UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

SPRING 2001 BOARD MEETING

SCIENTIFIC AND TECHNICAL ISSUES

May 8, 2001

Hilton Arlington & Towers
950 North Stafford Street
Arlington, Virginia 22203

NWTRB BOARD MEMBERS PRESENT

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

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P R O C E E D I N G S

8:05 a.m.

COHON: Good morning. My name is Jared Cohon. I'm the Chairman of the Nuclear Waste Technical Review Board, and I'm very pleased to welcome you to this spring meeting of the Board.

The Board meets three or four times a year, usually in Nevada, and most often in Las Vegas. but we also go to various towns and cities closer to Yucca Mountain. We try to meet at least once a year here in Washington, though we note that it's been more than a year and a half since we last met here.

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

In those same amendments, the Congress created our Board, and we were created as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. We're required to periodically furnish our findings to the Congress and to the Secretary, and we do this through Congressional testimony and reports. And, in fact, our summary report for the year 2000 was just issued about a week ago, and it's available outside on the table. Now, it's such a handsome report, I want to make sure you see it, and I forgot it in my desk here, but I'm mobile with this particular mike, so I can keep talking. And here it is. Isn't that handsome?

We have a contest on the Board for picking the colors, and we reached a new low with this. I'm told that this is formally called by people in the graphics art business pea green, aptly named, I would say. You can make of that whatever you want to.

A little bit about the Board and its members. We want you to know who the members are because you'll be spending the next day and a half with us. The 1987 Amendments to the Nuclear Waste Policy Act specified that the President appoints our members from a list of nominees submitted by the National Academy of Sciences. The Act further requires that the Board be a highly multi-disciplinary group, with areas of expertise covering all aspects of the nuclear waste management system.

And now it's my pleasure to introduce the members of the Board. Let me start with me. All of us have full-time jobs. We're all part-time special government employees in our role as Board members. In my case, I'm president of Carnegie-Mellon University in Pittsburgh, and my particular background is in and my expertise is in environmental and water resource systems analysis.

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.

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.

Norm Christensen, I'll save his introduction until he comes back.

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. She formerly was Director of the Center for Innovation and the Environment at the Progressive Policy Institute in Washington, D.C. and Deputy Assistant Secretary in the Department of Interior, and before that, she was a scientist at the U.S. Geological Survey. Her area of expertise is groundwater hydrology, and she chairs our 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, also here. She's a former professor at the University of Texas in Austin, and is an expert in geotechnical engineering.

Richard Parizek is professor of hydrologic sciences at Penn State University and an expert 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 now vice-president at Shepherd Miller, and his expertise is in geochemistry.

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

Jeffrey Wong is Deputy Director for Science, Pollution Prevention and Technology, Department of Toxic Substances Control in the California Environmental Protection Agency. He's 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.

Many of you know and have had the pleasure of working with our staff, who once again are strategically placed guarding our left flank or right flank, depending on which way you're looking at it. Bill Barnard is executive director of the staff, and we hope you'll get to know him and the rest of our outstanding staff.

Now, I need to offer our usual disclaimer so that everybody is clear on the conduct of our meeting, what you're hearing and the significance of what you're hearing. Our meetings are spontaneous by design. Those of you who have attended our meetings before, and many of you have, know that the members of the Board do not hesitate to speak their minds. And let me emphasize that is precisely what they're doing when they are speaking. They are speaking their minds. They are not speaking on behalf of the Board. They're speaking on behalf of themselves. When we are articulating a Board position, we'll let you know. And I'm about to do that.

But before I do, let me introduce Norm Christensen, who has entered the room. Norm, would you raise your hand? I've introduced everybody else. Don't worry. You're not in the hot seat. Now that Norm has joined us, let me also introduce him.

Norm has served with great distinction as Dean of the Nicholas School of Environment at Duke University for the last ten years. He's the founding Dean of that school, and he's done just an outstanding job. He is stepping down after ten years as Dean. And as a former dean, I can tell you that ten years seems like fifty as a dean, especially of a self-sustaining, independent professional school like the Nicholas School of Environment. Norm will return to his faculty position at Duke and start a well deserved sabbatical year this summer. Norm's expertise includes biology and ecology.

Now, as I warned you, when individual members speak during the meeting, they're speaking their minds, they're not stating Board positions. But as I also just warned you, I'm about to state a Board position, something I did in the opening remarks in our last meeting in Amargosa Valley.

At that meeting, we took the opportunity to announce a Board position, and the statement I'm about to read follows up on that position that we stated at that meeting. So here we go. By the way, copies of what I'm about to say will be available, they're not at the moment, at the table outside. So you'll have to listen, but you can read it again later.

At that meeting in Amargosa Valley, I stated that the Board believes that the DOE should focus significant attention on four priority areas, each of which the Board considers an essential element of any DOE site recommendation.

The four areas are:

(1) Meaningful quantification of conservatisms and uncertainties in DOE's performance assessments.

(2) Progress in understanding the underlying fundamental processes involved in predicting the rate of waste package corrosion.

(3) An evaluation and comparison of the base-case repository design with a low-temperature design.

(4) Development of multiple lines of evidence to support the safety case of the proposed repository. The lines of evidence should be derived independently of performance assessment and thus, not be subject to the

limitations of performance assessment.

Those are the four things, and what I've just done is repeat what I said at the meeting in January.

The Board also enumerated several specific investigations and studies that could support, complement, and supplement the four areas. By pursuing each of the four areas, the Board believes that the DOE can increase the technical defensibility of its repository safety case, thereby providing a sounder basis for the site suitability decision.

In subsequent conversations with a number of parties, two questions kept arising in reaction to our previous statement.

(1) Why were the four priority areas chosen?

(2) In the Board's opinion, should work on all four areas be completed before the Secretary of DOE decides whether to recommend to the President that the Yucca Mountain site be developed as a repository?

Now, why, and do all four have to be done before the Secretary makes his recommendation?

Let me now provide the Board's answer to the first of those questions. Why were the four priority areas chosen?

Three of the Board's priority areas were chosen to improve the quality of performance assessment calculations, a key element of the repository safety case. Uncertainty is unavoidable when making projections over long time periods. The uncertainty may arise, for example, from poor estimates of model parameters or from models that have not been validated adequately. The uncertainty also can arise from an inability to anticipate important scenarios.

Furthermore, as the Board observed in its letter to Representative Joe Barton written last year, "It is difficult to know whether the assumptions and parameters used in the DOE's performance assessments are truly conservative, or how the combination of conservative, optimistic, and realistic estimates affects overall dose calculations and the uncertainties associated with those calculations." That's a quote lifted directly from the letter that we wrote to Chairman Barton.

By meaningfully quantifying the conservatisms and uncertainties, which is the first of the Board's priority areas, the DOE will give policy-makers a clearer idea not only of the expected performance of the proposed repository, but also of the likelihood that the performance can be counted on.

The second priority area is progress in understanding fundamental corrosion processes. Because the waste package appears to play a central role in isolating waste from the environment, fundamental understanding of corrosion mechanisms, especially the relationship between corrosion rates and increased temperature, is needed to ensure that this barrier will function as anticipated and that long-term extrapolations will be sound.

Although we have the understanding and empirical foundation to predict confidently whether the passive layers that retard corrosion of the waste package will remain effective over a hundred years or so, we appear to have much less empirical evidence or scientific understanding to extrapolate that behavior convincingly over many thousands of years. We have to go from a hundred years or so to many thousands of years. In short, the DOE still has a way to go before its predictions are persuasive.

The third priority area is an evaluation and a comparison of the base-case repository design with a low-temperature design. The waste's temperature is a major perturbation of the natural system, and temperature may affect the performance of critical engineered barrier systems. Low-temperature ventilated designs can potentially simplify performance assessment and reduce uncertainty. Thus, it is highly desirable that repository designs having different thermal characteristics be understood better and that a comparison of designs be made both for the designs' expected performance and for the uncertainties associated with that performance.

The fourth priority area, the need for multiple lines of evidence, arises from the need for alternatives to the performance assessment methodology. Although the Board has endorsed performance assessment as an important element of the repository safety case, it observed in a 1997 letter to the DOE that, for each of the components embedded within a performance assessment, "methodological and empirical assumptions have to be made. Thus, uncertainties will unavoidably accumulate. They will be large, and they will become even larger as the time horizon for the performance projections reaches farther out into the future."

For this reason, one must view with caution the conclusions generated solely by performance assessment. Indeed, in its 1999 report on DOE's Viability Assessment, the Board noted the limits of performance assessment and expressed doubt that relying "solely on it (performance assessment) to demonstrate repository" will ever be possible. therefore, the Board consistently has recommended that additional lines of evidence be used to overcome performance assessment's limitations and to increase confidence in performance assessment's conclusions. The more these lines of evidence are independent of performance assessment, the more likely they can be used to bolster the assessment's conclusions.

Now let me address the second question. In the Board's opinion, should work in all four priority areas be completed before the Secretary makes a recommendation about developing a repository at Yucca Mountain?

The Board has observed that the decision to proceed with a Yucca Mountain repository can be made at any time, depending on how much uncertainty policy-makers find acceptable. There is, of course, no universally accepted uncertainty threshold. Any given level may be tolerable to some, but unacceptable to others. Thus, this is a matter of policy, albeit one that needs to be grounded in sound science. Policy-makers, not scientists, should make the decision.

The DOE may decide to make a recommendation about Yucca Mountain before it completes all work in these four priority areas. The Board, however, believes that it is reasonable to assume that the more those investigations have advanced, the more likely it is that the technical basis for the decision will be strengthened. Whenever a recommendation is made, the Board's judgment about the technical basis will be based on the repository safety case as it exists at that time.

That's the end of the formal statement. As I said, copies will be available later.

Let me now turn to the remainder of this meeting. And as all of our meetings seem to be lately, this one is of particular significance, and it is so because the DOE is in fact preparing its recommendation on whether or not to proceed with the development of Yucca Mountain as the site for a radioactive waste repository. This represents the culmination of many, many years of hard work by DOE, and we'll be hearing from Lake Barrett, the Acting Director of OCRWM, who will provide an overview of the program and discuss what the program will be focusing on over the coming months.

After Lake, we'll turn to the technical content of the meeting. Stephan Brocoum from the Yucca Mountain Project Office and Jerry King from Bechtel SAIC will describe some of the new work that has been undertaken partly in response to the Board's four priorities that I mentioned before.

You may recall, and we hope you will, if you attended our meeting in January, that there we started a new practice of conveying to the DOE very specific questions about aspects of the program, and used that then to guide and provide an outline for presentations by appropriate people from the program. We're continuing that format today for part of the program. It worked well for us, we think, and for the program and for the audience at the January meeting.

Larry Trautner will be talking on repository design in response to some specific questions we advanced. We'll have interspersed in the meeting the more traditional format that is more open ended without having specific questions posed, and Abe Van Luik will revert to that format in talking about this issue I just raised about multiple lines of evidence. And we'll hear from Bill Boyle, who will talk to us about DOE's efforts to analyze uncertainties and conservatisms.

We'll return to the directed question format after that when we hear from Saxon Sharpe and Jerry McNeish, who will be talking about long-term predictions of climate and how they're incorporated into performance assessment. Robert Howard and Robert MacKinnon will then talk to us about how possible differences between the design and actual fabrication and emplacement of components of the EBS are analyzed, those differences are analyzed in performance assessment.

And then, finally, to end today, we will be hearing from Joe Payer from Case Western Reserve University, who will be heading up a new waste package peer review for the DOE.

Tomorrow, we'll begin with Mark Peters from Los Alamos, who will give us an update on the scientific and technical work that the program has been pursuing, and from Narasi Sridhar from the Center for Nuclear Waste Regulatory Analysis, who will tell us about their work on corrosion-related activities.

And then we'll have a panel of people who will talk to us about the results and their interpretations of those results related to the study of the ages of fluid inclusions at Yucca Mountain. We'll hear from Jean Cline, the principal investigator of that study, from the University of Nevada at Las Vegas, Yuri Dublyansky, who is a contractor for the State of Nevada, Joe Whelan from USGS, Robert Bodnar from Virginia Tech. and a consultant to this Board. And then Bill Boyle from the DOE will suggest to us how this study might be used by the project.

As the topics of the meeting suggest, the DOE has gone to great lengths to address the Board's questions about the studies at Yucca Mountain. This is very encouraging for us, and we're very appreciative, and we look forward to reviewing the DOE's findings and conclusions in the coming months.

And speaking of the coming months, there will be a very busy time for the Board. On June 20th and 21st, the Board's Panel on the Repository and the Panel on Performance Assessment, both of which are chaired by Dan Bullen, will hold a joint meeting in Las Vegas. The meeting is timed to coincide with, or follow shortly, the release of DOE's study on unquantified uncertainties and other documents. Dan will have more to say about this later in the meeting for your information.

The second meeting we'll be holding will be an international workshop. We expect it's going to be held July

19th in Salt Lake City, but the planning for the workshop is still in progress and we've not yet finalized this, that is, the date. It will be held under the aegis of our Panel on the Repository. The topic is going to be the prediction of the long-term behavior of the passive layer, and we very much hope that this workshop will not only improve the Board's basis for commenting on this critical issue, but also complement the new waste package materials peer review that's being led by Joe Payer, and about which we'll be hearing at the end of today. Alberto Sagüés, our Board member, will have a few words to say also about this workshop following Joe's talk today.

Finally, let me just say a few words about public comment, something that's very important to this Board. We provide as many opportunities as possible for comment. There's a public comment period at the end of today and at the end of the meeting approximately mid-day tomorrow. We ask all those who would like to comment to sign the Public Comment Register that's located outside. Linda Hiatt and Linda Coultry sitting at that table will help you if you need the help. And, as always, we have to reserve the right to limit the time any single commenter has, depending on the number of people who sign up to comment and how much time we have left.

We will provide, as we have in prior meetings, an additional opportunity for people to submit written questions that we will then try to read, address to the speaker, during the meeting itself, that is, before the public comment period. If you have such a question, please write it down and give it to Linda Hiatt or Linda Coultry, and they'll give it to the chair of the meeting at the time. If the chair does not have the time to pose the question during the meeting, then we'll pose that question during the public comment period that follows that portion of the meeting. And, as always, we welcome written comments for the record. That's especially advantageous if you have a long comment that would be more appropriate submitted in written form for the record.

With that, welcome again, and I'm pleased to welcome Acting Director Lake Barrett, who will give us an update. Lake?

BARRETT: Thank you. Good morning. Welcome to Washington. Although it's very easy for me for you to have Washington meetings, I really much prefer the Nevada meetings, because that's where the bulk of the work is really going on in this very, very busy time.

This meeting certainly is a timely one, as we approach key decision points in the repository development process described by the Nuclear Waste Policy Act.

Over the past decade, we have met many times and discussed many issues. During those meetings, we've pointed toward a single objective: supporting a national decision on disposal of radioactive waste potentially at the Yucca Mountain site. We believe we are nearing that objective.

After we complete our present task of developing and strengthening the sound scientific basis for that decision, the Secretary of Energy, the President, and the Congress must decide whether to make a decision to move on to the next stage, and it is only the next stage, it is not an ultimate decision to close the repository. Their choices will be to permit proceeding with further development and submission of a license application to the Nuclear Regulatory Commission for the potential repository, or to adopt another unknown approach for meeting our national and international nuclear waste management obligations.

At your meeting in January, I informed you of former Secretary Richard's decision not to issue the Site Recommendation Consideration Report until the Department's Inspector General investigated into whether bias may have compromised the integrity of our evaluation of the Yucca Mountain site. That investigation is now complete, as you know, and the Inspector General has released his report that concluded there was no evidence to "substantiate the concern that bias compromised the integrity of the site evaluation process."

The Inspector General's report, however, also noted that four statements in a note to reviewers in the text of an early, never used, working draft Overview stated that "could be viewed as suggesting a premature conclusion regarding the suitability of Yucca Mountain." That concerned us, as some other remarks in the report as well, which we are presently evaluating.

It is my firm belief, Secretary Abraham's belief, and the Department's policy that all federal, laboratory and contractor employees must perform their work in a manner that reflects the integrity and objective approach necessary to conduct world-class science. I have demanded that all program participants remain vigilant in ensuring that we perform our work without any preconceived opinions or bias. In addition, we must ensure that our work does not raise the perception of possible bias. Public trust in the fundamental processes of government is crucial to the fulfillment of the Department's mission. I have asked that all of us who work in the program reaffirm our commitment to a site suitability evaluation process that is objective, unbiased, and based on sound science.

It is also important that our suitability evaluation process and the supporting science program not be inappropriately influenced by schedule considerations. The program has made tremendous progress, in my opinion, over the past several years, despite the funding shortfalls that we've had to endure. The progress we have made has contributed to a substantial momentum to discharge our generation's responsibilities for achieving the key milestones this year. I recognize that constrained funding can create a pressure to avoid any possible loss of momentum. However, achieving milestones must be predicated on appropriate, transparent, and defensible scientific technical work. Therefore, I have also directed formally to our Federal and Contractor management team to ensure that our planning decisions do not adversely impact the credibility of our scientific and technical conclusions.

Now, after almost twenty years of intensive investigative science to prepare a technical basis for making the next decision, we are implementing the next step in the long process. Last Friday, May 4th, we initiated the formal site consideration process with the release of the Yucca Mountain Science and Engineering Report. The Science and Engineering Report summarizes information and data collected to date in our multi-year study and the characterization of the Yucca Mountain site. The Department intends for the report, and its supporting documents, to be part of the technical basis for a site recommendation consideration, and to be used by the public as an aid in providing comments.

As the Board is well aware, the technical and scientific analyses are continuing. It is our intent to make the extensive information developed by the Department on the Yucca Mountain site available in stages, so that the public and interested parties have ample time to review all the available materials and to formulate their comments regarding a possible site recommendation by the Secretary.

Late this spring, we will strengthen the technical basis with the supplemental science reports that should provide a sufficient basis for the next incremental step. That step would be to issue a Preliminary Site Suitability Evaluation in the summer, and at that time, schedule the statutorily required hearings to inform and receive comments from the residents living in the vicinity of the site.

In addition to the release of the Science and Engineering Report last Friday, we released the Supplement to the Draft Environmental Impact Statement for Yucca Mountain. The Supplement evaluates potential environmental impacts that could occur, based on the design options and range of possible operating modes present in the Science and Engineering Report. The Supplement compared the impacts associated with the flexible design described in the Science and Engineering Report to the impacts presented in the Draft Environmental Impact Statement which was written back in July of 1999.

The additional program documents to update the

Total System Life Cycle Cost and the Nuclear Waste Fund fee adequacy report were also released. These documents provide the public and all interested parties with important information as we initiate the formal site consideration process.

We will consider the comments we receive during this process before making any decision whether to recommend the site. The Department is committed to making progress, but we will ensure that sound science governs each step and each decision as we go forward. For us to proceed further, the underlying scientific basis must demonstrate that the repository can operate safely, with adequate protection to public health and safety, and also the environment. The public's views on the validity of this work will weigh heavily on any decision by the Secretary on whether to forward a recommendation to the President.

While we are proud of our recent achievements, we recognize that we have additional work to do to strengthen the technical bases to support the next steps. Your recent communications, both in letters and discussions during meetings, has been very helpful in identifying and prioritizing this work. In particular, we appreciate the Board's feedback during the January meeting in Amargosa Valley. I am encouraged by the progress we have made this year in improving our communication with you, and am pleased with the positive reaction we received with respect to our efforts to address and resolve specific questions you have posed. We intend to continue to strengthen this communication process and address those areas where the Board has requested further information. Consistent with your observations, we recognize that we need to continue to provide information on investigations as they advance and strengthen the technical bases for all decisions regarding a possible site recommendation.

Your recent letter reiterates the Board's priorities for improvements to our technical program. In response to the concerns of the Board, we continue to implement and refine our plans and our activities for the additional technical work. Much of that will be discussed here in the next day and a half.

Our work remains focused on the four areas that you have recommended: (1) the meaningful quantification of conservatisms and uncertainties in the performance assessment; (2) progress in understanding underlying fundamental processes involved in predicting the rate of waste package corrosion; (3) an evaluation and comparison of the base-case repository design with low temperature designs; and (4) further development of multiple lines of evidence to support the safety case, the lines of evidence being derived independently of performance assessment and thus, not subject to the limitations of the performance assessment.

Our recent response to your communications last week describes our approach to addressing these priority concerns. We paid particular attention to providing details regarding our plans for evaluating and comparing designs in recognition of the importance of that design flexibility issue. Much of this information will be presented and discussed in the next day and a half in the context of the specific questions in the format of the meeting, which I think is very helpful to us. I look forward to further feedback from the you in the next day and a half.

We have made considerable progress to strengthen our technical bases and, despite enormous challenges, maintained the essential momentum to implement our Nation's policy for the management of spent fuel and high-level radioactive waste. We believe we have conducted a world-class investigative science program to determine whether the Yucca Mountain site is suitable for further development. We have now reached the next step in the process, and we have initiated the formal site consideration process.

Your constructive feedback on our activities is important to us to assure that we provide the decision-makers with a sufficient technical basis to support the next decisions in this program. I believe the Board's recommendations have led to a further strengthening of our technical program, especially toward influencing the evolutionary, stepwise design process and the analysis of the uncertainties for each of those steps. The stepwise development of a geologic repository, with the design and operational flexibility and reversibility, coupled with continuous learning feedback loops, is essential and important for a first-time endeavor like this. We have begun the science based site consideration process, as a part of the steps required under law to develop a geologic repository and hopefully to fulfill our generation's responsibilities and begin waste acceptance in 2010.

We continue to operate the program in an open and transparent manner, worthy of public confidence and trust. I believe that after 20 plus years, we are in a position to achieve important national and global decisions later this year.

I thank you for your attention, and would be pleased to address any questions that you may have for me.

COHON: Thank you very much. Questions from the Board?

Lake, would you comment on the budget outlook for the program?

BARRETT: Yes. We've requested $445 million, which was an increase in the Department, which was very good relative to other segments in the Department of Energy. I expect there will be a very difficult budget cycle for all involved, both the committees on the Hill, and the Department itself. We have our hearings starting tomorrow in the House, and the Senate I believe is Thursday, though that may be moving to next week. It's going to be difficult. There are many reductions that were taken.

We feel that we were successful within the internal reviews within the Department, which are sometimes the more difficult ones, the ones that are in the family, where the Secretary had to balance the needs of conservation, cleanup, national defense and ourselves, and we did well, relatively speaking. We had a lot of catch-up to do. We have deferred tremendous amounts of engineering activities for the license application in the preclosure area. We have focused pretty much exclusively on the postclosure period, which is appropriate, and I don't feel badly about that. But we've got a lot of catch-up to try to not allow the license application to slip any further.

So with the bulk of that money would be to do the catch-up for that. Also, with a decision on what we're going to do with Yucca Mountain toward the end of the year, that goes into the 2002 period. So we'll see how it goes, but I expect it will be a difficult cycle for everyone.

COHON: Dan Bullen?

BULLEN: Bullen, Board.

Lake, could you comment a little bit about the

Yucca Mountain standard and your understanding of the timing of when there might be a standard for Yucca Mountain specifically based on the release of the final Environmental Impact Statement?

BARRETT: Well, the Administration, and this is led by Administrator Whitman, is working on the standard, and the Administrator is personally involved in that. I don't go to those meetings. I know they are working on it, and addressing that. Exactly when they will reach a conclusion, I don't dare predict. I can tell you the process. Once the EPA makes their decisions, then the NRC would need to make their decisions to conform their regulations, and then we would just follow the two. It has been our goal to have our standard in place for any potential hearings, which could possibly take place in the summer. It must be done before the end, in my opinion. I don't know what that schedule would be, and we'll just have to wait those developments.

