accused me of looming when I stood over there, not lurking, but looming. I loom. But now I'm going to sit down.

(Mr. McGowan's complete written comments are attached hereto as an appendix.)

MCGOWAN: Thank you. Wonderful guy. Anybody got any lights? It's the public light; right?

The reason that the Europeans misunderstood Dr. Van Luik is because he also happens to be European. Is that correct?

VAN LUIK: Yes, sir.

MCGOWAN: Dutch?

VAN LUIK: Yes.

MCGOWAN: And I am advised that in fact, the Dutch invented the toilet seat. And a hundred years ago, the Germans invented the hole in the middle. So they do have some basis of discussion. That's all right, Dr. Van Luik.

Are you ready for this? Now, here's the public comment. That was whatever you guys do.

Tom McGowan, individual member of the human and universal public--how do you like that--residing in Las Vegas, Nevada. I'll cut directly to the bone.

On axiomatic grounds, and I won't belabor that, the official generic "you" inclusively are engaged in a prior knowledgeable, willful, deliberate and intentionally malicious conspiracy to commit the crime of mass genocide on a historically unprecedented human and universal scale, intergenerationally, and in perpetuity.

It doesn't make you a bad guy. You're all very nice, good and decent people. Believe it or not, Eichman had a great yin for music. He was a connoisseur of fine beers. Swell guy. Had great clothing and everything else, and he was miles away, okay, from wherever the action took place. It was horrendous. Separated by time and distance, but guess what? A tribunal at Neurenburg said that's not a competent legal defense, and it wasn't. So, I'm telling you this right now, this is the first day of the rest of your life. You're not at the ending of anything. You're at the beginning. Think carefully about that, folks, because I care about you. If I didn't, I wouldn't even bother with it at all. The opposite of love is not hate; it's indifference.

You will stand accountable, responsible and liable for your acts, omissions and recommendations before an international tribunal, consistent with the legal precedent of universal jurisdiction, not withstanding rank and station, or sovereign national allegiance--or whatever his name is. Okay? All these people say they didn't place it, they think they can get away with it because they're out of the country. Baloney. Okay? They're going to the universal tribunal, no question about it, and they should.

The following six questions are directed to individual members of the Board, and the final question is directed to Dr. Abe Van Luik--of the DOE.

Please respond in real time and on the public record, without reducing the limited time allotted for public comment. Thank you. Which is timed to consist of five minutes. Any reduction of that allotted time will be construed as public censorship, and charged accordingly.

You all remember Dr. Jeffrey Wong. You need not stand up. Consistent with your experience and expertise in the field of toxins and environmental disciplines, have you ever known of a toxic or otherwise hazardous contaminant whose enduring term has ensured impact upon organic life and/or the natural environment requisite to sustain life, which exceeds the maximum term of the design-engineered integrity of a secure containment material?

And, if so, what's the most effective means to ensure the permanent disposal of said toxin, or hazardous contaminant guaranteed secure from the imposition of impactive consequences upon organic life and/or the human accessible natural environment requisite to sustain life? That isn't to say toxic radionuclides. Is there any other kind of contaminant that outlives its container? Yes or no? A simple nod of your head, or whatever the hell.

Dr. Wong, hello?

WONG: I'm listening.

MCGOWAN: I can't hear you.

WONG: Do you want me to answer that question?

MCGOWAN: No, I was just practicing--

WONG: Yes or no?

MCGOWAN: Would you please?

WONG: Yes. Yes, there are.

MCGOWAN: Yes, there are. Good. How would you dispose of it?

WONG: Right now, the current practice in America is to bury it in the ground.

MCGOWAN: Really? Case number two. Thank you, Dr. Wong. And to continue with Dr. Wong just briefly, what parallel concern, if any, do you perceive in the instance of the application of your response to the question if and as placed in juxtaposition to the question of the underground storage of high level nuclear waste, if any? Do you see any corollary there?

WONG: I don't understand your second question.

MCGOWAN: Okay, good, that's fair enough. In the interest of time, we'll just cruise right along.

Board member Dr. Richard Parizek, over there. What method of ensured absolutely perfect vacuum, if any, would guarantee the permanent removal of 100 per cent of all forms of moisture from containerized high level nuclear waste prior to its emplacement in an underground repository?

PARIZEK: You mean the container, to make sure it's dry inside?

MCGOWAN: Yeah, completely. Before it gets into the wet thing.