BULLEN: Thank you.

COHON: Debra Knopman?

KNOPMAN: Lake, could you tell us whether anyone from the program or the Department or the M&O has spoken with the Vice-President's task force on energy policy, or has been asked to speak or present material?

BARRETT: We have not, none to my knowledge, let me phrase it that way. I have not spoken to the task force. I know people on our Seventh Floor and the Secretary's personal staff have. I know this subject has been brought up in discussions, but none of us have ever made a presentation, to my knowledge.

COHON: Any other questions?

(No response.)

COHON: Thank you very much, Lake.

BARRETT: Thank you.

COHON: Norm Christensen, Board Member, will now take over as chair of the meeting.

CHRISTENSEN: Good morning. Our first presentation in this session this morning will be consideration of the revision of the FY2001 work plan for the Office of Civilian Radioactive Waste Management. The presenters will be Steve Brocoum, Assistant Manager for the Office of Regulatory and Licensing Compliance at the Yucca Mountain Site Characterization Office, and Jerry King, Project Manager for Site Recommendation with Bechtel.

Steve?

BROCOUM: Okay, I'm going to talk a little bit about the path forward to a possible site recommendation, and then I'll get into the planning and how it relates to that.

So the next viewgraph says the path forward, the site recommendation document structure and the process as we understand it today, the purpose of our fiscal year 01 replan and our FY 02 re-plan.

We have announced, DOE has announced the initiation of a public comment period on the possible site recommendation of the Yucca Mountain site for development as a geologic repository.

We have released the Yucca Mountain Science and Engineering Report to facilitate public review and comments. The Yucca Mountain Science and Engineering Report, the associated AMRs and PMRs and the TSPA and other supplemental information provide the technical basis for the evaluation of a site suitability and meet the intent we hope of the Nuclear Waste Policy Act and Amendments, Section 114. Description of the proposed repository include preliminary engineering specifications, description of the waste form and packaging, and relationship between the waste form and packaging and the geologic medium, and of course discussion of the data obtained in site characterization relating to the safety of the Yucca Mountain site.

We also have released the Supplement to the Draft Environmental Impact Statement. That addresses the evolution of the potential repository design, reflecting the various design options and operating modes that could reduce uncertainties, improve long-term performance, and operating safety and efficiency. It presents the potential environmental impacts based on our evolving design concept and it provides for public review of changes in these potential impacts. We have copies of a Supplement to the Draft Environmental Impact Statement on the table, and I understand we will soon have copies of the Executive Summary, Science and Engineering Report. They're on their way. And those Executive Overviews have in them a CD that has the whole document.

This summer, we will issue additional information that the Secretary will or may use in his consideration that will include the results of ongoing sensitivity and uncertainty analyses, and they will be presented in a report called the Supplemental Science and Performance Analyses (SSPA). That has two volumes; Volume 1, which is Scientific Bases and Analyses, has all the technical information, and Volume 2 has that information and how it affects performance.

After the release of the SSPA, we will then release the Preliminary Site Suitability Evaluation (PSSE) based on the methods and criteria of DOE's proposed suitability guidelines, proposed 10 CFR, Part 963, and that's based on the Science and Engineering Report, and all the other information.

When we release the PSSE, DOE will also announce the dates and times and locations for the public hearings on its consideration of Yucca Mountain, and the date for the close of the public comment period. So the comment period on the Science and Engineering Report is an open ended comment period at this point in time.

Since we acquired additional information to enhance the technical basis for a possible site recommendation, we have expanded, or maybe a better word would be extended the site recommendation process.

We are hoping to provide the Nuclear Waste Technical Review Board, the public, the NRC, and other interested parties and stakeholders time to review available materials and formulate comments regarding a possible site recommendation. And we are releasing information as it becomes available in stages to facilitate that.

The next viewgraph shows the pyramid, somewhat updated. The bottom part of this pyramid shows all the kinds of detail reports that we've collected over the years that form the technical foundation of our program. The middle part of this pyramid shows more or less the reports that pull all this information together, for example, the TSPA, SR and the process model reports, analysis and model reports. The part of the diagram surrounded by the black forms what we call the comprehensive basis for a possible recommendation by the Secretary. That will consist of the science and engineering report, the site suitability evaluation, comment summary document, NRC sufficiency comments, and finally IS, along with the response document, and the fee adequacy and

TSLCC.

If they decide to go forward, he may issue a potential secretarial recommendation, and if the president decides to go forward, he may issue a potential presidential recommendation.

This diagram tries to show just in kind of a logical flow the sequence of events. On the 4th of May, we issued the supplement to the DEIS, and the Yucca Mountain Science and Engineering Report. We have an open ended comment period on the science and engineering report. The close of that comment period will be announced when we issue the preliminary site suitability evaluation and notice and have the hearings.

We expect to receive sufficiency comments, and if appropriate, the secretary will make a decision and notify the state.

The supplement to the DEIS was issued also May 4th. That has a 45 day comment period that starts this Friday the 11th of May, and ends I think it's June 25th, and the hearings will occur in the vicinity of the site around the 1st of June.

The bottom just you the key technical activities that are going on. We had our model uncertainty workshop earlier in the year. We initiated our waste package corrosion peer review. We initiated our TSPA peer review, and we'll be issuing this summer the supplemental science and performance analysis, the two volume report that I mentioned.

Now, we had originally planned to release a two volume site recommendation consideration report in late 2000 to initiate the site recommendation process, but deferred the release to allow two thing. One, the enhancement of the technical basis for a site recommendation, and second, the completion of the Inspector General's report.

Also, oversight and stakeholder comments indicated a need for a broader and more robust technical basis, and that's what we've developing now and we hope to present in the supplemental science and performance analysis.

So the work has been replanned. And in the next few viewgraphs, we'll talk about the replan.

We are now in the midst of approving an updated plan for fiscal year 01 with this revised approach to site recommendation, and we're currently reviewing within DOE for acceptance. That plan identifies a possible SR decision in early fiscal year 02, and a possibility, if the site is deemed to be suitable, is submitted to the NRS in 2003.

This plan includes analyses and documentation needed to enhance the technical basis for a possible SR. It builds on TSPA Rev 0, ICN1, which I believe was issued late last year, December, 2000, and compares the results, and it builds on the evaluation of a flexible design over a range of thermal operating temperatures. And it emphasizes what the effects on performance would be across that range.

The key thing we're trying to do in this replan is to integrate into our work all the TRB concerns, the four key concerns, and the KTI, the key technical issues from the NRC, so that this work, the TRB concerns are not add-ons, but they're integrated fully into our work.

So the plan attempts to address the key TRB issues, for example, the meaningful quantification and conservatism and uncertainties in our performance assessments. That will be addressed in the supplemental science and performance analysis, and in the international TSPA peer review.

Progress in understanding the underlying fundamental processes involved in predicting rate of waste package corrosion will be address in our waste package peer review report and in additional long-term testing.

Evaluation and comparison of the base-case repository design with low-temperature that focuses on the following consideration. The repository design parameters and thermal operating modes. Those have been described in the supplemental science and engineering report. The basis for using the process model reports over a wide range of temperatures, extrapolating from just the high temperature, will be addressed in the supplemental science and performance analysis Volumes I and II.

The effects of uncertainties over the range of operating modes will also be addressed in the supplemental science and performance analysis, Volumes I and II. And using the TSPA to evaluate the range of operating modes will be addressed in--have been addressed to some degree in the Yucca Mountain Science and Engineering Report, and will be addressed in the supplemental science and performance analysis.

Developing multiple lines of evidence to support the safety case that are derived independently of performance assessment will be addressed in the supplemental science and evaluation report, Volume I.

We do our planning in several stages. We're trying to put in place the plan for the rest of fiscal year 01. We are also planning for the next three years, fiscal year 02 and beyond, and that's the work that's going on right now. That guidance that we prepare through our contractor will emphasize continued work to address the Nuclear Waste Technical Review Board concerns, continued work to address the NRC KTI agreements. As you know, we have had at least ten meetings with the NRC where we've reached these agreements on what issues to address, key technical issues.

Completion of the site recommendation as appropriate, and revision of the technical basis for a potential LA, should the site prove to be suitable.

And this is the last bullet, how we go back and forth with our contractor when referring to that plan.

This was already mentioned, but a lot of the issues of concern to the Nuclear Waste Technical Review Board are being addressed today, and this is a list of people doing that. One thing I left off is the presentation of the International Waste Package Peer Review by Joe Payer. That's also being addressed today.

So, in summary, we have announced the initiation of the comment period for a possible SR decision. The science and engineering report, and the supplement to the DEIS are available for public comment. This summer, additional information will be made available. The supplement to the science and performance analysis will be issued, and shortly thereafter, the preliminary site suitability evaluation. At that point, DOE will announce the hearings in the vicinity of the site for a possible site recommendation and close the comment period on the science and engineering report.

We extended the process. We originally in our schedule had a possible site recommendation in July of this year, and that's been extended. We're updating our planning to fully incorporate into our work scope the Nuclear Waste Technical Review Board concerns and the key technical issues that have been identified by the NRC.

Of course, our ongoing testing and data analyses and design will continue to enhance our understanding of the site conditions. And beyond the supplemental science and performance analysis, if any new information is available, it will be released and made available to the public and to the Board.

Thank you. Any questions?

CHRISTENSEN: Debra?

KNOPMAN: Steve, would you clarify the process now for finalizing the now proposed suitability guidelines, 10 CFR 63, since your documentation is geared to the proposed guidelines as opposed to existing regulations?

BROCOUM: I'm not sure what the question is.

KNOPMAN: On Page 5 of your presentation, you say the preliminary site suitability evaluation is based on the methods and criteria in DOE's proposed suitability guidelines.

BROCOUM: That's correct.

KNOPMAN: All right. When are those proposed suitability guidelines being finalized?

BROCOUM: They have been submitted to the NRC for concurrence. The NRC has stated publicly that they were waiting for the EPA to finalize their guidelines, and then they'll finalize their regulations, and then they will concur on our guidelines.

However, from our perspective, that does not prevent us from issuing our preliminary site suitability evaluation, since it's only preliminary and it will be based on those guidelines. That schedule is not under our control. That's under EPA and NRC's control. So we would like them as soon as possible, and we stated that.

KNOPMAN: So just for clarification, the EPA standard is in the critical path of all these, finalizing all these documents, but you will proceed with the public comment period on--

BROCOUM: The EPA's interagency review in January, it's still an interagency review. We'll go as far as we can absent the final regulations. We believe we can issue the site suitability evaluation, go that far. What we do after that depends on the state of the regulations. If the regulations have a surprise, in other words, if they're different than the proposed, then we will of course have to go back and reassess, do more work or issue more work, or have another comment period even. But we'll wait and see what the final regulations are.

CHRISTENSEN: Dan Bullen?

BULLEN: Bullen, Board.

Steve, you mentioned the International Peer Review of the total system performance assessment. How is it going? When do you expect to have results? Will it be completed in time for the final SR decision?

BROCOUM: I think there will be a report this fall prior to the proposed SR decision. I don't know if it will be completed. Abe?

VAN LUIK: Abe Van Luik, DOE.

We spoke with both IAEA and NEA last week. They are mailing by snail mail the signature sheet for Russ Dyer to sign to note that there's agreement now after seven go arounds on the terms of reference. The IAEA is awaiting a purchase order. We are awaiting a grants application from the NEA. That's the way they would like to work it to maintain their independence.

As soon as those things are done, which we hope to be done in the next two to three weeks, we hope to, in about the second week of June, have a meeting in Las Vegas to orient them and present materials to them, and perhaps another meeting in August to answer any questions that they may have. By the early October, we hope to have a preliminary report with our major findings, and a very detailed report with all of our findings in about the February time frame. That's the way things stand right now.

CHRISTENSEN: Don Runnells?

RUNNELLS: Runnells, Board.

Steve, you haven't mentioned the revised repository safety strategy report. Is that going to be now a part of the SSPA report?

BROCOUM: No, that is not going to be part of the SSPA. that will be a stand alone document. I'm not sure exactly where that stands in the planning. I need to talk to Nancy Williams on that. But we would like to have that report in the fall.

RUNNELLS: In the fall?

BROCOUM: Yes.

RUNNELLS: Okay, thank you.

CHRISTENSEN: Richard Parizek?

PARIZEK: Parizek, Board.

There was a peer review report issued last week on the biosphere, and having read it, it seemed like there were some important points raised. One, it was complimentary to the program as to what was done. It also indicated other things that could be done to strengthen future biosphere considerations. And the question is to do anything for the future, does that mean for LA?

BROCOUM: Generally, new work means beyond this year anyway. Sure. I don't want to--Abe is here again.

VAN LUIK: This is Abe Van Luik, DOE, again.

Yes, we're very pleased to receive the final report. We haven't officially received it yet with a cover letter, but we will this week. What we intend to do as per our procedures is to write a reply to what we have received, and what we will do is categorize those things that we can do right away, such as some sensitivity studies that were recommended, looking at the impact of using ICRP 72, as was recommended, and then also prioritizing for future work those things that we can do within the next year and those things that will take a little bit longer.

But I think basically I'm very pleased with the content of that report. I think if we do a number of the things that they recommend, we definitely are on the Vanguard of the world's advancing method of dealing with the biosphere.

PARIZEK: Quite a few of the points were not mandatory, but just recommendations for you to decide whether you would or would not go forward with them.

VAN LUIK: Exactly. Yes.

PARIZEK: But the KTI process is also of interest in terms of the number of things to be dealt with. It sort of depends in the next three years on budgetary considerations as to whether you can really do all of the things that you need to do to focus on KTIs? And I guess it's almost inferred that you will do all those things.

BROCOUM: Well, I think we need to resolve all the KTI issues. That's kind of the basis behind all these things with the NRC. So we, in my view, could not submit our LA until we resolved all the KTI issues.

PARIZEK: And that's budget dependent in fact, too?

BROCOUM: That is, in part, budget dependent; that's correct.

CHRISTENSEN: Steve, thank you. Let me suggest that Larry go ahead with his presentation, and then if we have additional questions for either--Jerry, pardon me--for either of you at the end, we can come back.

KING: Good morning. I'm going to give you just a very brief overview of the FY 2001 plan, the process for developing that plan and approving it, a summary of the key elements of what we're calling our revised site recommendation approach, which runs throughout the plan and the presentations, you'll be seeing today and tomorrow, a quick overview of plan site recommendation documentation, which Dr. Brocoum has already touched upon, and then I'll get into the meat of the talk on the FY 2001 workscope, or the planned workscope. And I tried to organize the presentation of this around the Board's four key issues, quantification of uncertainties, corrosion, lower-temperature operating modes, and multiple lines of evidence. And then, finally, a summary of the revised SR approach.

Bechtel SAIC over the last three or four months basically did a complete replan of the technical work for fiscal year 2001, and submitted that to the Department of Energy on April 30th for DOE's review and approval. As I said, this was pretty much a complete replan focused on the

Board's four key issues and on the NRC's KTIs. The replan was not only identifying the workscope, but developing an integrated project schedule with all the logic ties between the activities and resource loading of those activities, submitting thousands of elements in that schedule. But now that we have it in place, it enables us to answer "what if" questions.

The plan focuses on the remaining analyses and documentation needed to support a possible Secretarial decision on site recommendation by early fiscal year 2001. It does reflect a revised SR approach, which I'll describe in a second. And it also includes high level planning for work beyond site recommendation to support the completion of a license application if the site is recommended and designated.

Okay, revised SR approach. The keystone of the SR approach is based on a flexible repository design that can be operated over a range of thermal operating modes. And Larry Trautner will be talking about this in some detail later on today. It builds on the total system performance assessment that was documented in TSPA-SR Rev 0 Interim Change Notice 1, which assumed a higher temperature operating model. Higher temperature is relative. It was still a lot cooler than the design that was in the viability assessment, but it forms sort of the high end of the spectrum of the potential operating modes we're considering now.

It evaluates repository performance across a range of temperatures, ranging from a heat load that would boil the wall rock about halfway into the pillars between the drifts, down to a low range that would keep waste package surface temperatures below about 85 degrees C., and develops design details as needed to support those performance evaluations, and to look at the feasibility of design and constructibility of those lower temperature operating modes. And it defers most of the other design detail development work until after the site recommendation, and if the site is recommended and designated, those design details will be developed consistent with the license application design.

As I mentioned, the revised approach addresses, or at least we hope it addresses the Board's four key issues: meaningful quantification of conservatism and uncertainties in TSPA, progress in understanding fundamental processes in corrosion rates for the waste package, evaluation and comparison of the base-case design with a low-temperature design. And as the Board is well aware, we're actually answering a somewhat different question than the one that you asked. We're answering the question of how would the repository operate with a single flexible design that can be operated over a range of thermal operating modes. We trust that that's going to be responsive to the Board's concerns, and we'll be talking about that quite a bit more today. And, finally, multiple lines of evidence for the safety case that are derived independently of TSPA.

So we have these four key Board issues and the NRC's key technical issues in front of us all the time as we did the replan for this year, and attempted to make sure that we addressed all of them.

SR documentation. Dr. Brocoum has already mentioned the science and engineering report, which we issued last Friday. It updates site and design information since the 1998, December, viability assessment, and it formally kicked off the final site recommendation decision process by announcing the secretary's consideration of the site and DOE's intention to hold public hearings.

Simultaneously with the science and engineering report, the Department issued the supplement to the Draft Environmental Impact Statement, which was itself updated, updates the draft Environmental Impact Statement to consider the range of thermal operating modes, and that kicked off a 45 day public comment period starting from this Friday.

Dr. Brocoum also mentioned the supplemental science and performance analyses, SSPA, which under the current plan, will be issued this summer. Volume I of that document describes the new science that is being incorporated into the TSPA model to provide input to sensitivity studies. That new science includes a number of things, including a new long-term climate model which Dr. Saxon Sharpe will be talking about. It includes an updated seepage model and a number of other things that Bill Boyle will be talking about in his presentation. It includes the description of alternative and usually less conservative and more representative process models, with revised ranges of uncertainties, and descriptions of how those process models were modified to reflect the potential effects of a cooler operating mode. Volume I has been drafted and is under technical review right now.

Volume II, which is still under development, the sensitivity studies haven't been run yet, is going to take those inputs that are in Volume I, documented in Volume I, and will run the TSPA model to perform sensitivity studies that work at the effect on performance of the alternative process models and revised ranges of uncertainty and the cooler operating modes.

You're going to hear quite a bit more about what this new science and new models are in talks following mine today and tomorrow. It will be touched on by Bill Boyle, Rob Howard, Rob MacKinnon and Saxon Sharpe.

Preliminary site suitability evaluation is also planned for this summer. The supplementary science and performance analyses is a key technical reference for this document, so that has to be done before this one can be issued. The SSPA, along with the science and engineering report, are the two key technical references for the preliminary site suitability evaluation.

As Dr. Brocoum mentioned, it's a preliminary evaluation against DOE's site-suitability guidelines in proposed 10 CFR 963. It will evaluate repository performance over a full range of thermal operating modes, and it will be updated based on public comments and any changes to 10 CFR 963, if there are any changes to 963 that would affect the suitability evaluation.

2001 Workscope. As I said, I attempted to try to organize this under the Board's four key uncertainties, but some of this is a little arbitrary, and you stick it under there because some of the workscope items address more than one, but I tried to put it where it seemed to make the most sense.

There are unquantified uncertainties in the current TSPA model associated with a choice of conservative parameter bounds, conservative and some optimistic models and assumptions, and conservatively biased parameter distributions.

The conservative bias in the TSPA-SR Rev. 0 was intentional. It was done with the intent of ensuring the defensibility of the outputs of that model. But there is an interest in understanding what do we think the site really would do with our best estimate? What is the impact of putting more representative models in? And what's the impact of more fully identifying a full range of uncertainties? What does that do to performance projections? So that's what we're attempting to address in the supplementary science and performance analysis.

The steps in getting to that is first to review the treatment of conservatisms and uncertainties that are in the existing TSPA-SR. That review has been done, and will be just summarized for you later by Bill Boyle. The second is to assess the unquantified uncertainties in the TSPA model inputs, which was done through a series of expert elicitations. Then to conduct component-level analyses of these uncertainties and to identify their significance. Bill Boyle will be presenting a talk on the interim results of this effort later on today.

The unquantified uncertainties there in the third bullet encompasses a large range of inputs, including uncertainties in the seepage model, the possibility of a drift shadow zone, which Bill Boyle will be addressing, changes to long-term climate model and net infiltration that Saxon Sharpe and Jerry McNeish will be talking about later, waste package and drip shield degradation, which Rob Howard will be talking about, and EBS transport that Bob MacKinnon will be talking about.

The uncertainties that are identified will be incorporated into the TSPA model and produce a supplemental TSPA using the TSPA-SR Rev. 0 ICN 1 model as the starting point, and that will be documented in the supplemental science and performance analysis report coming out this summer.

Following that report, we will continue to do work, including TSPA analyses on less significant uncertainties, and developing guidance for the treatment of uncertainties in the future analyses and modeling efforts. And the initial results from those efforts will be available at the time of the SR, although they won't be in the actual documentation basis for the SR.

In addition to the bullets I have here, I didn't quite know which item to put this under, will be continued testing that Mark Peters will talk about tomorrow, to include testing at Busted Butte on colloidal transport, and preparatory activities for multi-well alluvial testing down in Amargosa Valley, incorporating Nye County results on their saturated zone testing, and continued corroboration with the labs on trying to resolve the Chlorine-36 questions.

Okay, corrosion. The second bullet item to be included in our workscope is developing a conceptual model for passive film stability, identifying thermal and chemical dependencies of the long-term corrosion rates, looking at appropriate natural analogs that can give us some lines of evidence independent from the lab tests, and conducting short-duration tests and initiating--well, conducting the waste package corrosion peer review. And you'll hear where that stands later today by Joe Payer. And conducting additional testing and analyses to evaluate the corrosion degradation rates.

This testing, which Mark Peters will be touching on, includes analyzing dust for formation of hydroscopic salts, conducting phase stability studies, developing a thermal aging kinetic model, looking at microbial induced corrosion, more studies on stress corrosion cracking, passive film studies, and measurements of Alloy-22 and titanium corrosion rates.

The lower-temperature operating modes. I've lumped most of the work that we're doing under this heading. The first step here was to look at our requirements documents, design requirements documents, to identify potential conflicts with operating repository and a lower thermal mode, and there are a couple conflicts in there that had to be taken out. A specific example was there was a requirement in one of our requirements documents that the repository design showed that the repository could be closed as early as 30 years, or it must be designed so that it could be closed as early as 30 years. But you can't close it in 30 years and still emit some of the lower-temperature operating modes.

So we're making the changes to remove those conflicts, and then there will be a longer term effort to actually develop new and more detailed requirements in our system design descriptions for how the repository would be operated in lower temperature modes.

Workscope includes supporting the screening of design-related features, events and processes for lower-temperature operating environments. Bob MacKinnon will be talking about the results of that FEP screening in his talk. It includes conducting an engineering analysis of one representative lower-temperature operating mode to look at the design feasibility and constructibility of that. Larry Trautner will be talking about that.

It includes conducting parametric studies to explore ways in which lower-temperature operating modes could be achieved through variable design and operating parameters. Larry will be providing the details on that. And there's also other design work that's not directly related to thermal modes, including design work on the invert, the drip shield, seismic response and nuclear criticality.

Continuing on, the workscope includes identifying thermally dependent physical processes with the most potential impact on system performance, considering both model uncertainty and the ranges of thermal operating environments.

When the expert elicitations were conducted, the subject matter experts were asked not only, you know, what do you really think the range of uncertainty is, you know, what's your best estimate, other than a conservative estimate of this particular parameter or model, but also how would those estimates change as a function of temperature, if at all.

Review how thermal dependencies were incorporated into the existing TSPA model, development of alternative models that more fully encompass the range of possible thermal effects, and establishing whether existing abstractions for the process models are adequate and defensible over the ranges of operating environments. All that work will be documents in the SSPA this summer.

Continuing, the TSPA modeling work required development of numerical simulations of the thermal-hydrologic-chemical environments for the higher and lower thermal operating modes. And then what we call "one-off" calculations using the existing TSPA model with these updated inputs, including unquantified uncertainties and new science. So we'll take the existing model, we'll then take these modified inputs that would more fully reflect the uncertainties, reflect alternative, hopefully more representative estimates of those inputs, some of the new science, and see, conducting sensitivity studies, what does that do to the results. That's the first step in the PA analyses, and I'll get to the second step in a minute.

Then update the TSPA model to build a new TSPA model that actually includes the most important new science, most important meaning the science that's anticipated to have an actual effect on the outcome, including the long-term climate model, and the most important findings from the unquantified uncertainties work as informed by the sensitivity studies, and then to run this new full system TSPA model for both the higher and lower temperature environments. So all of that work that I've talked about there will be documented this summer in the SSPA report.

Following that, we will continue ongoing work, and starting some new work, including the initiation of in situ and laboratory testing to determine thermal rock characteristics. The most important rock characteristic is the thermal conductivity in the repository horizon. That will include both lab and in situ testing. Continue our laboratory ventilation testing to support preclosure projections of the environment in the emplacement drifts. It will also include some modeling of natural ventilation, looking at that possibility.

Continue model comparisons to observations from in situ coupled process testing. This testing includes both continuation of the drift scale test, the cross-drift thermal test, seepage test, investigations into fracture sealing, water and gas chemistry in the potential shadow zone. Mark Peters will be talking on more of the details of the testing program.

And then, finally, defining, developing and preparing to implement a systematic decision process to select the design parameters and ranges of operating modes for inclusion in the license application, if the site is recommended and designated. We will not have made the final design decision at the time of the final site recommendation decision, if the secretary makes that decision, early in fiscal year 02, but we would have a plan prepared that would detail how we would continue to evolve the design and on what time table and what the considerations would be in that design evolution.

Multiple lines of evidence. Dr. Van Luik will be talking about this in considerably more detail, but we're going to be documenting other lines of evidence that support our component models. We believe that there are a number of lines of evidence out there that we haven't, frankly, done a very good job of documenting and explaining to people. So we intend to do that. This includes documenting technical arguments based on multiple lines of evidence to support understanding of the natural and engineered systems, and the applicability of process models over extended ranges of temperature, and developing appropriate natural analogs that provide other lines of evidence related to corrosion mechanisms.

And there will be some site-specific natural analog studies that will continue, looking at data from Peña Blanca, the Mexican uranium mine, as an analog for a radionuclide transport, and looking at the Yellowstone site as an analog for thermal, hydrologic and chemical processes at Yucca Mountain. And a synthesis report on these ongoing analog studies is currently scheduled for November of this year.

Finally, a summary of revised SR approach. As i said, the cornerstone of it is a single flexible repository design that has the ability to be operated over a range of thermal operating modes. There are both design parameters that we have locked in at the moment for the purpose of analyzing the performance of our current design, but parameters which can be unlocked later, and operational parameters that can be varied even once the repository has been built. Larry Trautner will be detailing this in his talk.

It includes an analysis of previously unquantified uncertainties, both alternative models and parameter inputs, analyzing lower-temperature operating environments that would result in the in-drift environment. It includes a particular focus on waste package corrosion, because it is a key component of repository performance, and a better job of explaining and incorporating multiple lines of evidence.

And that concludes my presentation. I will be happy to entertain any questions.

CHRISTENSEN: Thank you, Jerry. Board members?

COHON: I have a general question about the various reports and studies and what state they'll be in and how they will affect the site recommendation, and then the Secretary's decision, and a specific question related to all that about uncertainties and the treatment.

Now, from Steve's talk and yours, but let me try to frame it this way, I imagine that there will come a time early in fiscal 02 where Lake is going to sign a memo and it will probably be signed by the Deputy Secretary and someone else, too, to the Secretary that says something like, just speaking hypothetically here, we think you should recommend Yucca Mountain, and then it's going to say why. Is there going to be something attached to that memo? And if so, what? I don't mean to trivialize this. The question is what is the Secretary going to base his decision on? And in that regard, is the site suitability evaluation report, is that the thing that will be attached to the memo?

BROCOUM: I don't know if it will be attached to the memo or not. But it's that area surrounded by black on the permit that's in front of you. In other words, that's the comprehensive basis for a site recommendation. So the Secretary will use everything that's in black, surrounded by black on that permit. That includes the suitability evaluation, and includes whatever else is in that that I went over before.

COHON: Okay. Of course we know that's thousands of pages of reports and CDs and stuff, so the Secretary can't reasonably read that, so let me use uncertainties as a specific example. One of the things that the Board has communicated with regard to uncertainty when it has interacted with DOE, both in writing and verbally, is that we've used the phrase, "meaningful quantification." I'm glad to see you've picked it up. But it also has to do with how that is communicated. So the question is what will the Secretary know about uncertainty? How will that be summarized and communicated?

And then the question I'm finally trying to get to, I mean I care about the answer to that question, but the related question is what will support that? And this goes then to what's available when, and how it gets used? And reading between the lines, let's see if this is correct; that if the Secretary is told something, or reads something about uncertainty, that whatever that is is going to be based on what you know now, on the results you have now. And that the results that you might generate between now and that recommendation will go into perhaps the SSPA, and may influence something after that, but not the Secretary's decision.

BROCOUM: Certainly we intend to include everything through the SSPA. The SSPA will come out first, and then we'll issue the preliminary site suitability evaluation, which will be finalized if the Secretary decides to go forward. We will also have other information, because the program, you know, keeps writing reports and keeps spending a million dollars a day, so there will be other information available, and that will be made available to everybody as we get it ready. But the intent was that the Secretary would base his evaluation on what's in those black lines, and each of those documents will have an executive summary. And we envision that the Secretary himself will issue sort of, you know, justification and his reasoning that he will issue to the President. That's how we envision the process right now. But basically, that's how we see it. And the Secretary will do what he wants to do. I mean, whatever he wants, we're going to give him. So if he wants briefings, summaries--

COHON: Yes, I understand that. And that's why I'm so keen on this issue of how uncertainty is communicated. Have you thought about how that will be communicated to the

Secretary?

KING: We have in our current planning basis a document we call a summary of the basis for recommendation, and we envision this being a Secretarial size document, i.e. 10 to 15 pages, that would summarize what's in the science and engineering report, the supplement science and performance analyses, the key arguments in the repository safety strategy. It certainly would have to touch on uncertainties and the meaning of those uncertainties. So we recognize we have to boil this down for review by the decision makers at the Secretary's level. So that's the current plan, and we will take a shot at doing that.

Your second question about the impact of additional work, we envision the bulk of the technical basis that the Secretary will consider will be completed by the supplemental science and performance analysis this summer. However, as Steve pointed out, our work isn't going to stop, but we have procedures in place that call for impact analyses of new information. So when new work is completed post-summer, but before the Secretarial decision, on an ongoing basis, we will be doing impact analyses of that work. And if anything does come up that has a significant, or looks like it could have a significant impact, then we would have to take appropriate action, incorporate that into the decision materials that the Secretary is considering.

Now, exactly how we will document that additional work and make it available to the public, that hasn't been decided yet. But we definitely will be doing the impact analyses.

CHRISTENSEN: We have questions from Priscilla Nelson, and then Dan Bullen, and then Debra Knopman.

NELSON: Nelson, Board.

My question relates to the concern about an appearance of maybe some departmentalization of these four issues that the Board put forth, and I'm wondering about--in particular, let me ask about two areas. One is dealing with this process of looking at flexibility. And it doesn't seem like there's an explicit way of actually seeking that feedback from what has been learned into the data prioritization. So I'm interested in that feedback, and I imagine the project has it, but it's not apparent in what we've heard thus far today.

And, second, what I was wondering about was maybe a higher order issue that processes these different areas. For example, there's a lot of discussion about corrosion and corrosion rates, but I'm constantly trying to understand what the project's conception is for the evolution of the drift environment with and without waste packages, outside and inside, and what the environment is going to do underneath the drip shields. Can you give me any input or tell me if someone today is going to be addressing those?

KING: Yes. Your second question, I think it's best to wait until Bill Boyle's talk, because he does go into exactly that, what is the evolution of the in-drift environment. The first question, feedback, we actually have a formal procedure called AP 3.14Q, which we use to transfer information from one organization that another organization needs. And in this case, Bob Andrews' organization, Science and Analyses, would make a specific request to Larry Trautner's organization, Design, that we need this information to run our next generation of PA models, and then Larry's organization formally transmits that, and it includes mechanisms to keep track of what that information was that was transmitted, and updating it if it changes.

I don't know, Bob or Larry, do you want to add anything, or, Rob, do you want to add something to that, how that feedback loop works?

HOWARD: This is Rob Howard, Integration Manager for Science and Analysis.

The first part of that question as far as the feedback loops go, as we're developing these analyses and looking at the results, you know, we have an opportunity in the next couple months to incorporate also into our planning process for next year any new type of scientific investigation or data needs that we need to get to address some of these issues that we discover through the evaluation of the different thermal operating modes.

So that's kind of what I wanted to add to that. The procedural mechanism is kind of the mechanical part of that, but we do use our noggins a little bit when we go into the planning process, based on what it is we learn from these calculations that we're doing right now. And that will occur in the summertime.

The other opportunity that you'll get to hear about evolution of the in-drift environment is when Bob MacKinnon answers the second and third questions on the engineered barrier system this afternoon, along with what Bill Boyle talks about seepage. We also have weekly Integration meetings with the design shop to make sure that we're communicating, we understand what they need for design and they understand what we need for our postclosure analyses.

CHRISTENSEN: Bullen, and then Debra, and relatively brief.

BULLEN: Bullen, Board.

A couple of quick questions. Could you go to Slide 5, please? You'll notice that you did one quick dodge here, because you have a caveat under the evaluation and comparison of the base-case design with a low-temperature design, which says you're going to address this by evaluating that single flexible design.

I guess the key question that I have is that if the goal were to--or one of the goals were to design a cooler repository, is this current flexible design the one that the program or maybe you would choose? Or what would you choose? And if it is, tell us why, and if it's not, how would you change it?

KING: Is this a trick question?

BULLEN: No. You may want to defer to Larry Trautner later. But I guess the key here is that it looks as though you're evaluating a single design, as you are.

KING: Yes.

BULLEN: And how would you change it if you really wanted to design a low-temperature design?

KING: Well, I think I will defer that to Larry. I'll just make one introductory comment. We are at a conceptual design stage at this moment. We wouldn't choose, certainly would prefer not to have to choose even the high-temperature design at this point, because the design details remain to evolve. But I really think Larry should probably address that.

BULLEN: Okay, we can defer that to later, and I'll ask the question again.

I guess the one other question I have that's also short, Mr. Chairman, is that we see TSPA-SR Rev 00, and then the changes that are going to be made to it. But in Steve's document when you see TSPA-SR, will there be a Rev 01, so that we can see how it changed, so that you can see a comparison between Rev 00 and Rev 01 in the TSPA calculations when you make the decision? I know that doesn't go into the black box that goes to the Secretary, but it seems to be a strong supporting leg to that.

KING: There's not going to be a Rev 01 per se, but there will be an updated TSPA model that will be documented in the supplementary science and performance analyses.

BULLEN: Okay.

KING: So you will see the updated model and its documentation.

BULLEN: Which will easily be documented so that we can see what changes were made, how it evolved? I guess what I want to know is how it changes, so we can see the comparison.

KING: Yes, it will be.

BULLEN: Thank you.

CHRISTENSEN: Debra?

KNOPMAN: Knopman, Board.

I hate to be the person blocking the break. Let me ask these questions real fast. Following up on Dr. Cohon's question, what do you tell a member of the public who wants a good overview of the Department's technical case for suitability during this comment period before its going to the Secretary? Where are you going to direct that interested member of the public? It may be a Congressional staff member, may be--well, it could be any number of people. What's the document? I mean, you've got five different things out there, all with executive summaries. What is the key integrating document available to the public that you will have?

KING: In this time period, I guess I would have to point them to the executive summary in the science and engineering report, and the executive summary we'll be preparing for the preliminary site suitability evaluation. I think those are the two documents that would come closest to performing that function.

KNOPMAN: Okay. And then quickly--

KING: We had an overview, but it met an untimely demise.

KNOPMAN: Yes, we know about that. Just quickly, the Board doesn't get into budget issues, and I'm not trying to do that with this question, but in the work plan, the revised work plan that you've just outlined for us, can you give us a rough idea of sort of the percentage of total FY 2001 work this represents, or total amount of sort of the part of the budget? You've just fiddled with 50 per cent of your remaining budget, or is this 5 per cent?

KING: It's probably closer to 50.

KNOPMAN: I'm just trying to get a sense of the level.

KING: Lake is saying even higher.

KNOPMAN: Higher than 50? Lower than 100?

HESS: Ken Hess, President and General Manager of BSC.

Let me give you a brief summary of what we did with this re-forecast.

First of all, we needed a firm basis as to what work needed to be done, and we did that through revision of schedules and manloading those work schedules. With our current organization, it's totally different than the previous contractor. The manager of project was a key to identifying what budget was required to satisfy the technical issues that we had to address for site recommendation, also to look at what work was being done for the license application, and what could we delay so that we could put that money toward the site recommendation.

One of the tasks that I had was to look at the entire project and see where did we have funds that we could redivert to three key areas. One of those key areas was the QA resources that we needed; secondly, the technical resources that we needed; and, third, to support some work that I thought needed to be done at the job site.

Where we found that money basically was a number of areas. First of all, DOE was able to get released some additional money that the Secretary had to request from Congress. That was about $10 million. DOE had performed a lot of work on the transition program for the project. We underran the transition by over a million dollars. DOE also allowed us to use some programs that we had available on other Bechtel projects. That saved us another half a million dollars. And then lastly, we also found money in the repricing of our contract structure versus the previous contract structure of about $10 million.

So, bottom line, what I needed was about $10 to $20 million. In fact, it was about $20 million that we needed to redirect to the project area, and $3 million in the QA area, and about $2 million in the field area. We were able to accomplish that through additional money that the Secretary got of $10 million. We had fee reductions and repricing that we did because of our contract structure of about $10 million, and then rediverting some of the other work necessary for license application to next year.

The other thing that we looked at hard was the work necessary for SR, did it all have to be completed before SR, or in this fiscal year. And we did move out, based on the availability of resources, some of that work. That also allowed us to get down to what we had to do in order to comply with this year's funding. That's basically what we did.

CHRISTENSEN: Thank you. And thank you, Jerry.

We will take a ten minute break, and reassemble here at 10 o'clock.

(Whereupon, a brief recess was taken.)

CHRISTENSEN: Our next presentation will be the first of a series today that will respond to questions that the Board has prepared. Let me read this question, and the presenter will be Larry Trautner, who is Project Manager for Repository Design with Bechtel.

It appears that the Yucca Mountain Project intends to evaluate and compare the base-case repository design with a low-temperature design by developing a "flexible" design that will then be evaluated for hot and cold operating conditions. What exactly does "flexible" mean in this context? What characteristics does the DOE use to determine flexibility? Is the current base-case design flexible? If so, explain why. If not, explain what would need to be changed. How much may a design be changed and still be considered the same design?

So, with those questions, Larry, we look forward to your presentation.

TRAUTNER: Thank you, Norman.

As norman indicated, I'm Larry Trautner. I'm the Repository Design Manager for Bechtel SAIC. I've been asked to give the design update today, and to focus on that specific set of questions.

Let me first apologize for my tone of voice. My sinuses have not enjoyed spring in the desert, and they're rebelling on me a little bit. So if you can't understand something I'm saying, please ask for a clarification.

I'll talk first about the need for flexibility in a general sense, then go into more specific questions, and again dwelling mainly on the first one that we're talking about, flexibility. I'll briefly talk about some engineering analysis that have been ongoing to support that effort, as well as what's next, and have a wrap-up conclusion.

The first thing we need to do obviously is establish a need for flexibility. And in a project like this that's science driven or science based, a key factor of the design has to be the ability to handle or to accommodate additional information that's generated.

This is a similar element in some ways to other first of a kind commercial projects or other science based driven projects where the customer or the owner, even after the feasibility of that new or unique process is proven, they still want to continue optimization. They still want to continue to work on that key process to design it to optimize the ability, the performance. So that optimization continues and ongoes after the decision is made to even implement it.

In parallel with that, the customer will also want to develop a design. They'll want to have a design for reasons of, well, licensing, regulatory requirements, usually the permit. The regulators will want to see more details in the design, and the design of other things, not just the key process. The customer or owner also is looking for additional cost information in terms of life cycle costing. And a lot of times there's a schedule driver in that. So in that respect, this project is similar to others.

And so in one case, you have ongoing testing, modeling and development, and in the other case, you have design that needs to be advanced to some extent. So there's a parallel nature of the two efforts, and it's interactive, it's an iterative nature, and I think there were some questions earlier that Priscilla had about that feedback. We are currently having weekly meetings, and we have a formal process that exchanges the information across, memos and things, but we routinely interact on that to make sure that the iteration is occurring.

So, the bottom line is that there is an absolute need. I can assure you this is my fourth major science driven project where I've been in a management role, and I can assure you that there will be new information coming out of testing and development that we'll have to be able to accommodate.

Now, there are other areas that require flexibility besides thermal. I'm going to focus today just on thermal because that is where the nature of the four questions were in terms of the flexibility as it relates to thermal, the thermal operation. I can conclude that the repository design needs to be able to operate, the design that we produce needs to be able to operate under a range of thermal conditions.

So as I focus in on flexibility of rates of thermal operating modes, what we mean in this context by flexibility, the first question is that flexibility in this context is the ability to control the thermal input into the host rock and the EBS systems, engineered barrier systems, the ability to control that thermal input into the rock, into the mountain.

How do we control that? By two sets of parameters; a set of design parameters, and a set of operating parameters. Design parameters, of course, are flexibility. They can be changed. The design parameter of a five and a half meter diameter drift, that's a design parameter. That can be changed during the design. Operating parameters, however, can be changed and are variable throughout the operations of the plant.

Let me get into more specifics on it in the next viewgraph. Here now we're controlling the repository with these parameters. When I'm referring to the design parameters, I'm using the term very broadly to include both design requirements, as well as design solution. So design parameters here would include things like the drift diameter, five and a half meters, the drift spacing, we'd show you'd keep that drift spacing during the SR phase at 81 meters. And, of course, when you combine the diameters and the spacing with other variables, you end up with a layout configuration.

Other design parameters. The waste package and drip shield designs are obviously key design parameters. They relate not as directly to thermal as some of the other parameters, but there is obviously impact on the drip shield. The amount of waste we're handling, 70,000 metric tons, obviously that affects the thermal input into the host rock. So that's a requirement, even though it still is--well, I'm going to define it here as a parameter--as well as the receipt and emplacement rates.

Another design parameter is the ventilation system, and by that I mean the diameter of the intake shafts and exhaust shafts, the exhaust mains, the fan configuration, we're going to have two large fans on each exhaust shaft, you know, for backup, and so on. That's what I consider design parameters.

Now, there's operational parameters, and I'll use ventilation as an example here because it's on both sides of that fence. The operator of the facility, of the repository, will have the flexibility to operate and to ventilate at different rates. It doesn't necessarily have to be at a given fixed rate, because you've got different pressure drops in the different drifts. So if it's a constant, he has to run more air through by adjusting the pressure drop across the dampers. So the operator has to have flexibility in his operations from a rate perspective.

From a duration perspective, obviously, they can operate those fans after closure for five years or fifty years or a hundred years, the only difference being is that maintenance and obviously replacement, periodic replacement of fans. But I consider that an operational parameter in the end. And, again, when you look at other operational parameters, there's the waste package spacing.

There's several elements that relate to I guess you might say the aerial mass loading. There's the heat load per unit of volume. That's covered by variables like waste package spacing, heat output per waste package. You can control that by blending the different temperatures. You know, some elements are hotter than others, so you can blend those, and the current design has that capability to blend and control it, so you can control the heat output per package.

You can de-rate the packages, make them smaller if you like, or put less fuel elements in each package. You can also look at the sequence of emplacement. And by that, I mean if you put in a commercial high-level waste canister, and the next one could be a high-level waste defense canister, the canisters are glass, the next one could be a Navy spent fuel. You have some control over that mass loading.

So in the operations area, there's really two main variables. One is the heat you remove, and you do that through ventilation, and controlling that ventilation, and the other main parameter is the aerial mass loading, and you control that by these parameters that are defined here.

Now, the bridge between design and operational parameters is that engineering will establish both. Engineering will establish the design parameters, but will also establish a range for the operating parameters. We call that in the engineering world technical specs, operational tech. specs, we call them. And that's true in most of the nuclear business. We will define, for instance, the ventilation rate, what the minimums and maximums would have to be through a drift. We will define waste package spacing, probably a minimum spacing and a maximum spacing. And so we will define ranges for operations, but operations as we see it will be given the flexibility to operate the repository over a range of thermal conditions.

Essentially what we're saying is these design parameters, when combined with the operational parameters I've defined here, really will end up, or may result in a different utilization of the layout configuration. The current layout configuration has about 148 kilometers of drift length in it. That's using the upper and lower blocks both, including the southern extension and the upper block, and that's, again, at 81 meter spacing. If we were to change that 81 meter spacing, and we may look at that subsequent to the SR, we could theoretically have more linear drift available.

So the next viewgraph really kind of in picture demonstrates this. If you look at the current repository layout in the center here, which has, again, the primary block with the southern extension and the lower block, that represents 148 kilometers, or about 2,900 acres of available space for the operations to utilize.

If you look at then the different kinds of operating modes, if you look at the upper right-hand corner, this represents essentially the acreage, for lack of a better term, that would be utilized in what was the base-case analysis up to last fall, which was the one-tenth of a meter waste package spacing, and operating the--50 per cent below boiling. With that kind of design parameters and operational parameters, you would essential occupy or utilize the space that's highlighted in that upper right-hand corner.

Meanwhile, on lower temperature operating modes, and I've just shown three scenarios here as examples, using that representative lower end one that Paul talked about in his presentation at Amargosa Valley, Scenario One, you'd use essentially the primary block, including the southern extension. And that potential space utilization would also work for aging, the scenario we looked at for aging. And, again, that would be ventilating for a shorter period of time and aging, as opposed to two meter spacing and a longer ventilation period.

If we look at just de-rating the packages alone, you would see that we use even more space, and of course with the six meter waste package spacing, which is another lower end thermal operating mode option, you would use more of the real estate, as I call it. So with this kind of a concept, the whole range of lower end operating modes can be accommodated.