PARIZEK: I understand there is a process by which wastes are to be shipped, and by which the container is supposed to be dried out prior to placement.

MCGOWAN: Okay. From Frenauld, for example? You don't have to answer that one at all.

And Board member Dr.--

COHON: Tom? Tom?

MCGOWAN: --Debra Knopman.

COHON: Mr. McGowan, may I just interrupt for a minute?

MCGOWAN: Yes.

COHON: This is fun.

MCGOWAN: No, it isn't.

COHON: But I'm not sure it's particularly valuable. Now, if you're trying to--if you would like the Board to consider certain issues, we'd be happy to hear them.

MCGOWAN: All right, here's--what you're doing is called public censorship, because you have no control over what I say during public comment. I will be as polite and sweet and gentle as is feasible and possible. I'm not here to embarrass any of you, and I hope you're not here to embarrass me.

Bear in mind I will not be an accomplice to suborn your activity, no matter how well intended you think you are, because in my impression, and I'm absolutely unswervable, you can't pump that stuff into the ground without killing somebody, if not in 10,000 years, in a million or 2 million years. They're not here today to speak with them. I will. Okay? I will speak for them. You will not stop me from speaking.

I will submit a hundred questions this week, and then a thousand the week after that. You will publish them in every single report to the Congress, to the President, and the entire world, because those issues--and all of us combined. If you don't believe that, leave, you won't be missed. But I will not--that's guaranteed. Your name is what? Right, we got that down, don't we. It is ineradicable throughout all of the history, you people will--future generation. That's what you were deemed, and here you are dealing with this stuff and that stuff and everything else.

Where is the repository? Does anybody know? Where is it? You are extremely well facilitated, except for one thing, the word begins with an I and ends with a Y, it's called integrity. But I've got enough for all of you, guaranteed. You won't forget me. I won't let you.

And, Dr. Wong, thank you for responding. The rest of you didn't get to hear the questions. You don't have to respond at all. I'll simply put down here that concludes my public comment.

Let the record reflect the Board's policy of allowing adequate time for the DOE's participation in the democratic process, does not extend to include members of the interested and affected public of this generation or any future generation. How dare they do something like that. I mean that literally. Think of what you've done, and all the work you've done is--by that one action, because now we know where you're coming from, don't we.

You want to hear the public? You haven't heard them yet. You won't hear them here? You will hear them out in the street. You will hear them in all of the streets of the world, because that public is not coming to your government ever again. They have lost faith, trust and confidence in you. Whose fault is it? Not mine. I don't make the laws. I'm as pleased as punch that Lake Barrett, Dr. Barrett, had the decency to--all those letters, didn't you, Lake. It did come. You sent them all, didn't you? Or did you give that--and your signature at the end of those letters, was that xeroxed, or did you actually--how many more letters have you got to sign, Lake?

Who the hell is kidding whom? You don't want to hear from me, sir. You know why? Because you're hearing the God damned truth. Pardon my French. I love Dr. Abe. I love all of you, every single one of you. And in spite of almighty God, it is you who holds the key to the future of mankind. You. And you sure as hell will live to my standards.

I'll be back tomorrow, or are we allowed to be here tomorrow? Do you want me to apologize? Forget about it. Never, ever apologize. Don't reinforce a negative. What for? I don't make the laws. I don't make the rules. You did. Now live with it. So, if you want to come back to me with maybe a card or some flowers or some damn thing and say, Tom, you know, can I have another shot at that, I'm not really afraid of your questions. These people are over 21. They're allowed to answer questions. Right, Debra? You don't want to hear your questions, never, nobody wants to hear it.

It's simply this, okay? So I'll direct it to mid air. How--science and technology. You tell me how Galileo did it, how Copernicus did it, or how any scientist worth his salt did it? How do you do it, and how do you live with it once you do it? This isn't about Yucca Mountain. This is about human nature and human means in public, Ladies and Gentlemen. I'm here to tell you that in my book, as--would say, you're all right, but you have room for improvement. You could strive to her idealized standard, and you will do that, whether I'm here or not. Because Halloween is coming and I'll come back, guaranteed.

Thank you.

COHON: Mr. McGowan, you're beautiful when you're angry. That last question was an especially good one about the separation of science and technology.

Tom, can you--now, listen to me. Okay? We're happy to have your questions. Questions come in various forms. If you really want an answer to questions, we'll consider answering them, but this is not the forum to do it.