This is a graphic example of the same material. And, again, this is in the engineering analysis that we're finalizing and is being checked right now, and soon will be completed. But, again, we've done a whole set of parametric evaluations like this to look at space utilization, and this one just takes waste package spacing compared to linear load in the drift, and again it fixes ventilation rates and it fixes the--this is using 26 year age fuel and those sort of things, so there's several things that are made constant here because there's so many variables, and if you look at the one at 1 kilowatt per meter and about a 2 meter spacing on the waste packages, you end up at a certain point on this curve. And this, again, is a whole set of parametrics that are in the engineering analysis.

If you drop down to the line that says 70,000 metric tons, you see that you use about 82 kilometers of drift length to accommodate that. If you would increase the waste package spacing or decrease the linear loads in the drifts, again, you'd use more of your repository layout. In this case, you see the primary block, or the upper block, the lower block expansion limits, and then of course you go beyond it, and there is space beyond the upper and lower blocks that could be utilized in the currently characterized area also. So this is just one example, and again, there's a whole set of these in the engineering analysis that demonstrate the flexibility of this one design over several operating modes.

So essentially what I'm saying here, I guess that's the first question, what do we mean by flexibility. The second question that was asked by the board is what characteristics does DOE consider in determining flexibility. And, again, essentially they're the same two issues as the design parameters, which again can vary during the design phase, but a set had been selected for the SR, but they're not fixed and those will continue to be reevaluated as the design evolves.

And there's the operating parameters, which of course will be flexible throughout the operating period. Those will be available and those will be variables that will be defined all the way through operations, again, within the limits defined by engineering. They won't be unlimited, of course.

The next question is is a base-case design flexible? And, again, we're saying yes. We're saying yes again because this base-case design can be operated both for the high end and the low end in controlled temperature and humidity in the repository environment.

The program continues to analyze those from a design, construction and performance, from an operations perspective by continuing those analyses, but up to this point, all the results confirm the feasibility of this layout, feasibility of this concept. So, again, the engineering analysis should be finished in the next month or so to finalize this, but as far as we can tell, every indication is is that this approach and concept works.

The last question was how much may a design be changed and still be considered the same design. And I wasn't sure if this was a trick question or what, because there's several different ways to answer this. What I chose to answer here is from a very broad, maybe a top level DOE perspective. From that perspective, the present design is essentially a set of large, long-lived waste packages that are horizontally emplaced in the drifts.

There's other parameters, such as the inverts, drip shields, there's other parameters that go with that, but I mean, in concept, that's a design that we now have. And then our approach toward operating over this thermal range, we don't vary those parameters. I mean, we're varying the spacing, but we're not varying that basic concept. So we're not changing the design per se.

Ultimately, from a regulatory perspective, this is more written from an NRC perspective, but once the design parameters are selected and finalized and licensed, any changes to those parameters, which would have to be an amendment to the license application, would certainly be considered a design change.

Now, from an engineering manager's perspective, we have a design control program that manages design change at a much lower level, but I mean those lower levels don't necessarily affect performance, if we're changing anchor bolt locations or, you know, those kinds of things. But this is the kind of level that we think you're addressing or interested in when you asked the question.

Let me just briefly go on from those four questions now and talk about the engineering analysis that's currently ongoing, and it's being checked right now. It's essentially complete. And in this engineering analysis, as Jerry Mentioned, we're analyzing a representative, not an optimized, but a representative layout, which was that Scenario One that Paul highlighted in January, which was the 2 meter spacing and 50 years of forced ventilation and 250 years of natural ventilation.

And why did we pick that particular scenario? Well, from an engineering perspective, it opened a new issue, which was natural ventilation. We wanted to verify that natural ventilation would work, and so we wanted to evaluate that element of the lower end thermal operating mode.

We also at the 2 meter spacing and this combination were able to use less real estate, so to speak, than some of the other lower end modes. So, you know, it represents what we considered a reasonable design approach toward that. But, again, it was kind of, I don't want to say arbitrary, but we wanted to select and verify at least one lower end operating mode, why did we do this, and the purpose of it was to verify we could design, we could construct, and could operate a lower end operating mode with this design approach.

And the results verified that we can design and operate this design. We can design and construct and operate this particular set of parameters in the lower end operating mode. So that was one of the key elements of the design analysis.

Also, we went beyond that and said, okay, in addition to this one design that we're saying you can construct it, I know Leon has asked questions in the past about constructibility, you could construct it, in addition, we looked at evaluating a representative lower end designs, did some parametric analysis of what those impacts would be of varying some of those operating parameters. And I showed you one of the charts earlier was a simplification of one of those charts in the actual report. But we looked at varying waste package spacing, which is what I showed on that chart. We looked at varying and de-rating the packages, making them smaller. We also looked at varying the ventilation rate, the duration and the method.

So, again, I mention we looked at natural ventilation and confirmed that yes, that would work. If you want to shut the fans off at some time in the future, natural ventilation would continue to cool the repository and keep it within the design parameters, the key design parameter here being 85 degrees Centigrade waste package temperature.

We also did some parametric evaluation of some of the design parameters, again to show that these are not fixed. We looked at the 81 meters and what impact that might have, and of course you don't have to be a rocket scientist to figure out that by reducing the drift spacings, you end up reducing the acreage you're using. You just result in longer forced ventilation. So we looked at the parameters as you varied them and turned the knob, as we say, on ventilation versus the aerial loading, the aerial mass loading of the waste. So we looked at some of those again from a design perspective whether it's constructible or not.

We also looked at expansion capability within the characterized area. Even beyond 148 kilometers of lineal drift, there's additional area in the characterized base where we could put additional drifts. So, you know, we found if we had to, we could possibly expand it beyond that. So we found that by and large, there were a lot of options available for meeting the lower end thermal operating mode.

What's next? We'll continue to analyze these operating modes, these parameters, over the thermal operating range. Jerry mentioned that we're reviewing the design requirements to see if any changes need to be made in the requirements documentation, requirements that may be prohibiting the lower end operating mode. And Jerry mentioned one of those, and we're looking at de-coupling. These are just a couple of examples of things we're evaluating.

And, of course, we're going to review and update the baseline to allow expanding the operating modes, the expansion concept with operating modes.

In conclusion, there's two things that I think we can draw from all this. One is that we have selected a set of design and operating parameters for the purposes of performing our site recommendation analyses. These are not finalized, but we've selected them just so we could perform the engineering and the performance analyses during this phase. And our results to date show that we can, with this set of design and operating parameters, we can operate either at the high end thermal operating mode or at the low end thermal operating mode.

A second conclusion is that these analyses that are ongoing are laying a solid foundation that will allow us, during the next phase of the project if the site is approved, to converge on those set of parameters and finalize it. In analyzing, we've proven that both in the high end and low end, a representative concept would work. Now we need to look at how we optimize and select the actual parameters for the preliminary and advanced designs.

With that, questions? Dan, you had deferred one earlier.

CHRISTENSEN: Dan, would you like to begin.

BULLEN: Bullen, Board.

Actually, this is maybe a precursor to that question, which is going to be the one I'm going to ask. I look at the operating modes that you've identified, and we're essentially looking at an above boiling versus below boiling design, and you're trying to make the case that we have a flexible enough design to do both. But from a licensing perspective, I'd look at this maybe from an engineering perspective better yet. I have a nuclear reactor that I want to run, and I have a boiling water reactor that I know how to run, and so the question is can I operate a boiling water reactor like a pressurized water reactor without a two-phase flow change. The answer is probably yes. But is it the right design to do that? And I think no, because I'd have a separate loop to do the heat transfer.

So, along those lines, I want to ask the same question. If you were going to design a low-temperature repository, would the base-case design, or the flexible design that you have, be the one that you'd pick?

TRAUTNER: If I had to make a decision today and there was no further advancement, that would be one of the options that we'd probably pick. But we're not optimizing the design at this stage. We are at the stage now where we are just in the conceptual phases and we haven't finalized that. I don't think this is exactly a boiling water reactor and a pressurized water reactor comparison, because I think what we're talking about here is to design a repository, a fuel handling system, and we're placing this waste into a repository, and so I think it's more like looking at a boiling water reactor, and do you operate at 1100 megawatts or do you operate it at 700 or 800 megawatts. And I think that's the kind of variation we're talking about in the sense that not can the operators turn to control the pressure or control the temperature of that boiling water reactor, so that you get the optimum operation and not necessarily, you know, a totally different design of one versus the other.

BULLEN: Bullen Board.

I understand that, and I probably used a bad example. But what I wanted to state was that there's a fundamental concept here that either we're changing phases or we're not changing phases of the liquid. Okay? And so I guess what I'm looking for is you've got a set drift spacing, 81 meters, you've got a set waste package size, which is big and heavy and full and hot, and so those are the limits that haven't changed. Even though you say you can change them later, those are the limits that haven't changed with this design.

So, essentially, in your analysis, you're almost stuck with the hot design that you're trying to make operate cold. Is that not correct?

TRAUTNER: I don't see it that way at all. No, I've got one design and I can--we can operate that design either hot or cold.

BULLEN: Okay.

TRAUTNER: And the Performance folks are going to analyze both hot and cold.

BULLEN: That's fine. A couple of quick follow-on questions. When you analyze the hot versus cold design, do you consider the effect of change in footprint between the operating modes? I mean, you're looking at the criteria for performance. Do you look at the change in footprint as it impacts performance?

TRAUTNER: The change in footprint as it impacts performance?

BULLEN: If you go back to the figure that showed all those footprints, which is on the slide of what, Figure 5 or 7? Figure 7. I have to make some selection that's based on how it performs. So is there a trade-off with respect to footprint size and total performance?

TRAUTNER: Rob, do you want to address that one?

HOWARD: Yes. Rob Howard, BSC, Integration Manager for Science and Analysis.

The postclosure analyses that we're doing for the thermal operating mode, the first round of those that's going to be documented in the SSPA, we are not changing the footprint when we do the calculations to total dose. There is information that's going to be documented in those analyses that talk about the implications of the larger footprint with respect to UZ flow and transport.

BULLEN: Okay. Bullen, Board.

Rob, don't go away. This is a real quick question and then I'll be done and I'll give it back to the Chair.

With respect to the analysis that you have on these figures, it looks like you're extending the footprint a long way north into the high hydraulic gradient. Did you analyze the effect of the high hydraulic gradient on that? And I guess the follow-on question is where is the exhaust main now, since with a 120 meter rise in the water table, with the pluvial conditions, is it going to be underwater, or are youi going to have it above, or what's the status with that high hydraulic gradient in your analyses?

HOWARD: So you're talking about coupling the effects of the long-term climate change in the analyses.

BULLEN: Actually, I'm interested in how is it going to perform? You've got a new design here. It's something that looks a little farther north than we've ever seen.

HOWARD: Yes, it is a little bit further north, and we are closer to what we believe is a steep hydraulic gradient. We're not explicitly calculating in the postclosure performance assessment the effects of that large hydraulic gradient. We are analyzing it with respect to space requirements and whether or not we will actually flood either the emplacement drifts or the perimeter drifts, and we don't have that analysis finished yet.

BULLEN: Okay, I guess the follow-on question is if it's a hot design and you're that far north, are you going to be mobilizing water from the water table when the water table comes up? I mean, you're getting pretty close to the repository with pretty warm stuff; right?

HOWARD: Well, yeah. I mean, a potential water table rise would come, in all likelihood, a very long time after the large thermal pulse from the hot repository design. So you would be basically in cooler conditions at that point.

BULLEN: I thought it was only 600 years. I mean, the first potential water table rise is going to be after 600 years when you go from the dry to the long-term average kind of time frames, isn't it? Or monsoonal flow. I'm sorry. Monsoonal region. So 600 years is still within the 2,000, 3,000 year thermal pulse. You're going to have the potential to move some water; right?

HOWARD: I don't think that we'll be elevating water tables that high in the first 600 years.

BULLEN: But they will be moving, and you're going to be getting closer; right? So have you done an analysis, I guess is the question?

HOWARD: The answer is no.

BULLEN: Thank you.

TRAUTNER: I can add to that, though. We have analyzed that hydraulic, the rise, and it will not impact the northern extension. But if we've flexible enough in this layout we don't need all this space, we can bring this back into where it was before and move it away from that hydraulic gradient if that's needed. Right now, our assessment says that it's not going to impact the performance. But if Performance comes back and says that that will impact that in the long-term, we can move away from it. We've got a lot of space out there. We don't have to go there if it's a problem with performance.

BULLEN: It might be a worthwhile analysis to at least make sure you've done that.

TRAUTNER: And, again, that would be part of the convergence, I would think, after we get into the detailed design, we'll look at those kinds of issues more fully.

BULLEN: Okay. I'm done, Mr. Chairman.

CHRISTENSEN: Richard Parizek?

PARIZEK: Parizek, Board.

Sort of along the same lines. That northern extension is in the wetter area of Yucca Mountain, I guess, from an infiltration point of view. So right away, in that sense, it may be quite important to run that analysis, not only a shorter distance to the saturated zone, steeper gradients, there's a lot of factors that would come into seeing whether that design is going to be stable or not, and one just being the higher infiltration rate. So it seems to me even if you went west, again, you have data to support shifting in that direction, because it's flexible, you've got to see what the consequence of picking another alternative is in terms of performance.

TRAUTNER: Well, absolutely. You know, that was just an engineering kind of optimization. The guy wanted to get all one level, and we can easily change this. We've got room at the lower level. We've got different ways of moving away from that gradient. We will analyze that in detail. This is a conceptual design basis, and when we get into more detailed design, we'll make sure we avoid things like that. And that was one of the parameters we looked at, is there more--and I don't have the chart here. I guess I could put it up. But there's a lot more expansive area in this regime where we could go southern, or different areas. So it's not that we had to have that northern extension.

PARIZEK: Each direction you shift in has a geological and hydrological implication to transport and performance. And so from a performance point of view, someone would have to then run through that analysis to say did it make any difference, beneficially or harmful.

TRAUTNER: Absolutely. And that's why performance would be hand in hand with the performance.

CHRISTENSEN: Debra Knopman?

KNOPMAN: I want to go to the question of what is flexibility here, and what do you have flexibility for? Now, you've discussed it in the context of the thermal regime. But what are the underlying objectives that you're actually being flexible for? Is performance simply the end point dose 20 kilometers away, or are there a set of criteria that you're looking at that will tell you, you know, when you need to exercise the flexibility? There's implied in what you've said, but you've not made explicit, what your decision rules are here for exercising the flexibility, or just justifying why you're trying to design for flexibility.

Now, one possibility is robustness, which is really a different concept, and you haven't quite talked about that, that is, that you'll have a design that will deal with uncertainty. It's sort of an all weather design, no matter what the conditions may be. But I haven't heard you say that, and I don't know if you're trying to minimize effects of uncertainties at the time of construction. Tell us what your criteria are for determining the exercise of flexibility in your parameters, design and operational.

TRAUTNER: Yes, as the design evolves, there will be several areas we'll look at for flexibility, not just thermal, I mean, rock conditions, Priscilla brings this up periodically, construction, if they end up hitting a pocket of bad rock, we'll have a design that will allow them to move around that or not utilize that. Another element is retrievability. I mean, retrievability in a way is an element of flexibility. We have to be able to retrieve the waste, and we have to build that into the design.

Another area is the ability to have it blend in the pool. That's in the concept of element of flexibility, because it depends on which order the waste comes in. If we get a lot of hot reactor fuel over a short period of time, we'd better blend those with cooler fuel temperature-wise in the blending pool. So there's a lot of elements of flexibility, and all those will be defined, and it's part of our risk management plan as we go into more detailed preliminary and final designs.

KNOPMAN: Okay, it's part of risk management. I'm just trying to understand what risk you're talking about here. Let's talk about rock properties. You start excavating in an area and determine that the rock isn't what you had hoped. What would be the tipping point of deciding not to go into an area? What is it that you'd know from performance assessment that would tell you, what, that you're going to have rock falls immediately, or what would--I'm trying to understand what criteria you're using to exercise flexibility?

TRAUTNER: And that's a very good question. I think the timing of the question is the issue, because we would define all those criteria as we advance the design. Right now, we're at the conceptual stages of the design, determining suitability. And when we're talking about giving construction an option or direction on when they should continue to use that rock or not, or whether they should go around that rock, those are the kinds of things we'll put into our detailed design specifications, construction specifications, and operating specifications. If anybody else wants to add anything to that?

BARRETT: Lake Barrett, DOE, maybe I can add a little bit.

The overall guiding design principle is to look for the most certain isolation that we can get, and trade-offs that go with that. That's really what we're trying to do. We haven't frozen any design. We're not even close to optimizing this design. But we have a piece of real estate that we think is a good piece of real estate. Perfect real estate it is not. Okay? So we are trying to do the things that we can do to maintain flexibility, do the trade-offs on all of these things that are all complicated trade-offs of competing good. I mean, you know, there is the two-phase flow issue. Even when you're below 85 degrees, you're still going to evaporate water.

So, I mean, these things are constantly being traded off as we get into the design, and we have not optimized the design at all. At this stage, we are trying to develop the best available technology for a repository that is flexible, forgiving, robust, all weather, some of those things, and trying to balance that as we develop, and not foreclose options in the future. We're very carefully about not foreclosing options, to try to do the best we can with the knowledge we have to design a facility to best isolate this material for a very long time, to the best certainty that we can do.

KNOPMAN: If I could just follow up? This is a really important point, and it needs to be really clear in public documentation as to what the basis is for, one, laying out a design and, two, building in flexibility. And your answer, Lake, suggests that uncertainty and uncertainty reduction is the key pillar of the justification for flexibility. Now, if that's not right, help me on understanding what the multiple criteria may be for building flexibility into a design.

BARRETT: I would say it's much more than just the uncertainty issue. And I think it was in one of your early Board letters where you asked sort of a rhetorical question about are you better off with a low mean number, okay, and maybe a higher uncertainty value, or a higher mean number, and a lower uncertainty value. Okay? So there is no "right and wrong." There is no right answer. It's a balance of these things. What we are trying to do is go for the best isolation in the future, which may be a low number, with a higher uncertainty; or it may be a higher number with a lower uncertainty. And we're balancing these off in various trade-offs that we're doing.

For example, in the DEIS supplement, you will see, for example with the larger volumes of excavated dirt, higher radon doses. That's an up-front dose, higher dose, but nonetheless, for a small dose, versus potential dose many millennia into the future and uncertainties about a zero dose.

So these are the things that balance back and forth, and there is no crisp, clear line or curve that says don't do this or don't do that as we develop it. But your points are very good on this, and I don't think we articulate this that well, and we're going to certainly work on it. This is very constructive.

CHRISTENSEN: Paul Craig?

CRAIG: Yeah, I found this presentation rather disturbing, in fact. Looking for one design that seems to be a workable design, and indeed the Board has made statements about this. Then later on, after you have one workable design, you can improve it. That makes sense. But if you don't have one workable design, it's not at all clear that you have a project that one should be enthusiastic about. And you have a long history of new information coming along, and then things change.

So now what you seem to be doing, as I heard the presentation, was to raise flexibility and moving target to a high art form, which means that it's going to be exceedingly difficult for someone looking at this from the outside to figure out what it is that you're talking about. And you may be sacrificing so much in order to get your flexibility that the whole thing simply becomes mysterious and murky.

Personally, I would like to see one design that you think is the best, trading off all these different things that you need to trade off, and you specify how you trade them off, and then we can look at that and we can say okay, I can look at it and I can say all right, does this meet my requirements as being scientific defensible or doesn't it? But with this design, this approach, it just looks exceedingly difficult to do that. And so the impression that I came away from this presentation with is that you're going backwards, not forwards.

TRAUTNER: Well, I think when we're talking about design, we have one design. We have selected this design, and this design is flexible enough to operate over a range of temperature conditions. And, I mean, that's--maybe obviously I didn't make my point here, but that was what I was trying to say. We have a design. It's a single design, and the operators will be able to operate that over a range of conditions. But in the end, performance and reduction of uncertainty, all these things are going to drive, along with cost, of course, which options we choose. But we've proven that we can design and construct a low end mode. We've proven, you know, that we can design and construct a high end operating mode, and we haven't optimized either one of those particular representative designs. They're just in the spectrum of things, so to speak. They're not one hot and one cold. It's representative. But it's a single design in the end.

CHRISTENSEN: Dan Bullen?

BULLEN: Bullen, Board.

You mentioned that you had this one design. But was this the design that was analyzed in TSPA-SR that was released last year?

TRAUTNER: I think the answer to that is yes.

HOWARD: Yeah, what we released in the TSPA-SR Rev. 0 ICN was the analysis of the high temperature operating mode portion of this design. What we're doing now in the supplemental science and performance analysis is evaluating the lower end of the range, and some space in between.

BULLEN: Okay. So the high end design from TSPA-SR was the top figure? Was it that footprint, or was it different.

HOWARD: I'd have to go back and look at the actual drawings.

BULLEN: But I don't think it was, was it? Because you said you didn't go that far north.

HOWARD: Yeah, I'm sure that's not drawn to scale, but it does not look like we were that far north in the analysis.

BULLEN: Okay. So it's not really the analysis that was done. I'm sorry, the design is still changing.

HOWARD: Well, I think what we're talking about is what drifts we actually load in that footprint, aren't we.

BULLEN: Well, what analysis did you do for TSPA-SR, and is it that design, was the question.

HOWARD: Okay. The answer is we analyzed, and I'll have to go get the specifics of which drifts we loaded for the high temperature operating mode, but it's probably the same acreage that you would see there, but shifted to the south.

BULLEN: Okay. One quick follow-on question, Mr. Chairman.

Could you just go to Figure 13? You talked about the analyses, current engineering analysis summary. On the previous page, you gave us basically a document. Where can I find the analysis of, for example, the parametric evaluation of operational parameter flexibility, the parametric evaluation of design parameters, and the potential expansion capabilities? Where do I see that?

TRAUTNER: They're in the same document.

BULLEN: Same document? So if I go back to ANL WERMD 5001?

TRAUTNER: It's being checked right now in draft form. It will be issued by the end of this month.

BULLEN: Okay. So it's coming out? I guess I'll ask the Board do we have this? Okay, thank you.

CHRISTENSEN: Priscilla Nelson?

NELSON: I've been listening to this, and I think a part of the problem here is that from a perspective, which I'm increasingly finding my mind going to, what's presented here is actually something where the flexibility of operation for a given design is investigated, and I don't feel that the flexibility of design has been investigated. So maybe that's a semantics issue, but it seems that many of the parameters that you're talking about, you're not changing a great number of the input parameters. I mean, you change the spacial array, but not particularly anything else about the rock or inputting some variability in rock properties. It seems more of an investigation of operational flexibility for a design than it is a real investigation of flexibility of design in this mountain, with the accent on operational changes.

Can you comment on that? I mean, why is that wrong?

TRAUTNER: That's not wrong. I think that's correct. And I'm not sure what design parameters you would say we should look at in terms of looking at flexible. We're not trying to say that the--the five and a half meter drift diameter, we can change that. That's not fixed. We can make it six, we can make it seven, we can make it five possibly.

That's not a fixed parameter. It's a design parameter. But how would we change that to significantly impact thermal performance? You know, the design, a lot of these design parameters that we're looking at, as I say, I've tried to separate what we consider design. You know, we looked up in Webster's dictionary what design means, and it's a very illusive word because it means a lot of different things to different people, and it's very, very broad sense. Everything we're doing in this repository is a design. And in that sense, yeah, we are changing the design in the sense that an operating parameter is part of the design.