If the questions are rhetorical, we're happy to have those as well. If the purpose of the questions is to stimulate the Board to think about certain issues, we're happy to hear those as well. But, here's the key point. The Board's agenda is its own.

MR. MCGOWAN: I can't hear you--

COHON: You're very mobile now. I mean--the long and short is we'd be happy to have your questions. Okay? But the quiz that you entered into I don't think is an appropriate thing.

If you would give it to us in writing, we would very much like to have it. Okay, we'll see you tomorrow.

Sally Devlin?

DEVLIN: Mr. Chairman and fellow people, I'm here because I was remiss when I did my report this morning, and I didn't tell you where I got all the information, and it was from the Draft Environmental Impact Statement for a Geologic Repository for the Disposal of Spent Nuclear Fuel and High-Level Radioactive Waste at Yucca Mountain, Nye County, Nevada. And it's Volume 2, Appendix A through I. So, if you want it, this has everything I was looking for. As far as I was concerned, there was nothing in the PEIS that was really comprehensible.

But I want to talk about transportation, because nine years ago when the only plan you had was bring the stuff through Pahrump on the railroad, I said, "Over my dead body," and I've kept true to that. And I think it's most important for the people here, especially those on the Board, to know what the transportation situation is in Nevada.

And when we had the Ninth International Conference, the people from Connecticut and Maine, called the Yankees, came and they were wonderful about how they had closed down one nuclear plant and stopped another, and so on and so forth. They even lost their livelihoods because everything was predicated on the nuclear plants. And they said we've never heard anybody from Nevada say why you don't want our waste. And, so, I stood up and I told them why we don't want their waste.

And I'm prefacing this with that comment, because I cannot understand how you can do any licensing, or even write the report, when there's no transportation studies. And I am quoting verbatim, as I did in my report on the first EIS on Yucca Mountain, you had 200-something pages on transportation, and then you had the disclaimer. You had umpteen pages on the canisters, and then you had your disclaimer. And, of course, you use your wonderful word, absurd uncertainty, is what I call it.

So, I'm using that term in a very negative way, because I really feel that way. Everything you have done, you have a disclaimer, and it's in there, and I write it in my report. Just read 200-something pages on transportation, and then you'll see that you say, and I am quoting the DOE, we have no transportation. We have no canisters.

In this book that I referred to, they show you all the canisters for the DOD stuff, and as everybody knows, it's taken me years to find out what DOD wants to put in our mountain.

If you remember, the Navy came out to placate me, and I wouldn't talk to them. But I did find out how much spent nuclear fuel from the submarines there was, and there was very little. The other stuff, again a disclaimer, we don't know. You don't know. Abe and I don't know. And I don't think the Board knows, because this was all hidden. There are 8500 canisters of something of this. They're going to be in steel canisters, and of course I have to mention my favorite bugs, because my bugs will eat it all. And this goes, again, to the disclaimer, and this is rather serious.

Now, let me go back to my original thing about Nevada and why we don't want this nuclear waste in Nevada. Number 1, we have one intrastate highway, and the report came from INEEL, and it is a 9 hazard highway. There is none higher. 160 is a 7 hazard, almost as high. You cannot come through Pahrump--I mean, you can't go through Las Vegas, you come through Pahrump. Now, we have no railroads, and of course you talk railroads.

And when I started, the cost of the railroad was $1.8 billion. This would be from Jean down Sandy Valley to Pahrump. Well, I have recently requested of our commissioners that we get a properly surveyed by a surveyor with a stamp, the boundaries of Pahrump. We are somewhere, and I have this from the census, because I did it in '90, 370 square miles. Now, that's twice the size of L.A.

Now, what is Pahrump and where is a Pahrump and what is a Pahrump? A Pahrump is on the von Schmidt line, and that von Schmidt line has never been properly surveyed by a proper surveyor with a stamp, and the reason is that's the boundary between us in Nevada, we're Nevadans, and California. It also is the boundaries between the brothels and the von Schmidt line and California.

And I always ask the question because I like a good laugh, is where do these truck drivers go to the bathroom, and of course the answer is to a brothel or a casino, because we are known as the hospitality state. Right?

And when you look at the distance between Pahrump and Tonopah, 220 miles, that's a long way between drinks. And, again, we always extend our hospitality.

So, there are no boundaries. I see my pretty Wendy Dixon back there with the plan on the rim, which is the BLM land also, and that's in the flood plain. Now, we're even working on getting a flood plain map. We haven't had one since 1985, and of course Pahrump will soon be 120,000, 150,000 people because of the number of parcels being sold. So, we'll learn some planning if we have a boundary, and so on.