From an engineering perspective, I get a little more detailed and I'm saying design parameters are things like drift diameters, drift spacing, waste package materials, how much waste I have to put into the mountain. Those are things that I have very little--you know, have very little impact in the long term, either on I can't change it like the 70,000 metric tons I have to deal with, or things like the waste package materials, or the drift diameters that don't have a big impact on the thermal response of the mountain. The things that do have the major change and impact on the thermal response of the mountain end up being things like waste package spacing or ventilation duration rates, flow rates, and those are, as I say, they're design in the sense that will define the limits, but they're not, you know, the things that we have to operate. I can't tell the operators that they're going to have certain waste. The fuel we're going to get 15 years from now is going to be variable, and I have to be able to handle that variation through the operations. The design isn't going to solve that problem.

NELSON: Well, I'm trying to capture the sense of the Board's wishes to not to only have this design, which has a range of parameters associated with the design, exercised to investigate operational flexibilities, but also to say if you're really going to make a design or an option, I don't know what the word is anymore, that really takes advantage or works to create a best environment that takes advantage of low temperatureness of a mountain operation, it probably would not be this particular layout, this particular configuration. So that's sort of a suspicion, and the idea to see that suspicion investigated by the project I think is part of what the Board has been thinking about.

I think that there are questions that relate to how this flexibility is being investigated, whether it's operations or design. A lot of it has to do with if the accent is on uncertainty, then how are the models that are being used capable of reflecting changing uncertainties in operation? How do the models reflect low versus high temperature water movement, uncertainty about that? And thermal pulse during time, how is that affected? How is the changing assumptions about waste package spacing and rating, et cetera, changing the understanding about the heterogeneity of the thermal field and of the rock mass field as it varies through the mountain? Those kinds of things aren't necessarily being investigated in the current context, being pretty much delayed, I guess, for fine tuning or later design work.

But for right now, it seems that a lot of those that really deal with modeling and change in state of fluids really aren't so much being investigated. And what's happening here is this design is being investigated for operational flexibility, rather than a real design flexibility investigation. That's the perception.

WILLIAMS: Nancy Williams, BSC manager of projects.

As we discussed when you were out here, Priscilla, we are going to investigate the design. I am going to bring in an independent team, and that's still on the boards.

CHRISTENSEN: Dan Bullen?

BULLEN: I'm out of time?

CHRISTENSEN: No, you've got two minutes. Use them wisely.

BULLEN: Bullen, Board.

Could you go to Figure 8, please? Maybe one of the things that would be illuminating for us is that if we talk about uncertainty, could you put some error bars on this? I mean, what kind of error bars would you have on linear thermal load versus waste package spacing, or what kind of error bars would you have on the required length of the repository drifts, or those kinds of things? I mean, these look like they're very definitive lines, and I could pull off a design because you've done that based on the fact that you've, you know, made drawings and said, well, this is the number that we have. Are there big error bars on that, or can I actually figure a waste package spacing of 1.98 meters works this way?

TRAUTNER: I would say that the error bars are fairly narrow. They're not wide. Because, you know, the fact is that the linear, it depends on the program, what we're looking at in terms of the heat transfer within the drift now we're talking about here, as opposed to rock. And we're talking about linear load per meter, we're not in the host rock here, we're in the drift and in the waste packages, and the calculations are fairly--I mean, there's error bars obviously, but not high.

BULLEN: Bullen Board.

I guess you run into the problem with respect to you do get tied into rock property parameters, because you need to know what the thermal conductivity is and you need to know what the moisture state of the rock is. And so I guess what comes to mind is that the large scale or drift scale heater test that you've run gave us very good information, but in essence, you missed the prediction because you don't know where about 25 per cent of the power went. And so I would argue that the error bars on this are big.

TRAUTNER: Well, maybe we're not interpreting it properly.

BULLEN: No, I understand what you're saying, is that I know how much power is coming out of the waste package.

TRAUTNER: Right, the power--

BULLEN: I know exactly what that means.

TRAUTNER: Versus the waste package spacing.

BULLEN: But where that power goes to keep a temperature limit is indicative of the environment, not necessarily just what comes out of the waste package. And if the environmental parameters, one, vary, or, two, aren't well known, then essentially I've got to have error bars on that. Otherwise, if I don't get the heat out, then I'm not going to be on these curves. I'm going to have to have the waste package spacing be farther apart, or closer together, or one of the two. And so I guess that's the tie-in there, is I think there are probably larger error bars than, you know, the lines indicate.

TRAUTNER: Yeah. But, again, the idea being that we've got a lot of real estate here. So if you take this line and you say it's plus or minus 10 per cent, 20 per cent, even 30 per cent, the mountain has a lot of capability to handle that.

BULLEN: Thank you, Mr. Chairman.

CHRISTENSEN: We have one final question from Staff. Carl Di Bella?

DI BELLA: Yes, Carl Di Bella. Thank you.

You and Lake have both mentioned that the designs are not optimized, neither high nor low. And I would certainly say amen to that, particularly for the low temperature design, because it requires a lot of ventilation and it's based on a design that didn't require a lot of ventilation.

Within the last few days, DOE has released the total system life cycle cost, and there is some mention of incremental costs, or costs of the lower temperature design. What's the meaningfulness of a cost estimate of a design that far from optimum, and why did you put the costs in there in the first place when there's no requirement for it for the low temperature design?

BARRETT: Barrett, DOE.

The total life cycle cost report, before we had basically, you know, twelve months ago, we had basically a single point design which was, you know, the warm, hot, you know, basically the EDA 2 modified design, because we looked at six different things way back when, multiple years ago.

That was the cooler EDA 2 model. We now are talking about a range. We're looking very seriously at a colder design, as we've been discussing. We felt we needed to address that in the TSLCC report as well.

We are certainly nowhere we have a number. We did not put in it's going to cost "X" dollars more specifically, so we put the basic assumption, the basic facts are that as you drive more tunnels, excavate more rock, you are going to have more cost. If you don't segment the drip shields, you're going to have more materials.

So in NEPA space, we wanted to evaluate the rent, and we did all that to 148 kilometers, but we didn't want to put in, well, the cold design, and speculate it would cost "X" billion dollars more on top of the increases we've already had, so we tried to write it down as sort of parametrics that went along the line, additional titanium costs so many dollars per pound of titanium, additional excavated rock costs so much, et cetera, additional ventilation, you know, which is billions of dollars in ventilation cost, long periods of time, et cetera.

So we didn't want to get into specifics, so we tried to do it parametrically. We felt that we ought to at least acknowledge that there would be potential cost increases with these designs. There's also performance increases with the designs, but we're not making any conclusions at this time.

CHRISTENSEN: Thank you.

Our final presentation of the morning will deal with multiple lines of evidence, and the presenter will be Abe Van Luik, who's Senior Policy Advisor for Performance Assessment at the Yucca Mountain Site Characterization Office.

COHON: As Abe is getting wired, let me just point out that if this session ends before 12:30, substantially before 12:30, we will have a public comment period until 12:30. But if it doesn't we'll just have public comment at the end of the day.

VAN LUIK: This talk is in a series of talks and discussions that we've had with the Board on this topic. The Board recommended, and this has been mentioned several times, essential elements of any DOE site recommendation has certain components. We're talking about the fourth component only, development of multiple lines of evidence to support the safety case of the proposed repository.

We had a meeting with the Panel on April 13, just last month, where we talked about considering various multiple lines of evidence, such as alternative analyses, natural analogs, simplified calculations, and direct observations.

We discussed use of multiple lines of evidence, we being those people who presented at this meeting, some of whom were brought in by the Board and not representing our project, but what was discussed there was multiple lines of evidence to provide a clear and transparent safety case. And we all acknowledge that total system performance assessment is an important part of the safety case, but these are other arguments in addition to performance assessment.

DOE will use multiple lines of evidence to show that scientific work underlying the site recommendation is competent, technically defensible, and that there is a basis for having confidence in the safety case. That's our goal.

We have already acknowledged that the scientific method itself requires consideration of multiple lines of evidence in the development of conceptual models from data and observations. This is the way things are done.

The international community, as we've talked about also in this panel session, also recognizes the importance of multiple lines of evidence. The International Atomic Energy Agency, has a technical document that speaks to it. The OECD/NEA Integration Group for the Safety Case, of which I'm the Chairman, and I reported at the Panel meeting, has been investigating how you go about addressing multiple lines of evidence. And DOE agrees that multiple lines of evidence should be part of the documentation that provides the technical basis for a site recommendation.

Prior to the site recommendation, however, and this is kind of a confession statement here, the DOE addressed, we felt, but did not emphasize, multiple lines of evidence in the scientific and engineering programs.

The documentation was in supporting documents, such as the site description and process model reports. A lot of times the documentation was implicit, like if you read very carefully, you say oh, yeah, this is based on other things, but it was not explicit. Now we see, because of the Board's urging on this matter, that we missed an opportunity to highlight a lot of work that was done, and we are now taking the opportunity to correct what is really an oversight on our part.

So what we are doing now is bringing the discussions of multiple lines of evidence into the current documents in preparation at this time, the supplemental science and performance analyses report. We will have there discussions of other lines of evidence summarized for major process models.

In addition to that, later in November, we will have a synthesis report looking at the results of ongoing analog studies.

We will continue after that, this is a good start, but we will continue after that to provide more emphasis and visibility to multiple lines of evidence. In other words, it's kind of a disease that we caught, and now we're enthused about pursuing it. Identifying additional lines of evidence will be a continuing effort even beyond site characterization.

As we move, if there is approval of the site and we move forward, as we move forward, it becomes of continuing importance to build a credible safety case, and to, you know, as the licensing steps are going forward, to make that safety case as strong as possible.

In the SSPA that I mentioned that will be coming out this summer, I'm just going to walk through all of the subsections where multiple lines of evidence are going to be mentioned, and in some cases are already mentioned.

I'm going to talk about just a couple of examples, the yellow highlighted ones, just two examples, and I don't want to get into a big technical discussion. I just want to show the types of things that we're throwing in.

If you look at Chapter 3, this is the listing of subsections in Chapter 3 where there is a fourth level which says other lines of evidence, multiple lines of evidence, whatever seems to be appropriate for that model.

Chapter 4, the same thing, and I'm talk a little bit about what's in the seepage section right now. If you looked right now, you would see that some of these have content, some of these have a lot of content, and some of these are TBD still. They're still being written.

Chapter 5, Chapter 6, 7, 8, 9, 10, 11, 12, 13 we skipped, 14.

We talked a little bit about work in multiple lines of evidence in previous meetings. In fact, what we talked about in the January 2000 Board meeting is analog studies, and radionuclide flow and transport studies at Peña Blanca and other analog sites. And we looked at qualitative verification of models for seepage using natural analogs, too. We talked about that over a year ago.

In the January 2001 Board meeting, we talked about passive film stability, and summarized some of our ongoing studies of Josephinite. And these are the types of things that we are now documenting in the SSPA.

I want to talk about a couple of examples. These are yellow highlighted things in the long list of sections in the SSPA document. If we look at lateral flow within the Paintbrush Tuff nonwelded units, if you remember, Montazer and Wilson were the first real scientific interpretation of the flow in the mountain, and they hypothesized that the PTn, the nonwelded units, caused lateral flow, so that downward flow within the Topopah Spring unit would be smaller than in the PTn. That was their hypothesis.

Current models do not show that. What current models show is that there is a redistribution of the infiltration in the nonwelded units, but the fluxes in the PTn and the TSw are not significantly different.

So what is the basis for that? Well, the dampening and lateral flow within the PTn reduces spatial heterogeneity predicted by the infiltration model. We have made independent observations and done independent analysis to support this reinterpretation, this new conceptual model.

It's based on calculated fluxes within six boreholes, and the appropriateness of the current conceptual model was also tested against other observations and analyses. We looked at spatial distribution of chloride concentration secondary minerals in lithophysal cavities, for example, and we have a chloride-based infiltration map that is almost an independent check on the other infiltration map. So we feel that here we are doing the job that you suggested we do, and that is to say what have you done in addition to the straightforward calculation. These are the types of things that we've done that give us a pretty good feeling that we're on the right track.

Another example is seepage. The unsaturated zone flow model predicts most water will be diverted around emplacement drifts. We'll have very little seepage. The drifts act like capillary barriers, and we have evaluated this in part by looking at other lines of evidence.

One thing we looked at, and it was also used just a moment ago for the other example, is lithophysal cavities. There are no stalagtite deposits in lithophysal cavities. Nothing is hanging from the ceiling of the little cavities that we're talking about. So there's no evidence of dripping there.

The seepage rate calculated, and it says from these deposits, what it means is from the deposits in lithophysal cavities, which tend to be along the bottom, is less than the seepage model predicts. So we have an indication that we're on the conservative side.

When we look completely away from Yucca Mountain at just the general topic of excavated openings, we see no evidence of dripping in tombs in Egypt. We see that paintings in temples carved into basalt at Ajanta, India, and this was a long time ago, these paintings are very well preserved because there's basically no water dripping from the ceiling and running over and evaporating and covering these things.

Now, one of the things that we like to say in the discussion of natural analogs, and this is an anthropogenic analog actually, is that for every analog, there is a counter-analog. One of the reasons that some of the cave paintings in Spain and France are preserved so well is because there is a moderate amount of seepage through the rock that evaporates, leaves calcite behind, and so there's a calcite coating basically protecting the paint materials.

But understanding the differences, it's just like the analog of Roman concrete. Roman cements are preserved in many places, and they're gone in other places. Two things that people who have studied this have learned is that, one, there was no quality assurance program that the Romans used. Sometimes they just made bad concrete. And in other places, you know, there are environmental parameters that are obviously different in preserving these materials.

The same thing with Roman nails. In England, there's one place where the Romans, when they left and the Barbarians took over, that what they did is they didn't want these people to have nails, because nails could be used to make fortifications, and other things, and so they hurried and took all their nails and dumped them into a hole and buried them in such a way that water, which is plentiful in the English countryside, basically saw capillary barrier moved around the nails, and they're perfect. They can still be hammered. Other places, obviously, there are no nails at all, and we know that they used them. So, you know, there are things to be learned from analogs and counter-analogs.

Caves in southwestern U.S., plant and animal remains preserved for tens of thousands of years. And, of course, a good example of this is the preservation of the mummy and spirit cave, a 9,400 year old mummy that they were able to tell from the intestinal track what his last meal was, and that, you know, he was basically having his last meal and expecting to die. Caves in Europe, these are all indicators that there is very little seepage into openings from rock.

Our own exploratory studies facilities, we have no observations of natural seepage. We have no construction water observed into the ESF at the crossover point of the ECRB. These are all additional indicators that we're on the right track in saying that seepage is an unlikely event.

Now, another thing, and I just mentioned, you know, for every analog, there is a counter-analog, but we do have a couple of observations that we are also documenting into the same report that in one place that's potentially conflicting. In another one, it's apparently conflicting, but maybe not. The water that's observed in the middle non-ventilated zone between the second and third bulkheads in the ECRB. Our analysis and modeling suggests that the source of the water is condensation; that basically you have a temperature gradient, and towards the cooler end of things, since we are talking about 90-some per cent relative humidity, you get condensation.

We're doing ongoing work to evaluate if it's condensation, construction water, or seeping pore water. If it's seeping pore water, then we have found a conflicting line of evidence. But we're evaluating it. This is work that's in progress.

Another apparently or potentially conflicting line of evidence that's been mentioned by many people, including ourselves, is there is seepage into the tunnels at Rainier Mesa, or at least there was. The stratigraphy is generally similar. Precipitation is about double Yucca Mountain.

During tunnel construction, the joints yielded water. We don't see that in Yucca Mountain. We did see it there. Additional work in that area has suggested that this is seepage from an overlying perched zone. What we saw was seepage fractures are only in the zeolitic, and not in the vitric tuffs. This suggests that seepage is localized and restricted to certain flow paths and geologic units. That general statement is not inconsistent with Yucca Mountain seepage modeling. But, obviously, you know, this is something that it would be derelict on our part if we do not take that into account.

So what are we doing on multiple lines of evidence? We are highlighting the consideration of multiple lines of evidence in the currents, meaning currently in preparation SR related documents. I believe that the Board is owed a vote of thanks in stimulating this effort. We were basically very slowly moving towards this, but this really accelerated the effort. And we see now that this effort is resulting in a more complete and transparent discussion of the scientific basis for our models.

The discussions, as I said, they're in process, are not yet robust. They focus primarily on analogs, direct observations, and alternative analyses. And we will continue to improve the documentation in the document, the SSPA that we're working on right now, and we are looking forward to continuing this process of not only looking at, but also documenting multiple lines of evidence into the future.

Thank you very much.

CHRISTENSEN: Thank you, Abe.

Questions from the Board? Paul?

CRAIG: Thanks, Abe. This really seems to be a good direction you're going. And I want to turn to the ECRB non-ventilated zone, which was Number 16, I think. I'm not so sure that I would consider that to be a multiple line of evidence kind of example.

COHON: Paul, I'm sorry. Could you stay closer to the microphone?

CRAIG: Okay. Craig, Board. I'll get it this time.

To repeat the first remark, you're going in a really interesting direction here. And on Number 16 where you talk about the ECRB section, I'm not so sure I would consider that a multiple line of evidence. This, rather, seems to be a situation where you have a wonderful opportunity to test the models, because there are rather explicit predictions, and if it turns out that it's condensation, this is really good for the models, and if it turns out that you can actually demonstrate that it's seepage, then there are problems with the models.

So it seems that this is a place which is a little bit different from the rest of your presentation, like Ajanta, India, and it would be absolutely wonderful, and very important, to find out whether the models are supported or whether they're in trouble, and it's nice because it's a prediction and nobody knows what the answer is for sure at this stage. It's what you want, is predictions, so when they come in, they'll really carry a lot of weight whichever way it comes in.

So the second point that I wanted to make is you gave the examples that can cut either way, and there certainly are such example. It would be extraordinarily nice to look at these examples and to be able to say why they go either way, and what the implications are for Yucca Mountain. It may not be possible to do that in a lot of cases, but on the other hand, in some cases, it may be possible.

VAN LUIK: Yes, I would agree that if the opportunity is there, we ought to take advantage of it, yes. And I think the remark about the ECRB is noted, and we will look into that predictive modeling, and this is why I have it on the list, it could go either way, depending on the outcome.

CHRISTENSEN: Dan Bullen?

BULLEN: Bullen, Board.

Just to follow up on what Paul said, maybe go to Slide 18, I actually also had the same type of question as a follow-on. The analysis and model reports suggest that the source water is condensation. I seem to recall that early on in the experiment between the bulkheads, there wasn't any water, and the cause of that appeared to be we left the lights on. And if you left the lights on, you could figure out how much power, integrated power, would go in there. And could you not then predict, okay, with that amount of power, we didn't see significant condensation or water present, can you then use it to predict what you think the tunnel performance might be long term? You know, how much of a de-rated waste package do I have to have and still not get condensation on surfaces? And is that kind of analysis underway or being considered?

VAN LUIK: I don't know if it's underway or being considered, but perhaps Mark can say something about that.

PETERS: Mark Peters, Los Alamos.

Dan, one clarification, there was water. You see water in that middle section the whole time.

BULLEN: Okay. Bullen, Board.

And was that water essentially near the lights? I mean, it seemed to me that the water moved from the lights.

PETERS: It was within that same general section. It's probably changed in spatial extent to some extent, but the lights were a source of heat, so we turned them off to limit that source of heat. The TBM is now pretty much the only source of heat.

BULLEN: Okay.

PETERS: But there was water.

BULLEN: I guess I just wanted to reiterate that. I know there's a source of heat, and the source of heat seems to be moving the water. So can you analyze how much heat do I need to actually move the water, or have the water condense in certain areas?

PETERS: Right, that's what we're looking at in modeling space right now.

BULLEN: Great. That's the question I wanted to ask.

VAN LUIK: I think these questions are reflecting the fact that I'm giving a talk saying we're doing this, and you want to hurry and get past that and get to the technical meat of things. And I think some of your enthusiasm about some of these opportunities to question our models and either support or change them is the same enthusiasm that's now catching us, too, because we're seeing this as an opportunity to either shine or make corrections and shine later.

CHRISTENSEN: Alberto Sagüés?

SAGÜÉS: Yes. Has there been any recent progress in the area of metals performance?

VAN LUIK: the reason that I didn't use that particular one as an example is because the content of the one that I was reading is basically the same as we presented on the Josephinite work. I'm under the impression that work is continuing there, and there is progress, but I'm not familiar with it, and if someone here wants to make a very short statement to that effect? Yes, we do have a volunteer.

SUMMERS: Summers, Livermore.

The work that was presented or referred to here is the same work that was presented in Amargosa Valley, and that is the work that is continuing right now. But there are no new results.

CHRISTENSEN: Thank you. Leon Reiter, Staff?

REITER: I have two short questions. The first one, Slide 16, please. Is that stalagtitic?

VAN LUIK: Yes.

REITER: What does that mean?

VAN LUIK: Well stalagtite means something that's hanging down.

REITER: Stalagtite, okay. Well, if that's the case, it's stalactite, I think.

VAN LUIK: I'm sorry. We're DOE. We change the language at will.

REITER: You might want to coordinate this with some people from the USGS, because probably we'll hear tomorrow one of the evidence that they pose is that the lack of calcite, or secondary deposits on the roof of the cavity, which I assume is stalactite, is an indicator that water was downwelling. And if that water is distributed both on top and the bottom, then it might be an indicator that water was upwelling in the saturated zone. In some ways, you're conflicting with what they're saying, and maybe we'll hear more about that tomorrow.

The second question has to do with whether or not you're planning anything on Paiute Mesa. That was supposed to be an example of someplace where you had a thermal intrusion in the past, and you might look at the thermal effects on rock. I know other people have brought it up. Is anything being planned to be done on that?

VAN LUIK: Not that I'm aware of. Is there anyone that can shed more light on that? Mark, are you aware of anything at all?

PETERS: You mean Paiute Ridge? You mean--

REITER: Yeah, Paiute Ridge. I'm sorry. You're right. My mistake.

PETERS: Mark Peters, Los Alamos.

Where there's been a basaltic sill intruded into the zeolitic tuffs, there's ongoing work looking at THC type of effects associated with that intrusion.

REITER: Yes, because the Board has heard Walt Matascala talk about that.

PETERS: Right. And the project is doing some work, Los Alamos and Berkeley are both looking at that.

REITER: Will that material be ready by the end of the year?

PETERS: It will probably likely be included in the synthesis report at least as an update. It's ongoing right now. It's funded this year, and continuing.

CHRISTENSEN: Richard Parizek?

PARIZEK: Parizek, Board.

Abe, I want to compliment you on the approach that you're taking here. As you recall, from the multiple lines of evidence work session, Bill Dudley went through sort of a tedious review of all the lines of evidence, and suggested that infiltration was relatively low, and he kind of went through the history of that. And the Board I think that were present were pleased with that, by saying look, no matter how you cut it, I think it's credible, because you really have come at this in a way that's multi-faceted. And in your approach, if you do it this way, you're doing the same thing. It's data that already exists. It's observations people have made, but it hasn't always been integrated in a way that you can see how these analogs help.

In this November report that's due out, I would hope that that report does more than just sort of name a group of analogs, and then sort of suggests in what way they might be helpful, but rather shows how you intend to integrate the analog in some part of the analysis. That's sort of what you were doing today for us.

VAN LUIK: Yes. In fact, the coordination between that report and this work here is almost total. The same people are involved.

PARIZEK: It's a question of how do you get your money's worth out of that effort to make it clear. It's a transparency issue, in part.

VAN LUIK: Yes.

PARIZEK: And then the question about the Yellowstone that was mentioned earlier today, a little bit on that one, and again, that's probably a thermal hydrological consideration, but I'm not familiar with all of the details of what's planned there.

VAN LUIK: Yeah, I'm not familiar with the details of what's planned there either, and I'd have to call back on Mark, if he knows.

PARIZEK: We could save that for later.

VAN LUIK: I was under the impression actually that this was more a review. You know, the project actually did some work in Yellowstone very early on, and I thought it was a review of the applicability of that work at this point, where before, we just kind of dropped it and didn't look at it. But what did we learn from that, and perhaps sometime in the future, we will do some future work. I'm not under the impression that we were planning to do specific pieces of work in Yellowstone National Park. No one to contradict me?

CHRISTENSEN: Priscilla Nelson?

NELSON: Nelson, Board.

I find this interesting. I'm very happy that you're pulling the many lines together, but it's unclear to me right now exactly to what extent these are going to be anecdotal, and present an assembly of cases generally referred to, or to what extent they're actually going to be used to perhaps validate models or processes that have been asserted as operating in the mountain. And some of those might be like capillary barriers, the assumption about influx related to rainfall, precipitation, effects of natural ventilation. A lot of those excavated openings, whether they be in Egypt or elsewhere, on the test site, Rainier Mesa, we went into a couple of the tunnels and some of them are dripping.

I guess the idea of actually using these as more than examples of how to think about how the mountain performs, but more than that, to actually link it into effectively the experimental program, and actually deal with the gleaning information from it that could actually be used in validating or extending models.

VAN LUIK: I think you've hit--you know, that was a long question, but I think you've hit on something when you read what we have in there right now, you will find it's a mixture of all of the above. In some cases, there is explicit analysis that shows that the modeling is on the right track. In other cases, like these, they're anecdotes that are just generally support the idea that there is little seepage, for example, into openings. And in some cases, the anecdotes need further analysis, and I think this is why we're going to, you know, continue this work.

Let me tell a little story about Spirit Cave. The Amy Dancee at that time employed by the Nevada State Museum was the chief anthropologist. I read every paper that she had written, and then called her and said, "My observation is that the upper mummy in sand was very well preserved. The lower one was not as well preserved, because it was lower and there was more moisture." She said, "No, you wouldn't get this from reading the papers, but the moisture conditions were exactly the same. What happened was that the lower one, a rabbit burrowed into it, built its nest in its chest cavity, and basically that destroyed that mummy. The upper one was protected by rocks, and so the rabbits, you know, just couldn't get to it."

So, sometimes when you read the literature, you get one impression. When you look at the reality of the situation, you get another. And the reason those sites are so awfully dry, even though there's more precipitation there than here, for example, is because they are open to the atmosphere, and so they never reach that 99 to 100 per cent relative humidity. So, you know, an opening is much better than a cave for preserving things. And you were alluding to, you know, what was the exact water balance, for example, the water budget in some of these anecdotal things. It takes a lot more analysis than just listing them to make them applicable to your modeling.

NELSON: Well, I like the idea of rocks protecting. This is good. Keep it in mind. But let me just come back to the fact that--or the question is this particular part of the project on multiple lines of evidence really integrated with the experimental information producing part, in terms of strategies for opportunity finding and model validating?

VAN LUIK: The answer to that is in large part, yes, it's the same people involved. And in some cases, we are diligently working to bring it into the mindset of the scientists working the issue. But we do have basically the GSs looking at these things, and it's integrated into the work that they're doing. So, the answer is it's becoming more and more yes as time goes on.

CHRISTENSEN: Don Runnells?

RUNNELLS: Runnells, Board.

I was going to ask a question that Dick asked, that is, what is being done in terms of Yellowstone Park, and then your answer was that work was done early, but probably nothing going on now. That has always surprised me, frankly, that you haven't carried through with Yellowstone because of a couple reasons.

Number one, it's clearly a coupled thermal hydrologic-chemical system. More than two reasons, I guess.

Number two is Bo Bovardsson has a strong background in geothermal systems, I'm sure, and probably implicitly thinks about these things. When he does his modeling, I'll bet a hundred dollars that he's thinking, incorporating what he knows about those systems.

And, number three, there's a huge long history of work at Yellowstone by others. DOE doesn't have to do it. USGS has worked there forever, and I would hope that--well, almost forever, not quite forever, but almost.

I would urge you to rethink the Yellowstone situation as possibly ripe for plucking in terms of testing predictions, in terms of testing models. The closest I've seen you come to that is at Peña Blanca. There's some good analytical work I think going on there to apply at least to your models, to a natural situation. I think Yellowstone stands in the same sort of category.

And I'll just repeat what other Board members have said. Three years ago, or so, this was sort of lip service that DOE was paying to these natural analogs or to the multiple lines, and you've come a long, long way in going beyond that.

I finally want to thank you for sharing with us the story about the rabbit in the chest cavity just before lunch.

VAN LUIK: As a vegetarian, that doesn't bother me at all. But I think you'll be pleasantly surprised by the write-up on Yellowstone, because we have, and in fact Bo is involved in this, we have exhaustively exhumed the literature on that.

RUNNELLS: Back to the archeological sites. You've heard me ask a couple of times about the Repository Safety Strategy Report, and that report contained the best synthesis that I've seen from DOE concerning natural analogs. And I suspect that you're probably building on that, at least I hope so.

VAN LUIK: Yes, we have the same author working for us on the discussions of analogs here to give it an integrated feel and view, yes.

CHRISTENSEN: David Diodato, Staff?

DIODATO: Diodato, Staff.

Back to the earlier comments. I mean, the

Department has obviously recognized that multiple lines of evidence may possibly be used to build confidence in the process models and other predictions that the program has made, so that's encouraging.

But we look at some of the stuff under the unsaturated zone, Chapter 11, Page 11. We might just go ahead and bring that up. And the concern that I have is that if we're trying to build confidence in the models, for example, that Item 11.3.3, the discussion there mostly centers on matrix diffusion between the random time transfer function implementing the FEHM approach to unsaturated zone transfer, versus the DCPT, and they give different results. So that doesn't necessarily build confidence in the program's predictions of that.

And then with the drift shadow zone, at the High-Level Waste meeting last week, we asked the principal investigator about that, what was the evidence that the drift shadow zone would occur, and they said, well, the seepage model predicts it. And I said, well, the seepage model is what we're trying to build confidence in, in part, so that doesn't make you feel so much better, and you might point to other examples if you know of any, or the drift shadow zone would be something that you could believe in, or some other line of evidence to support that. I just kind of toss those out as comments or ideas.

VAN LUIK: One of the reasons that I didn't highlight anything here and give you examples is because these are still under construction, and the challenge that we have given the authors is fill in other lines of evidence if you can think of any. Now, the confidence building would be a very nice outcome, but it could also be what are the insights that you gain from looking at other related systems, and these kinds of opportunities. And the insight might be that your model is lacking in some sense, too. It's possible that that would be the outcome.

CHRISTENSEN: Other questions from the Board? From the staff? Is this in response to one of the questions, Bob?

ANDREWS: This is Bob Andrews, BSC.

Let me just follow up on Dave's question. It's a very good question. Not everything that we're doing, you know, is focused on multiple lines of evidence. I think we're taking all the Board's concerns equally, and some of those related to uncertainties and a meaningful quantification of conservatisms that were in the Rev. 0 analyses and models. And the two that you cited, Dave, you know, on the drift shadow zone and comparison of matrix diffusion models are both primarily getting at the conservatism issue.

The mass release from the engineered barrier system into the unsaturated zone was very conservatively treated. I think we talked about that a little bit in January. Bob MacKinnon is going to talk about it a little bit more this afternoon, and that's that 11.3.1 issue, and removing that conservatism, we wanted to evaluate the significance of that potential alternative conceptual model.

The same is true of the issue associated with the comparison of various transport algorithms for unsaturated zone transport and in particular, associated with the matrix diffusion comparison between those, which is that 11.3.3.

Again, the Rev. 0 analyses used a very conservative particle tracker within FEHM rather than that dual continuum particle tracker that's represented there. And I believe the results that were shown last week showed the degree of conservatism, at least at a subsystem level. Whether or not we carry some of those subsystem conservatism analyses into evaluation of their significance from a system performance perspective, you know, is decisions that we're still wrestling with. Sometimes we don't need to. I think the Board has correctly pointed out that there's a lot of value to be gained by looking at the significance at a subsystem or component level without always going to performance assessment. So I just wanted to clarify that a little bit.

VAN LUIK: I think I need to clarify what he just said, though. We basically move into Bill Boyle's talk and his topic with that comment. The reason that these are on my list is because there's also a subsection under these saying other lines of evidence, and I believe in those two categories, right now there is nothing in them. But the challenge is there for the authors to bring in the basis for their insights and document them in those spaces. And so what Bob says is absolutely true. We're hoping that the authors will come forward and, as David pointed out, they didn't do so in the exchange at the last meeting. But I hope they will come forth and fill in the blanks.

CHRISTENSEN: Abe, thank you very much.

Mr. Chairman, I turn it back over to you.

COHON: Thank you very much, Norm. I appreciate your fine job as Chairman.

We'll now turn to a public comment period. Judy Treichel is the only person to have signed up.

Judy, would you like to come up here?

TREICHEL: I want to thank Abe for giving the fastest presentation he's ever given. And I want to thank the Board for making this time available, because this is really, really important.

Last Friday, a bunch of documents were dropped out, and a public comment period was officially started, and this Friday, another extremely important public comment period does start. And I've got one question for Lake, and this is a yes/no, that's it, because this is important time.

But I want to know if the train has left the station, if we are officially in the site recommendation situation right now. Have we officially entered that phase of the project? You wrote a letter to the governors which you've signed, and the Department put out documents and started public comment periods. Does that mean that we are now officially in the site recommendation process?

BARRETT: We've entered into the next phase. Whatever the Federal Register notice and my letter says is what we're doing.

TREICHEL: So it is in the eye of the beholder?

BARRETT: Well, that's what the letter says. We've entered into the public comment period, and it is extended, there is no close date on it.

COHON: Well, Lake, you might just review what you and/or Steve mentioned before about what the law requires in terms of the site recommendation. Was it Steve that talked about this?

TREICHEL: It seems real fuzzy. I can't tell if we're actually doing our site recommendation thing right now.

BARRETT: The science and engineering report has the bulk, not all, but the bulk of the scientific bases that would be the scientific foundation of any potential site recommendation. We had that pretty well written, and we felt it very important to get that out to everybody when we had it. Okay? And we think it's the best articulation we've ever had to date of the performance of Yucca Mountain and the uncertainties and the work that's going on. So we felt it very important to put that out, and we wanted to put it out for comment for all. So that's why we put it out. We felt that this was getting close enough to, you know, in the process, so we announced the initiation of the public comment period, and that's what in the Federal Register notice.

Now, exactly where is that? You know, it's not specified in the Act. It just says we shall have public comment. We shall eventually have hearings. We did not schedule the hearings. We did not schedule a close, because we feel there is more information that's needed before we reach those points.

TREICHEL: But you are officially considering recommendation of the site? Yes/no?

BARRETT: Let me look into my Register notice before I can answer it. If I'm only allowed yes or no, I want to go back and look.

TREICHEL: Okay, that's fine. But as was brought up, and I want to thank Debra very much for having asked the question about isn't the public at a disadvantage when you're talking about things that comply with a guideline, or a rule, that's not yet final? And, yes, the public really is, but there is even more to it than that, in that I'm guessing the

Board probably has seen what that proposal is, but the public isn't allowed to. We've asked for what is the proposal, even if it's not final, what is it that you're working with, and we're not allowed to see that, or the NRC thing. We've discussed with EPA what theirs is, and we know that there is a range of options, and they're more open about it. But we're not able to see any of these things, and it--

COHON: Judy, let me interrupt. You seem to think the Board is privy to material that you're not?

TREICHEL: I was assuming that you probably had seen what's being proposed as 963.

COHON: Oh, proposed? We commented just the way the public commented. I think that's all we've seen.

TREICHEL: Then none of the public comments were considered if that's the same thing that came out.

COHON: Well, DOE can speak for itself. But not acceding to a public request doesn't mean it wasn't considered. It was considered and rejected. That's always a possibility.

TREICHEL: Yes, okay. In King's presentation, there was a last bullet on Page 8 that said that 963 will be updated based on public comments. But if that's in the future, you're using something else now to show compliance, or we're being told that the SER is based on this preliminary proposed guideline. So it's all very, very fuzzy. I'm not here to argue with anybody. I'm just saying we've got a real problem, because we really don't have any firm footing that we're standing on, and we don't know. This would almost indicate that there would be another public comment period at this stage of the game, but I'm sure that that's not the case, on 963.

And then there was another slide on Page 17 of King's presentation that there was a revised site recommendation approach. Well, if we've already been given a document that's part of a site recommendation to comment on, then I wouldn't think that there should be a revised site, that was why I asked the question, a revised approach to site recommendation.

And another thing that seems backwards is the new ornament that's hanging off the pyramid with the supplemental science and performance analysis, which is not yet done, but which feeds the science and engineering report, which is done and on the street and out for comment. So it's very, very difficult then.

In Larry Trautner's presentation, there is still more talk about learning new information, and that the design is in the conceptual phase, and we've just been thrown at us, the public, a supplement to the EIS regarding the design. And it's still being talked about as conceptual, and I will tell you as a personal view on this thing that when the public sees those new drawings with the various layouts, and getting longer and bigger and more stretched out, that to them is not going to appear to be--and I'd love to know that they're wrong on this--but it's not going to appear to be managing heat. It's going to appear to be making this place capable of taking way, way, way more waste. And it would be nice to be guaranteed that all that was was flexibility for a heat load and not flexibility for how much waste gets piled in there.

But I want to make it very clear to everybody here, and I think you knew that, but you need to really know it, about how difficult this is, because we've got a 45 day comment period on this supplemental EIS document, and yet it's still a concept. And so there might be a whole lot of those going, but the clock's already running, I think, on site recommendation.

So, thank you.

COHON: Thank you, Judy.

Are there any other members of the public who wish to comment now? Steve?

FRISHMAN: Steve Frishman, State of Nevada.

This won't be my usual type of comment. I was just thinking about Priscilla's dilemma over flexibility, and it occurred to me that quite a few years back, I remember commenting to the Board that someone should be watching very carefully whether the MPC was driving the repository design. And I think we're in that situation, and I think it's finally come to a head.

At the time that the MPC was the rage, the conceptual repository design was for vertical emplacement. And vertical emplacement of relatively small containers, and vertical emplacement of probably stainless steel containers. Now we have a design that is a response to a transportation and storage concept that is no longer the concept that the MPC was.

So I know it's sort of a tired term about thinking out of the box, but I think we are deeply buried in the box for a reason that had nothing to do with respository design in the first place. So, just keep that in mind.

COHON: Pyramid might be a better metaphor than box at this stage.

Other comments or questions from the public?

(No response.)

COHON: All right, thank you very much. I thank all our speakers this morning.

To all of those who have complained in the past about our short breaks, keep this one in mind. We will reconvene at 1:30.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

COHON: Board member, Alberto Sagüés will chair the meeting this afternoon. Alberto?

SAGÜÉS: Good afternoon. I'm Alberto Sagüés. We're going to start the afternoon session that deals with uncertainty analysis and performance assessment, and we're going to go straight through it. We're going to have our first presentation that is entitled Uncertainty Analyses: Current State of Activities, and it's going to be presented by William Boyle.

BOYLE: Good afternoon. Thank you for this opportunity. It's a follow-on presentation to the one that I gave in Amargosa Valley a few months ago. And as I mentioned at the Amargosa Valley meeting, what I present represents the work of a lot of other people, and I can't thank them all, but I would like to acknowledge Ralph Rogers and Kevin Coppersmith, and Bob Andrews and Dave Szrubian (phonetic) for the TSPA portions, and some of the new modeling is by Mike Wilson and Cliff Ho of Sandia. Another new modeling is by the people at

LBL that work for Bo, and also some calculations by Tom Buschek. And I'd like to thank Rob Howard for getting it all into Volume I of the SSPA.

So, the briefing objectives, they're shown here in the bullets, and as at Amargosa Valley, I'm going to start off with a review of the uncertainty treatment in the Rev 00. I'll then switch to a question that Dan Bullen had this morning, or a comment, what's in and out of the updated TSPA. I'll introduce some of the subsequent presentations of the afternoon, and present some examples myself on new models and new data.

Okay, I talked about this topic at Amargosa Valley. There was a review of the uncertainty treatment in the Rev 00 analysis and model reports, process model reports and TSPA-SR that was requested by the DOE and it was conducted by our Management and Technical Services contractor. So it was an independent group of people reviewed the treatment of uncertainty. And what they reviewed were these things, and it's all documented in a report that was recently delivered to DOE, and I think copies were delivered to you, to the Board.

And what the reviewers were asked to do is look at, well, how were these things documented in these documents. It wasn't a task to ask people how would you do it. It was, well, go out and find out what was done, including looking at the model relationships, you know, what was the source of the uncertainty, how it was treated, the impact of the uncertainty, and how it was documented.

Well, this is a graphic figure from that report, and this is a construct that the reviewers found helpful to themselves. Some of you were present at the High-Level Waste Conference last week, and there was a session on uncertainty, and Hans Riotte of the NEA mentioned some NEA documents that looked at this topic of uncertainties in TSPA. And the approach used in our review report follows along those same lines, but a little more detailed, that the TSPA documents from the NEA, they do deal at a TSPA level and didn't go down to the level of detail that the reviewers associated with this report did. They went all the way down into the analysis and model reports, which are based upon tests.

But this construct they use is very similar to what you might find in one of those NEA reports, and it shows that although it's possible to start dividing an overall model up into parameter inputs and conceptual model and the representational model, that these aren't mutually exclusive items with, you know, hard boundaries, that the minute you choose a conceptual model, it starts to affect the parameters and inputs associated with it.

But the review is organized around this graphic. So if you go into the individual chapters, you'll find that the reviewers commented on, well, how were the uncertainties related to the conceptual model handled? How were the uncertainties related to the parameter inputs handled? And also, you know, how were uncertainties in the abstraction handled, and also the representational model.

These types of charts, you can see this was for the seepage model, and these were created for each and every one of the major models that was reviewed, whether it was seepage or UZ flow, unsaturated zone flow, or saturated zone transport, and it shows a linking of the models and analyses and what they feed into. And such a diagram, the reviewers found helpful, particularly with respect to tracking, well, how uncertainties were propagated or transferred through from one model to another, to another, and on into an analysis.

So, the review of the uncertainty treatment. These are the principal recommendations from the review, and I'm pretty sure these are word for word verbatim, cut and pasted out of the report. And so the first is consider developing a systematic process for identifying, documenting, characterizing, evaluating and quantifying uncertainties.

As I've already mentioned early on, like those divisions between parameters and conceptual models and representational models, they're not hard and fast and distinct, and perhaps we should, as one example, develop a more systematic process for identifying them, which would then help in terms of quantifying the uncertainties.

Another principal recommendation, provide better discussion of the bases for determining parameter values and probability distributions. Many times, you'll have a dataset, let's say measurements, and at first glance, any number of distributions might seem to fit the data equally as well, and yet these different distributions have different parameters associated with them. You know, maybe it's a one parameter distribution, or a two parameter distribution.

So as people choose a particular distribution, and the parameter values associated with it, perhaps we should have better discussions.

Related to that is perhaps provide more robust and consistent justification for parameter and model bounds. For example, should we use a 95th percentile value, or a three standard deviation value, or three orders of magnitude greater, or something. We should consider having a more consistent justification for the bounds that we do use.

It was recommended for each of the large complex models, have an overall conceptual model analysis and model report, and also to improve the conceptual model discussions within the analysis and model reports.

The unsaturated zone does have such an overall conceptual model analysis and model report. So it's a good example of how to do this.

And, in general, in the course of this review document, you know, many reviews will tend to focus more on the things that could be done better, but this document, in addition to identifying the things that perhaps could have been done better, also identified areas where things were done well, to use as examples or templates throughout the rest of the project.

And there was the fifth bullet there, which was be certain that we describe how uncertainties are propagated and incorporated through.

All right, so I'm switching topics now. I've been talking about this review report, which I think has been made available to the Board. If you have any comment on it through time, you can get them to myself or Claudia Newbury or anybody else.

But I'm switching to a new topic, the supplemental science and performance analyses that's already been mentioned by Steve Brocoum and Jerry King and others today, and it's the supplemental scientific investigation and analyses that have been ongoing since the completion of the Rev 00 documents.

That supplemental information is being developed to provide additional data to support the suitability evaluation, as mentioned by Jerry King and is shown in black in that pyramid. And I believe Jerry mentioned this this morning, that there were three basic new types of information. We continue to quantify some of the unquantified uncertainties. There was ongoing testing and updating of models. That was since the completion of the Rev 00 documents. And we also looked at some models and analyses to examine the range of thermal operating modes.

And just as with the division between conceptual model and representational model, there aren't hard and fast, you know, discrimination between some of these items.

Slides 9 to 16, they're a long table, and this slide gives the headings for the columns of that table. And what those tables capture are the things that will be in Volume I of the SSPA, supplemental science and performance analyses document, and also what will be in Volume II. And as I think Steve or Jerry mentioned this morning, Volume II of the SSPA will be TSPA calculations, total system performance assessment calculations. And Volume I will be the new models and new science and new data upon which the new TSPA is based.

So as you look at those pages, 9 through 16, you'll see that the first column is Key Attributes of the System, and it's a brief description, and we borrowed those from the repository safety strategy and it consists of items like limiting water contacting waste package, prolonging waste package lifetime.

The second column is Process Model Factors, which that's the next level of detail down. And in that column, you'll see reference to the chapter in the science and engineering report that was just published where that particular process model factor is discussed.

Then the next column is even a greater level of detail, the particular topic of the supplemental science analysis.

Then the next three columns are these, and it's, well, what was the motivation for including this work in the first place? Was it driven by a consideration of quantified and unquantified uncertainty, or was it driven by an update in scientific information, or was it driven by consideration of looking at the range of operating modes. And these, no matter which one of the three, or two of the three, or three of three drove the new information or new modeling or new analyses, this will be captured in Volume I of the SSPA.

Then the last two columns will indicate, well, how was it treated, if at all, in the TSPA, which would then be captured in Volume II of the SSPA. And the two types, there's the two columns that deal with was a TSPA sensitivity analysis done, holding everything else constant, as in the base case, just change one thing, that's the sensitivity analysis, and also the final column was was it included in the supplemental TSPA model. And what's meant by that is in

Rev 00 ICN I, which I think Steve Brocoum referenced this morning, published in December of last year, that's a hot TSPA.

And what's being done in Volume II is we're slowly adding some of these things to it, keeping it hot to begin with, and what we'll end up with is a new updated hot TSPA, supplemental TSPA. And after that's all updated, then, in a sense, we'll turn the temperature knob, and run that updated model, but as a colder TSPA.

So at the end of Volume II of the SSPA, you'll be able to make a comparison between the updated hot and cold TSPAs, but you will also be able to compare the new hot supplemental TSPA with Rev 00 ICN I, the older hot TSPA.

So, it might help to go through some examples here, and if you could--well, to reiterate, this is really the Xs tell you what's in Volumes I here, or in Volume II, technical item by technical item, and it's cross-referenced to the Chapters in the science and engineering report.

So can we jump forward to Page 12? Spent nuclear fuel, no changes whatsoever were made, no Xs at all, is one example. So not everything is being changed from Rev 00.

Can we go back to Slide 9? Another example is down here at the bottom, effects of drift degradation and rock bolts on seepage. You'll see that it was primarily motivated last summer as part of the unquantified uncertainty analysis.