But, again, NDOT, which I hope is being investigated by Senator O'Donnell, promised us a four lane road, and of course they've reneged on this promise. And I've always said if we had four lanes, would the loads get half lighter when they go on the single lanes, and that's the situation that we have. We have no roads. We have no railroads.

And I'll get to something else more important than that. I just did a report for the governor because we've had three E-coli epidemics or outbreaks, or whatever, in Pahrump. And I learned there are 107 different kinds of E-coli, and this was the children of our newly elected district court judge, and they had many problems, and what happened was confidential. So, when I called the Health Department, they said it was confidential, and I said, "Just tell me one thing. Do you test the water?" And they said, "Well, we test the water if it's on the municipal water system," which is CalVada, which is a CCNR, and they're about 1200. 8500 of us are on wells and septic. That's me. And if I were dying of E-coli, the state would not test my water.

So, I went one step further, and I said, "Okay, what if we have pandemics, epidemics, bioterrorism, what happens?" The only agency in the State of Nevada that is open 24 hours a day with 48 hours of training is the Highway Patrol. There is no hospital in Nye County. There is a private hospital, a twelve bedder, in Tonopah. We will get one in about two or three years, a 40 bedder. But that's a long way off.

So, I'm getting back into you have no transportation plan. Let's get into that. And let's get into the canisterization, which is in this great big book, and you have no canisterization, and you say so. You disclaim everything. And the worst part of it is to the public, and that's me, when I see 200 pages and then a disclaimer, you not only have no transportation and you have no canisterization, but what is even worse is the plans for the future. And that is you're trying to rush through a license in October and then in 2002, and you really have nothing but, as we say in the brokerage business, blue sky. And that is my interpretation of it.

And I want to read one little thing, and I think everybody should get this report. Thanks to Russ, I talked to him about emergency preparedness, and only our Highway Patrol can handle anything. When we have had accidents, and we've had a few of them, the only service to us in the rurals, which all the stuff will be going down by rail or by truck, is from Fallon Naval Station. And it takes them from four to seven hours to get to an accident. And if that isn't terrifying, I don't know what is.

So, you know I have been to every DOT meeting, and DOT is not here. Where are they? Because the transportation of this stuff to get to Yucca Mountain is numero uno on my list. The trucks, and of course when Lake Barrett--is he here--good, okay. Remember when you said we will privatize the transportation and that will save 20 billion? And I took great offense at that, because to me, this is government waste. The government should transport it, and let those people die, because again, in this book, it says on the workers, 7, 8 and 12 out of 1000 will die of radiation poisoning, and those are your numbers, not mine, from the DOE.

So, this is very scary. That's 7, 8 or 9 for the different categories, or 12, out of your book, and EPA says 1 out of 100,000, 15 millirems, and NRC says 25 out of a million.

So, I'm going to conclude, but I do want to recommend a book for everybody to get, and it's called "The State Road and Spent Fuel Transportation Safety" and it's done by a gentleman--I had the one in '95. This is the current one out of Denver, and this is the list of what the different states, 22 states, charge for bringing high-level waste through their states.

Nevada, for example, charges 500 for the trucking company, $150 per truck, plus actual cost for investigation. Now, the way the State of Nevada works, and again it's with transportation, you have an accident, which will happen, and remember me bringing Price Anderson, which is mentioned in this, you have an accident and the Highway Patrol takes care of it. Fallon comes down and cleans it up, 11 hours since incidents have happened. And then it goes to the Department of Motor Vehicles to their accident department, and some guy says, gee, that's not nice that it caused $3 million or whatever worth of damage. And they sit up there in Carson City, and he's a bureaucrat and we get these reports.

So, this is the reason that I said to these Yankees that we don't want your waste because there is no way in this world that Nevada can handle it. And that's it. So, I don't know how you can do the licensing.

Abe, I'm sorry, we're getting divorced. We're not living together for 225 years.

COHON: Thank you, Ms. Devlin.

That concludes today's session. The meeting reconvenes tomorrow morning at 8:30 in this room. At 7:15 in this room, there will be coffee and donuts and an opportunity for informal interaction.

My thanks to all the speakers and all who participated today.

(Whereupon, the meeting was adjourned, to be reconvened at 8:30 a.m. on September 11, 2001.)