We were going back to the scientists and asking them, well, how might you do things differently, different representation. So, they actually did analyses related to drift degradation and rock bolts, and those analyses will be documented in Volume I. But it was determined that as a result of these analyses, the end results weren't that different, so it was never propagated through to TSPA. Based on the changes here, the subsystem results didn't change that much from the Rev 00, so these changes and models weren't propagated through here.

Now, one thing I want to bring up is is when you see no Xs for the TSPA in Volume II, there will be at least a qualitative description of the different models that were considered. So, just because there's nothing here doesn't mean that it wasn't documented in Volume I. If it's over here, it will be documented at least qualitatively and descriptively in Volume I.

All right, so I've shown you one where it was driven by an unquantified uncertainty analysis. This one shows where new analyses were driven by consideration of the cooler thermal operating mode analysis. And I think Rob Howard mentioned this this morning with respect to the repository footprint, that they weren't going to include that new footprint in, but people did do some, you know, process level models to look at, well, what would the infiltration be. But as you can see, it didn't propagate through the TSPA.

And as a final example on this page where something else drove the change, effects of lithophysal porosities on thermal properties. It's listed in the update and scientific information, representation, the model representation for the thermal conductivity as driven by the lithophysae, that model was changed, and so there will be a description of that. But it wasn't propagated all the way through as a TSPA sensitivity analysis, nor was it propagated through to be included in the updated TSPA.

All right, so there's five columns here and they either have an "X" or they don't. So there's two to the fifth, or 32 different combinations, and I'm not going to go through all 32. You can go through yourself. I've shown you where individual items were considered and not carried through to TSPA. You can see that sometimes the work was motivated by consideration of two items, and actually all three couples are there. You can go through the tables and you'll find an entry where there's an "X" and an "X" here, or an "X" and "X", and at least one entry has Xs all the way across. And for these columns, you'll find areas where there's two Xs or perhaps just one "X".

But I just want to reiterate as you go through all these, this is a brief summary of what's going to be in

Volumes I and II of the supplemental science and performance analyses.

Okay, now, what's in for the rest of today's examples? We're going to have some discussions of how the TSPA has been changed. I'll give some examples, and in response to some of the questions, some of the other speakers.

You're going to hear right after my presentation from Saxon Sharpe and Jerry McNeish on these two. I will talk about this one myself near the end of my talk. If you go to Page 11, Rob Howard is going to talk about, I'm pretty sure, some of these in here on the waste package performance. And go back to Page 10, Bob MacKinnon is going to talk, I think, about this one and perhaps some of the others down here, and also on Page 13. I think Bob MacKinnon has a couple of these in here, I think it's this one and that one.

So these are some of the examples you'll hear today. We're certainly not going to discuss all of them, but I think it's also important to point out some of the things that won't be in the TSPA for Volume II. So go back to Page 11, if you can.

Here's the local environment, you'll see that, as I've already mentioned, there will be a description of how these are handled in Volume I, but they're not propagated quantitatively through into the TSPA, nor is the long-term stability of passive film. You'll see we'll have a descriptive treatment of this in Volume I, but it's not propagated through into the TSPA.

So now we can jump past those tables. You're free to keep them. That's a good summary of what's in Volumes I and II. You know, the TSPA calculations for Volume II are still ongoing, and so if anything, some of those items may fall out. They may find that it's just too difficult to actually incorporate the change within time. But many times the analysts look at it and say, yeah, I can incorporate that change, but then when it comes to actually putting it in the code and running it, sometimes they run into difficulty.

So, since January, more of the uncertainties have been quantified, and we'll see some of them today. Also looking at the sensitivity analyses to determine which of those should make it into the TSPA. If it doesn't have much of an effect, we might not put it in the updated TSPA.

And so related to that, they're being incorporated for those that the results of the TSPA are sensitive to it, and we're also trying to incorporate things that may shed light on the difference between the hotter and colder ends of the thermal range.

So, here's what you're going to hear the rest of the day. From myself first, I'll talk about some seepage models in the drift shadow zone, to be followed by a talk on climate and net infiltration. You'll hear from Rob Howard on waste package and drip shield, and you'll hear from Bob MacKinnon on the engineered barrier system.

Now, for many of these, they actually represent brand new models where we have no model at all in the Rev 00. On the other hand, others represent new information, and I believe you'll hear from the speakers perhaps on which is which.

For my own, I'm going to talk about flow focusing. We had a model, but we changed it. We changed the range over which it operated. This episodic infiltration isn't in Rev 00 at all, so this is a brand new model. And we revised the seepage model, because of new test information from the Exploratory Studies Facilities, which that new test data led to revised block properties.

But we also included thermal effects, that in the Rev 00 TSPA, they used to do calculations to find out, okay, what happened to the water around the drift, would take it five meters above the drift, and use that as an input to a more detailed seepage model. But that seepage model itself was isothermal. It didn't have thermal effects, even though heat had put the water up there in the first place. But we have now incorporated thermal effects in the seepage model.

So, even though flow focusing was shown first, I'm going to go to episodic seepage. And what this is is I think setting aside rocks for a minute, is people see this on pieces of glass with mist, or even on your pitchers of water, that you'll see that eventually a drop will get enough water vapor, you know, and it will get converted to liquid water, gets big enough, and then it will run. And you can get similar things with rock fractures due to their rough nature. The water can hang up in a fracture and acts like a little dam, and the water builds up behind it until it reaches a sufficient volume, and then it all just flushes through.

So, we didn't have anything like this at all in the Rev 00, so now we have put in a new model that switches to a system where there is flow, no flow; flow, no flow, along the lines of, you know, like a drop that comes down, you wait ten minutes, another one will come down. So that's what we incorporated.

And by incorporating that into it, you'll see that just looking at the mean seepage flow rate, it leads to an increase in comparison to the Rev 00 base case. In black, it will always be the base case, and the new model or the new data will be in red. This change here represents a climate change at 2000 years, incorporated over a time step.

And so when they put that new model in, they can run it through the TSPA, and you'll get these horse tails. But the important thing to look at is, well, what does it do with respect to dose. We see that there is an increase in seepage flow rate. But for dose, we don't see an increase in dose until we get past 50,000 years, and that's because these releases really aren't driven by seepage, so increased seepage doesn't affect this part of the curve. But you'll see with this new model, that there is an increase in dose rate.

And the reason it led to higher seepage fractions and seepage flow rates is you can think of these asparities as capturing water that normally would have just gone around the drift and never shown up as seepage at all, and now it gets sucked in behind this dam and actually does contribute to seepage.

Here's a different change we made. We had a flow focusing factor before, and you can think of this as a funnel, and we were really uncertain about it in the Rev 00, so it's a multiplier, if you will, we let it range from essentially one, no multiplier at all, up to 50, which with a number of 50, it implied that the flowing fractures, the fractures out there, were 20 to 30 meters apart.

Well, based upon more modeling, and also examination of the rocks out there, people thought, well, maybe it's not. Maybe the flowing fractures aren't that widely separated, and they led to a new representation. Instead of a factor from 1 to 50, it now is from 1 to 6, exponentially distributed, with a mean of 2. So that leads to mean seepage flow rates that have decreased from the base case. The base case in black, and here's the new representation.

But what we did here, as I've already mentioned, we had a big funnel, a few big funnels, if you will, in the Rev 00 TSPA, and what we did here was we replaced the big funnels with a lot of smaller funnels. So that led to an increased possibility of seeps, but each one had less seepage, and when you put the two effects together, it leads to no increase in dose, because here, you see the seep flow rate, but we're not plotting up--I mean, decreased flow rate, but we're not showing an increased possibility of seeps, but that is captured down here. So those two cancel each other out.

Now, this has in it the revised test data. And so we also changed it to a thermal model, but these results only show from a thousand years on, and it shows that in this case, the updated model has a higher mean seepage rate than we had in the base case. But as with the first example I showed you a couple slides ago, because the early releases aren't dominated by seepage, the increase in seepage doesn't cause an increase in the mean dose rate. But we do see a slight increase here, and it's because diffusion is dominating the results.

All right, this is the drift shadow zone with the concept that, you know, the drift itself causes the water to go around it, and it will end up drier underneath the drift. And I think Bob Andrews mentioned this in part this morning in response to some questions. This isn't represented at all I think as a drift shadow zone, not by that name in Rev 00, but what we had in Rev 00 is whatever radionuclides were in the drift, the minute they got to the rock, they were all assumed to be in the fractures, which is the most, you know, that has the fastest transport.

So what was done in this model, it was a switch. They switched the model such that if a radionuclide in the drift was diffusing, they put it into the rock matrix, and if it was advecting or flowing, they put that in the fracture. So now instead of having everything in the fractures, as in Rev 00, there was a partitioning. Now some of it is diffusing through the matrix, and some of it is flowing through the fractures. And by doing that, you lead to delayed releases, about 10,000 years, and also a decrease in the amount of dose.

And they're looking at changes in this model, particularly with respect to how you treat that interaction between the fractures and the matrix, the two different continuum, because not only is there advection, but there's diffusion, and do you allow it one way, and if it's one way, which way, or both ways, or no way.

So, here's what you heard at Amargosa Valley. A lot of things have changed since then. The SSPA didn't even exist then at the end of January, and now it's roughly a thousand single sided sheets of paper, and it's due to be delivered on June 1st.

So, this is what you heard we were doing, and the reports that were going to be generated, and they were discussed at the January meeting. Here's what's being discussed today. You've heard reference to the SSPA, Volumes I and II.

And I'd say at a high level, the technical work hasn't changed at all. It's just which report that things end up in.

The evaluation of uncertainties is a work in progress. The review provided valuable the lessons learned. The SSPA is providing additional insights. And we are specifically looking at the thermal dependencies.

And with respect to the ongoing work in progress, I believe Abe Van Luik mentioned at Carson City last year, and I know I showed a slide in January at Amargosa Valley that, you know, this really is ongoing for a long time to analyze the uncertainties and evaluate the significance and communicate them and manage them.

So, that's my presentation, but I saved a few minutes here at the end so that I might personally try and answer the question posed earlier today on how to communicate these uncertainties. And this is my own personal point of view. I'm not speaking for the Department right now.

But first, I believe it was posed communicating to decision makers, and I think first of all, what would be communicated depends upon the particular decision maker. For example, if Lake Barrett is the decision maker, he knows an awful lot about Yucca Mountain and is very comfortable with statistics and all the PMRs and everything else. And so that can be done at one level.

The Secretary of Energy is probably a different matter. You know, I sincerely doubt whether any Secretary would read all the AMRs, PMRs, SSPA, and all the rest, and my guess is that a decision maker at that level is going to want something very distilled. And it probably won't be myself, I wouldn't presume that, it might be somebody like Lake, but if it were myself, I personally would just take in a few pieces of paper to try and get the point across.

For example, one piece of paper or one bit of knowledge I would bring in is with respect, well, what happens at 10,000 years, because that is the regulatory time frame, and I think what we'll find is we'll do our TSPA for post-closure performance, and whether it's 300 runs or 500 runs, we'll find that a very small number of those have any dose at all at 10,000 years, and that dose for those that do have dose, if any, it's a small dose. And I would probably show it's small by reference to what somebody gets in Denver or Washington, D.C. So that's one bit of information I would try and get across. But it would just beg the issue of, well, does it get better or worse?

And so another plot that I might show is the probability density function of peak dose. No matter when it occurred in time, just go through a horse tail diagram and pick off the peaks and plot them up. And the reason I would use a probability density function is even though many people don't deal with probability and statistics, I think just in the course of going through school, people hear of grading on the curve and the bell shaped curve, and most people can recognize a roughly bell shaped curve, which is probably what we're going to end up with.

And I would show that curve to a decision maker, and I would focus in on probably the mode. Rather than the mean or the median, I think most non-trained people, they're just going to look at the peak of that curve, and I would compare that, I would have on that plot, you know, what somebody gets in Denver or Nevada, or perhaps Washington, and show how far we are away from it. And at the same time, the decision maker might say, well, okay, that's what this curve looks like today, but is it going to move tomorrow based upon new science or a new model.

So another thing that I might show is our older, more conservative TSPA, and show that, well, here's how it changed as we became less conservative. And if anything, we still have conservatisms in it. It's hard to avoid them. When you switch to idealized model, the odds are, if anything, we're going to add conservatism rather than optimism. So I would try and convince the decision maker, if anything, we'll still continue to move in that direction.

And then the final thing would be not withstanding that, there still is some possibility that the curve would move in a bad direction. But I would point out that, you know, we've studied the mountain for a while, and we don't expect that to occur, but we also have the requirements for performance confirmation and monitoring, such that if anything bad did happen, and we were starting to move in the wrong direction, we'd actually know it.

And so that's how I personally would try and communicate the uncertainties.

SAGÜÉS: Thank you, Dr. Boyle. Some questions? Dr. Cohon?

COHON: Cohon, Board.

Bill, I think that's excellent. I think that's an outstanding answer, and I really congratulate you and admire you for putting it that way. From my personal point of view, that's the maximum I think I could have hoped for in terms of what you would be able to do and would be willing to do with the communication of uncertainties to the Secretary.

One fine point in presenting that PDF, I think it's worthwhile putting out to the Secretary the extremes, the mode and probably the median to talk about, but it could be as low as and it could be as high as, but the chances of as high as are one out of 500, or however you quantify it. So, great. Congratulations, and I find this, everything you just presented very impressive.

A couple of questions about the big table, and how all of this may be used, sort of the content of what will be behind whatever the Secretary sees. First, on the table. It cannot be avoided that judgment, technical expert judgment, as contained in the program, has to be applied in deciding which of those many parameters should make it to the next step of analysis using a sensitivity analysis with TSPA, and then inclusion in the TSPA.

There are a couple things about that that I'm confident you're thinking about, but I want to ask you as much to get on the record as anything else. One is that you're looking at the subsystem level, one could see small changes, but if it's a very important parameter, it could have big implications, or bigger implications for TSPA for dose than one might expect from just looking at the subsystem. So that's one issue.

And if I understood you correctly, the first screen was basically, well how much change do you actually get in that parameter, seepage, or whatever it was.

BOYLE: Right.

COHON: The other thing is the various parameters and processes interact. So a small change here and a small change here could together produce a significantly larger change. How do you deal with that? And I have one more followup question.

BOYLE: Yeah, both points are well taken. It is a non-linear system, and a seeming small change here could actually lead to a big change elsewhere. And with respect to that, and also the coupling, you know, the interactions, I guess my answer would be is personally, I'm relying upon the expertise of the analysts. You know, they've done enough TSPAs up to this point that they generally know what have been the more important things or not. So when they're looking at the subsystem level and they don't see much of a change in whether it's seepage or something else, they already know from the prior calculations that in the bigger picture, well, seepage doesn't make that--you know, they know that they need a bigger change in seepage to see it in dose.

And also, on the interactions, I would rely upon the expertise of people to do that, in the absence of an exhaustive, you know, treatment of all of them. And so that gets back to the judgment point you made.

COHON: Just to narrow it down, the analysts you're relying on are the TSPA analysts, not the individual subsystem scientists or PIs; right?

BOYLE: I'd say both. They really do talk to each other and interact together.

COHON: Then my followup and my last question, and now I'm going to be tougher. Everything you're doing is great. I've sung your praises already. I won't do it again. It all sounds terrific, but it will only really matter if you really do all this, and then it gets used in whatever is presented to the Secretary.

So the question is will the supplemental TSPA get factored into, will it be part of SR?

BOYLE: Well, I'll answer that this way. I believe it was shown on the slides this morning, that the SSPA, it's in for certain, there's no doubt about it, it's being considered. The other issue is, as Steve Brocoum said, we spend a million dollars a day, and so we're going to continue doing work on the uncertainties through the course of the summer.

And then it becomes an issue of does that subsequent work, like let's say we publish a document in September, and let's assume that the public comment period is closed then, then we get to Judy Treichel's concern, if Judy is still here, now here is new information and the public didn't have access to it. That's a tougher call as to how that gets factored in or not, and I think that involves people senior to myself, and the Office of General Counsel, and things like that. But the technical work will go ahead.

COHON: Right. Of that I'm sure.

But just to nail this down, has anything precluded--I'm trying to figure out how to say this without too many negatives in the sentence--the dose numbers that the Secretary will see, is it possible in terms of the schedule right now that those numbers will be based on the supplemental TSPA rather than--

BOYLE: Yes.

COHON: Okay. Thanks, Bill.

SAGÜÉS: Dr. Parizek?

PARIZEK: Parizek, Board.

You came with some papers to the Secretary's office, and only a few pages, and without you, what would he get, or other people would get? You would have all these big volumes and all of the thousands of pages. You visualized sort of a simplified presentation to give the highlights. The rest of us, what would we read, without you, because you won't go everywhere?

BOYLE: Right, exactly. You know, there will be all the AMRs and PMRs and everything else. I don't know anybody who's going to read them all. I don't know anybody that has. And as you go up from there, you know, there's various distillations. You can view a process model report as a distillation of a series of AMRs, and up and up and up. And in one of the discussions this morning, and maybe it was Jerry King or Steve Brocoum mentioned look at the Executive Summary of the PSSE and the S&ER, science and engineering report. And Jerry mentioned we once had an overview. Perhaps we'll have some other descriptive document. I don't know. But I think the project is aware, depending on, like you as a group, I would think would not--one or two pages wouldn't be sufficient. Otherwise, the staff would go through it too quickly, you know.

So your point is well taken, that depending upon which group, which review group, which decision maker, but I doubt that Lake would be satisfied with just the page or two himself.

SAGÜÉS: Dr. Nelson?

NELSON: Thanks, Bill. I've got two questions that would help me to understand the full scope of what's going on here.

We've asked in the past about the uncertainties related to the fact the processes are actually three dimensional, and are in most cases reduced to a two dimensional, or in some cases, a one dimensional process, and raised questions about spatial variability. How has your review group addressed those from the standpoint of uncertainty?

BOYLE: You know, I'd have to ask Ralph. I don't know if that was specifically treated at all. To rephrase the question, is in the review of uncertainties, did people specifically look at that if a model was two dimensional, when in reality of course the world is three dimensional?

MR. ROGERS: Ralph Rogers, MTSI.

The answer to the question is we definitely looked at that when we were reviewing the documents. But, remember, what we did was look at what was said in the documents. And also in answer to your question, there are some places in our documentation where that issue is addressed quite thoroughly actually, because it clearly is the case if you're going to use a one dimensional model, that introduces some uncertainties.

NELSON: Well, that will be an interesting part of the report to look at, is how you handle the uncertainty introduced there. Thank you.

And, secondly, because you offered a couple of cases where you showed some of the seepage related analyses, I've got a question about how that propagates through from the standpoint of seepage, not episodic, but seepage rate considerations.

In the documents that you're talking about, would we see, for example, the thinking that went on about changing from one flowing fracture spacing to another?

BOYLE: Yes.

NELSON: And the rationale behind that?

BOYLE: Oh, yeah. Yeah, I was reading those sections over the last few days, and they're in Volume I. And, for example, on the episodicity feature, page after page of discussion, equations, and, you know, it's there if the staff wants to go through it.

NELSON: All right. Well, the propagation of that is that you've got more seepage locations, which combined with some measure of uncertainty about flaws in waste packages, could actually potentially increase the likelihood of a failure, because more are being impacted by the seepage.

And then following that through, you've also got the same sort of a scenario on the exit of water in terms of how frequently are those places where the flowing fractures are below the repository. Are those connections made all the way through?

BOYLE: In general, I'd say yes. What I showed today were like one-off derivatives, if you will, just changing one thing. And if you actually look at I think it's pages 20, 21 and 22, you will notice the first failures are the same in every one of those horse tail plots, because the waste package wasn't affected by the change. So the waste package performance was always the same.

So, in those plots I showed today, you won't see that coupling together. But as all these changes, all those Xs in the far right column of those tables, as they're all added in together, then they do, that's where the coupling will take place, if you will. Like if somebody, when Rob Howard gets up and talks about the waste package, if he changed those properties somehow, and its affected by an increase in the number of seeps, but less seepage, well, it should be taken into account.

NELSON: Thanks.

SAGÜÉS: Okay, we have about six minutes and three questions. Dr. Bullen?

BULLEN: No pressure, Dr. Sagüés. Bullen, Board.

Actually, this may be a little bit fundamental, coming back to the changes that you made in the PA analysis, but it may also help me understand if you make these kinds of comparisons. I look at, for example, Figure 21, which you just referred to, and I see that essentially the dose curve is the same. Okay? I don't see any difference in the lower right-hand corner.

BOYLE: Right.

BULLEN: But I've changed something that you say is essentially one effect is counter-balancing another.

BOYLE: Right.

BULLEN: And I guess the step that I'd be interested in having then walked through is to start with the TSPA-SR that you gave last December, Rev 00. Then you said okay, we've modified it with the unquantified uncertainties and additional data and model updates, and so you've twisted that knob a little bit. And then you're going to take it again and you're going to reduce it to a lower thermal operating regime, and you're going to twist that knob. But what I'd like to see are changes maybe in the important subsystem models, and you decide what's important, with respect to how I would see those changes and understand them without having to worry about masking. And I worry about masking because, well, you say it doesn't affect the waste package.

Well, of course, if the waste package lasts 10,000 years, then I'm not going to see the effect of if I dump all the water from the mountain or I dump none of the water from the mountain on it, if the waste package lasts 10,000 years, I get the same dose.

What I would really like to see is how in the subsystem, maybe not taking it all the way out to dose, but how in the subsystem do I get, you know, "X" more radionuclides or "X" less radionuclides because of the performance associated with each of those steps, hot repository, cold repository, on the subsystem level, so I don't see the masking. Because immediately when I see this, I know you explained it as counter-balancing effects, but I look at it and say, well, it's masked by the waste package.

So, I'm trying to figure out a way that you could present it that we would understand it and see that yeah, there is an effect, and we've got the effect managed. But in the grand scheme of things, it doesn't matter because the performance, the overall performance of the site isn't compromised. So is that too long and convoluted?

BOYLE: No, I think I get it. It's a number of responses. One is I believe that some of what you're asking for will be covered in Volume I of the SSPA, and isn't covered here just because that's a thousand pages, and this isn't.

But in those sections, each of the sections in Volume I always starts out with a description of what was in Rev 00, and the changes made to it, including, you know, why the change was made, with some exploration of, well, did it make a difference, and where. And although this may be masking by the waste package down here, here is a subsystem, you know, parameter, and this was on the other charts as well. You know, we're not talking dose. We do show that that's an order of magnitude difference in flow rate, a decrease, but it is masked or cancelled, it's cancelled by something else I didn't plot on here at all. I just, you know, mentioned and that's because although there is this decrease, there were many, many more of them.

BULLEN: Thank you.

SAGÜÉS: We're going to have to move fairly quickly here. We have a question by Dr. Runnells.

RUNNELLS: Runnells, Board.

On your Slides Number 9 and 10, Bill, if we could just look at those? Down a third of the way from the bottom, there are coupled effects on UZ flow. There are Xs in two boxes on the left column, and no Xs on the right column.

BOYLE: Right.

RUNNELLS: Now, if we could have Slide 10, those are the coupled effects on the mountain scale that we just looked at. Again, at the top, there are coupled effects on seepage.

BOYLE: Right.

RUNNELLS: Of the three, there are only--there are two that are blank in the right-hand column, and thermal hydrologic effects on seepage does appear on the right-hand column.

BOYLE: Right.

RUNNELLS: That's one in six of the couple effects that was carried into the supplemental analysis. Could you explain that?

BOYLE: Yeah, why one of six?

RUNNELLS: Right.

BOYLE: Yeah. I don't know off hand. Perhaps Bob Andrews or Rob Howard remembers why these were propagated but not the other ones.

RUNNELLS: I notice in particular the chemical effects are not propagated, and there are those who believe, you know, that chemical effects could be quite important in opening or closing fractures, and so on.

BOYLE: Right.

ANDREWS: This is Bob Andrews, BSC.

Going back to the first ones on the more regional--regional is probably a bad word--but large scale flow effects, mountain scale flow effects caused by coupled processes, it was determined in those analyses that the changes in the flow fields, which is what that's getting at, are fairly short lived and are fairly local to the drifts. So the need to consider the larger scale, if you will, changes in flow fields associated with the coupled processes, they were more, if you will, driven by the boundary conditions than they were by the thermal chemical, thermal mechanical effects, boundary conditions being infiltration, and that infiltration change with time.

When we come to the smaller scale effects of coupled processes, the focus was on those short-term transients, the first thousand years or so, and its effects on seepage, in particular, to get at an issue that was raised in the Rev 0 model where a very conservative assumption was made on incorporation of that thermal hydrologic effect, and I think Mark or somebody alluded to it this morning, of taking the percolation five meters above and applying that to a local seepage model. So we wanted to focus in on that one, because it did raise a lot of questions in the Rev 0 analyses and models.

And, quite frankly, those next two were very difficult. The actual coupled effects on seepage, both the thermal hydrochemical effects and the thermal hydromechanical effects, there are analyses that are being worked on right now that are going to go into Volume I, as Bill points out there, but they're in some ways more qualitative than quantitative. There's still considerable uncertainty associated with that, and that uncertainty is being described in somewhat more qualitative terms and its potential effects on seepage rather than in a full quantitative fashion that's incorporatable, if you will, into a performance assessment.

SAGÜÉS: Thank you, Dr. Andrews. Thank you, Dr. Boyle. We're going to have go on to the next subject here, which is Performance Assessment, the Natural System, and this is going to be a two-part presentation. The first one is going to be given by Dr. Saxon Sharpe, and she's going to address the question of what is the long-term climate model, and what it is based on. And then Jerry McNeish is going to take up that question and the following questions which are listed in our program.

Dr. Sharpe?

SHARPE: Okay, thank you.

Well, just before we convened for this afternoon, someone said to me, oh, you're going to talk about the weather. So that's what I'll be doing. The future climate model goes out to a million years in the future.

And what's in the report? Okay, first of all, it identifies four potential future climate states, the interglacial state, which is the modern state that we're in right now, glacial state, intermediate/monsoon state, and intermediate state. And these different climate states are listed in the back of the material on Pages 19 through 22 to give you the analysis of what each of these states involves.

Secondly, it estimates future climate timing and duration of the different climate states, and then it estimates the annual temperature and precipitation based on modern meteorological stations, which we call analog sites.

They are input into the performance assessment, and they utilize fundamental knowledge with little, if any, abstraction, and they're based upon three things that I'll be talking about in sequence. One is past climate states and their magnitude. Secondly, the Devil's Hole chronology and celestial mechanics. And, third, modern meteorological stations that represent past climate states.

First, I want to compare this with the previous AMR, which went from present to 10,000 years in the future. This was done by USGS, Rick Forester. And the difference between the two models, basically USGS says that we're in a modern climate state from present to about 600 years in the future, and I say that we're moving into the monsoon climate state. And I'll talk about these in a minute.

I want to jump down to the glacial climate state. USGS estimates 30,000 years in the future to 50,000 years in the future will be our first glacial state, and I come up with 38,000 to 49,000.

Now, this monsoon and intermediate climate state, it looks like there's a fair amount of difference here, but this is an artifact of how I included the monsoon climate states. If you look back at the paleo environmental record, which is the Owens Lake record from California that I'll be talking about a little bit later, there are bursts of monsoonal activity, and that's increased, summer precipitation, that come into the record maybe for 200 to 300 years. One of them lasted 2,000 years, but mostly these are very short climate intervals. And so the way I included them, so that it could be modelled, would be to include two 1,500 year monsoon climate states within this intermediate climate state, and I just broke it up as a conservative estimate. So that's what the difference is right here between these two, and hopefully I'll be able to convince you that it's not significant when you actually look at the infiltration model. And I think Jerry has a slide to talk about that.

But, essentially, the difference here is that the USGS says that we're going to have sometime in the next 30,000 years, 1,400 years of monsoon climate, and I say within the next 38,000 years, we're going to have about 2,000 years.

The assumptions and uncertainty and potential factors not considered and the timing methodology are part of your handouts, and these are at the backup section, and I just don't have time to go into those during the main part of my talk.

Both of the reports use local and regional paleo environmental records to determine climate states and the magnitude of those climate states. And the different paleo environmental datasets, we're really lucky in Southern Nevada, we have a number of long-term really good records. One is the Owens Lake record from California. That essentially records Sierra and snow pack, and what Owens Lake is is a proxy for Sierra and snow pack, which is a proxy for regional climate signal.

Vegetation and packrat middens, that's a robust dataset, and that essentially gives us a magnitude, and it also gives an anchor point in that we were able to anchor temperature and precipitation with the last glacial state from the vegetation and packrat middens. Death Valley has a number of lake shore levels, and those can be used, and then marsh deposits in the Las Vegas Valley are used to calibrate hydrology and also to look at temperature.

So, in terms of the different climate states, we've got increasing temperature on this axis, increasing precipitation up here. These are the glacial states. We came up with three different magnitude glacial states, and I'll be talking about those in a minute, and these are essentially determined from the ostracode record in the Owens Lake core, which goes back 800,000 years. So these are the two glacial states, intermediate state, which is kind of a catch-all state. This is the interglacial or modern state right here, and then the monsoon state up here.

Now, this graph also shows effective moisture in that. Effective moisture is a combination of temperature and precipitation where you have greater effective moisture here, where you have greater precipitation, and less temperature, and less effective moisture here, with greater temperature and less precipitation. So it's kind of a continuum.

Notice that in our modern climate, we are in a time of least effective moisture out of all of these climate states.

So you put these climate states into a sequence. This is the interglacial or modern state here, that's kind of an end member, this is the glacial state here, the other end member. We've kind of lumped the other climate states into a catch-all intermediate climate on both sides. So you've got interglacial, and it moves into an intermediate climate, and then a glacial, and then an intermediate climate.

For the modeling purposes, because the monsoon intervals are very, very short, I put those into this intermediate climate state, although there are also monsoons in this one. But, again, this is about, in the Owens Lake record, it's about 2 per cent of the time, so I figured about 3,000 years total monsoon, and put those in two 1,500 year lumps right here.

So the four climate states that I'll be talking about and that Jerry will be talking about would be the interglacial, and then this intermediate/monsoon state, glacial state, and then the intermediate state where no monsoons are modelled into this part.

This is a real brief over-simplification of the climate states, but in terms of the performance assessment, it's adequate and it represents the different climate states and changes.

Okay, the second dataset, this is the Devil's Hole chronology, and the celestial mechanics. I had to put everything on this graph. What we have here is time on the bottom, this is 500,000 years before present, and 250,000 years before present. The red line with the dots, this is the Devil's Hole record, this is oxygen isotope curve over here, essentially looking at groundwater recharge, and it signals the glacial and interglacial periods.

The orbital parameters are graphed on this side. This is eccentricity. It's on about a 100,000 year cycle, and four of these make up a long eccentricity cycle, which is a 400,000 year cycle. And precession is the blue line here. That's about 19,000 years to 23,000 year cycle.

And if you look at this long enough, you can see that there is a pattern with the double cold cycles and the orbital parameters, and essentially kind of the pattern, that's on the last page of your handout, and I don't have time to go into that, but kind of what you're looking at here, these are the interglacials, these peaks up here, and these are the glacial states down here. So we've got the interglacials happening. The I's are the initiation of the trend that goes toward a glacial period. So, essentially, these sort of the lines in between the two, those are all transitional climates.

I've put on the oxygen isotope stages. These are designated from deep sea cores. They're found in paleo environmental records worldwide, and these just designate the glacials.

So, essentially, we have the Devil's Hole chronology which defines the timing of climate change. It's an ironclad chronology, an excellent record. And then by comparing the Devil's Hole chronology with celestial mechanics, you can determine past correlation.

This is the second part of the Devil's Hole record because I couldn't fit it all on one graph, 250,000 years ago to present. Devil's Hole record ends about 60,000 years before present. We're anxiously awaiting the rest of the record when it gets published.

Again, glacial period here, glacial period here, interglacial up here. Essentially, where we have these initiations, the timing works very well with the precession parameter. It's within about 2,500 years from the time we move from an interglacial, moving toward a glacial period. So that's actually a pretty close correlation.

Essentially, what this analysis does is take the last eccentricity cycle, which is 400,000 years ago to present, and look at the Devil's Hole record and the orbital parameters, and find that correlation, and then that can be tested with the penultimate eccentricity cycle 800,000 years ago to 400,000 years ago, because the local and regional records in Southern Nevada are good and show that.

Essentially, this does seem to work. This timing methodology that's on the last page does work for the previous eccentricity cycle.

The third thing using modern meteorological stations as future climate analogs, and these were based on modern atmospheric circulation patterns, and then past atmospheric circulation patterns, geography and past and modern ostracode and diatom occurrence, and that's from the Owens Lake record. And the modern stations define the climate history, essentially temperature, precipitation and snow cover, for inputs to the performance assessment.

These are where the stations are located. I should say that for uncertainty, we came up with upper and lower bounds for each of these climate states. So the modern climate state would be the regional records around the Yucca Mountain area. The monsoon climate state, which is the next state up with greater effective moisture, the two southern stations here, because essentially you get Gulf monsoonal flow presently, and that's probably what happened in the past for these monsoon period, and so these would be the upper bounds for the monsoon state, and Yucca Mountain is the lower bound for the monsoon state, as it is also the interglacial climate.

For the intermediate climates, those transition climates, these sites in here for a lower bound, and then the

Spokane, Rosalia and St. John sites in Washington for an upper bound. And then the stations all up here, including these three, were used as the different upper and lower bounds for three different glacial states.

These are the estimated temperatures for each of these climate states. The temperature on the bottom here, increasing going up this way. Modern climate here is Yucca Mountain, and you can see that this is the lower bound for the monsoon state. Here's the upper bound. And, essentially, this graph is the same one as the one with the circles. Lower effective moisture here, greater effective moisture up here.

So, monsoon state, when you move into greater effective moisture, this is the intermediate climate states here, the three glacial states are this one, this one and this one in the dark blue. The three states that we came up with, we came up with a warm wet glacial state here, and these are the oxygen isotope stage analogs that we found in the past that we've projected into the future.

So this one is the warm wet, 8/10, this is the cold dry, and then this is the one with the most effective moisture, the 6/16.

The thing to take away from this graph is basically, all the values are wetter and cooler than modern values, except for this one right here, and the cooler wetter values were used in the infiltration model for a conservative estimate.

In terms of trying to validate this type of climate modeling, I took the Owens Lake record in this middle pie chart, and just added up the ostracode occurrence, because they're very sensitive indicators of hydrology, and Owens Lake is linked to climate so, therefore, they are a climate indicator at Owens Lake. 22 per cent of the time, glacial ostracodes were found; interglacial, 18 per cent; and then intermediate/monsoon ostracode, 60 per cent of the time. And this is actually both the intermediate/monsoon climate state that goes from interglacial to glacial, and it's the intermediate climate state that goes from glacial to interglacial. Monsoon is only about 1 per cent of the time in this time period.

So, comparing it to the past and the future, these percentages were based solely on the orbital parameter data, the little rosetta stone that's on the last page of the handout, and they compare fairly well with 24 per cent glacial; 19 per cent glacial; compared to 22 per cent; 12 per cent; 18 per cent; and 14 per cent; 64 per cent and 60 per cent and 67 per cent.

So the intermediate, it's intermediate/monsoon and intermediate climate state is by far the most common climate state in both past and future, and the interglacial or the modern climate state is the least common. That's this one up here. And it has the least effective moisture relative to the other climate states.

So, I think this is a pretty good approximation just in terms of total duration for the performance assessment.

This is my last slide. In terms of the summary, I wanted to just look at the glacial states, because these are the ones with the most effective moisture, and these are the ones that would potentially affect infiltration the most. This is the oxygen isotope stage analog, so this is the cold wet glacial, the warm wet glacial, and the cold dry glacial, and these estimates, I used modern at Yucca Mountain, 15.4 degrees, and 189 millimeters.

These are the estimated precip, both upper and lower bounds, and temperature. And in terms of departure from the average, with the cold wet, it looks like we've got 130 to 325 millimeter increase over modern. For warm wet, 240 to 350 millimeter increase over modern. And 55 to 130 millimeter increase over modern. And the temperatures were much colder, 11 to 15 degrees; 7 to 8; and 8 to 11, and these seem to be corroborated with other paleo environmental data from Southern Nevada.

So, that was the long-term climate model, how it was developed, and the results, and the following pages in your handout, the big charts, I won't put up here. But they tell you the timing and duration of the different climate states. So that's the climate sequencing that was used for the TSPA, and that's the subject of the next presentation.

SAGÜÉS: Thank you, Dr. Sharpe.

We are going to go--I guess that Dr. Nelson is very eager to ask a question.

NELSON: Nelson, Board.

Are there no unconformities in the geologic record that open up gaps that aren't explained by ostracode history? Are there any opportunities for uncertainty because of uncertainties in the geologic record, say at Owens or elsewhere?

SHARPE: Right. Just looking at the Owens record, there are gaps in it. There's also the timing is not really well worked out for the Owens record. But those gaps are filled in by other datasets, such as the midden record, pollen record, you know, just different datasets. So you're kind of compiling these together, but yes, there are gaps.

NELSON: Okay. Are those gaps explained geologically as to why the unconformity occurred?

SHARPE: In terms of the Owens Lake record, periods of very dry climate or saline lake, if you got a desiccation, it could deflate. There are periods where different playas and areas in Southern Nevada that had lakes deflated and that record is lost. There are also shorelines that have been lost. So, there are a lot of gaps in the record.

SAGÜÉS: In the interest of ensuring that Jerry McNeish has an opportunity of presenting his entire presentation, we're going to defer further questions until after Jerry finishes his presentation. There's plenty of time?

Okay, very good. So then we're going to entertain additional questions right now. Dr. Knopman?

KNOPMAN: Thank you, Alberto.

On Slide 3, you do a comparison between your scenarios versus the survey's. And let's just look at the last line for the glacial climate state, you explain you have this difference. Do you mean to suggest by this chart that there are effectively no error bounds on those intervals, so that there's truly statistical significant between, let's say, 50,000 and 49,000, or even the 30 to 38,000? Is there that much precision?

SHARPE: No, there's no way there's that much precision.

KNOPMAN: So what really are the bounds on those?

SHARPE: I tried to come up with estimates of error, and there's no really good way to do that. I was able to estimate errors within the entire glacial cycle, but in terms of looking at this, or if you, you know, look at the charts, you know, just I guess after Page 11, there's no way to come up with error estimates on that. That's why we used the upper and lower bounds and different climate states. Hopefully, that will be enough of a conservative estimate to give us a good indication of what infiltration will be in the future.

But, no, there's no way I'm going to say that we've got this starting, you know, the first glacial is 38,000. It's a best guess. Plus, there are so many other uncertainties in the climate system that there's no way we can predict that that's adding error on top of this error. So, yeah, it's a guess.

KNOPMAN: Okay. Well, it would seem to me this is particularly important potentially in the thermal regime of the repository operations, starting a couple hundred years from emplacement, to 2,100 years. And if you could either be in a monsoon climate state, I mean, there's pretty significant differences, and maybe actually it would help if you could just explain to me where we could be in terms of we could be in terms of we could potentially be in a glacial warm wet period?

SHARPE: Right.

KNOPMAN: In that thermal period, or that would come much later?

SHARPE: The next, you know, as best as I could tell, the next glacial period would be starting about 38,000 years ago, and it would be the warm wet.

KNOPMAN: 38,000 years?

SHARPE: I'm sorry, 38,000 years in the future.

KNOPMAN: Okay.

SHARPE: And it would be the warm wet.

KNOPMAN: Okay. But what would be ticking in potentially 600 years from now is which one here?

SHARPE: It would be either very close to modern or maybe a spurt of monsoonal activity, which would be increased under showers in the summer. I know Jerry has a slide that shows the infiltration, and really the monsoonal infiltration is really kind of just a little blip. The real big infiltration estimates are for the glacials.

KNOPMAN: I'll just wait for his presentation then. Thank you.

SAGÜÉS: Dr. Parizek?

PARIZEK: Parizek, Board.

Where in all of this is the fossil fuel story for a thousand years? I mean, it seems like you've gone with records which are paleo records to calibrate all of this. But then if we have global warming, as we think man is inducing global warming, is that in here, or is that not in here?

SHARPE: No, that is not in here.

PARIZEK: Shouldn't it be in here? Or would you know what to do with a thousands years? Would it be warmer and wetter, or warmer and drier?

SHARPE: That's really impossible to say. I mean, I'm not trying to cop out on this, or anything. The thinking previously was that if we have global warming, that will make everything warmer. And now there's some papers coming out, and there are some papers being written and papers in press that indicate that if we have global warming, that could actually kick us into an ice age sooner.

And the way that would work would be essentially you've got the heat transport that goes up to the poles. With global warming, you would get melting of the ice sheets, which would then give you greater depth in the shallow seas, and in the Pacific. With those shallow seas, there's more water to warm and you get greater vapor transport going up to the poles, and you've got to, to grow ice sheets, you've got to get that water up to the high latitudes to grow the ice sheets.

So theoretically, global warming could take us into an ice age, but the jury is still out on that. Essentially, computer models have not been able to generate ice ages. We don't know how the climate system works. We just don't know well enough to be able to predict that.

PARIZEK: It's in the context really of whether one should worry about a thousand years of models, you know, in terms of performance assessment, or not, assuming you'd put higher numbers in or not. You've sort of caught it by calling it monsoonal earlier, I guess; right?

SHARPE: Right.

PARIZEK: Well, it's in your monsoonal.

SHARPE: Yeah, it's in the monsoonal, and we will probably have, you know, the interglacial with these little intervals of monsoon, and they're probably going to be, you know, a couple hundred years. They're not going to be this huge 1,500 year chunk that I suggest could go in the model. But for modeling purposes, I thought that that was a conservative estimate, and so that's how I broke it up like that.

PARIZEK: Okay.

SAGÜÉS: Very good. We'll continue with the next presentation. Jerry is going to introduce also the rest of the sub-questions on the performance assessment, natural system issue at the end of his presentation.

MC NEISH: Yeah, my name is Jerry McNeish, and I'm one of those Barbarian Scots that Abe was talking about. And, actually, I wish, like those Lake guys, that we have 60,000 years to put out our documents. I mean, that would be great, publish our data.

I'm going to talk today as a follow-on to what Dr. Sharpe has already presented in terms of the technical basis for long-term climate, I'm going to talk about how that is implemented into the TSPA, how it's abstracted, and then try to answer the remaining climate questions that are on your agenda.

As an overview of my presentation, first I'll quickly go through the questions that were raised about the long-term climate, and then talk briefly about the nominal case climate model, and then some detail about the extended climate model. And then in terms of results, I'm going to be showing a case to show what the extended climate effect is on the nominal case and the peak dose.

And then these last three points, what the impacts are on the igneous intrusion, sensitivity studies, and then on multiple barriers and defense-in-depth analysis. Basically, I don't have any new results in that area. I'll talk briefly about that, but just to give you the punch line for those in advance, I don't have any new results there.

So the major questions basically are similar to-- Dr. Sharpe has given the technical basis. And then this is the area where I'm going to be presenting some results. What are the effects of the model when you don't incorporate a reduced neptunium solubility into the model along with the climate change. So, in effect, just look at the nominal case for the TSPA Rev 0 ICN I, and do an extended climate case on that particular result and see what happens, rather than incorporating also the neptunium solubility reduction, which is what was presented in the TSPA-SR document. And then the other two questions about what additional effects this climate has on the sensitivity and multiple barrier analysis.

Dr. Sharpe has given the durations and ranges for the various climate states, and ranges of time, and then also given us the precipitation ranges and temperature ranges for the various climate states.

And in TSPA, we've abstracted that information. I mean, she had several pages listing the different climate state changes, and we don't incorporate all of those basically for computational reasons. So we consolidate the overall number of climate changes in the TSPA.

And as she mentioned, we utilize a combined monsoon climate state and intermediate, rather than having intermediate, monsoon, intermediate, monsoon sequencing. And we've evaluated the effect of that previously, and found it makes sense and it's relatively conservative to go ahead and do that consolidation.

We don't have any variability in terms of when the climate states start, so we've basically used the numbers that Dr. Sharpe came up with. And as she mentioned, she provides a range of precipitation values and then we discretize that in the TSPA into low, medium and high case, and then sample off of those during the TSPA analyses.

Now, just to lock onto the TSPA model itself, you've probably seen this TSPA wheel before showing the major components in the TSPA model, starting with unsaturated zone flow, going through EBS environment, waste package, waste form, and on through the system to the biosphere. And this also shows the sub-models within each of those major components.

So what we're talking about is climate, and you'll see it's basically at the top of the system affecting UZ flow. So if we go up the UZ flow component to see, you know, what are the subcomponents in there and how are they influenced by the climate, you can see the climate here which provides us the precipitation and temperature, and then that is fed into the infiltration model to develop the infiltration maps that are used, the infiltration maps of the repository region. And that affects, obviously, the mountain scale flow calculations, thermal hydrology, and then ultimately seepage into the drifts.

The next slide shows just a review of the nominal case climate, and this is--you know, Dr. Sharpe has presented an update on this, but this is what is in TSPA Rev 0, ICN I. For the first 600 years, we model the interglacial or modern climate state, and then went into an intermediate/monsoon climate from 600 to 2,000. And then from 2,000 on, whether we were doing a 10,000 year simulation or on out to a million years, we assumed an intermediate climate. And this is the basis for what we're talking about now, how do we discretize beyond 10,000 years in terms of the overall climate state?

The extended climate model for each climate state provides a range of precipitation values, low, medium and high. Those are shown over here in this chart, with precipitation on this axis, and each of the different climate states identified here, the modern, the intermediate/monsoon climate state, intermediate, and then going into the three glacial climate states, which are identified by the glacial state.

And then that precipitation information is fed into the infiltration model to develop the overall infiltration maps. And, likewise, they have a low, medium and high case, with the averages shown here. Obviously, there's some spatial variability in those infiltration maps. I think it's primarily dependent on elevation. But this information is all based on the analog information that Dr. Sharpe presented earlier.

The next slide provides some additional information about the extended climate states, the four major types of climate states that Dr. Sharpe talked about. And in our TSPA model, we used a total of 45 different climate changes from 10,000 years out to a million years. And these charts show first the zero to 10,000 year infiltration averages for the three different climate states that we had in the 10,000 year model, and then the bottom chart shows from 10,000 out to a million years, all of the cycles in the climate.

An important thing to note is these spikes, basically where we have the glacial climate states, those are--you're going to see those on the dose results that I present a little bit later.

Another thing I wanted to mention is in the extended climate model, we didn't do any new thermal hydrology because you see the major--the first major climate state change after 10,000 years is this 38,000 year one, and that's basically after the thermal conditions have gone back to ambient.

Also, the seepage was altered for each of these different climate states, even though it wasn't thermally perturbed, it was altered based on the increased infiltration.

The next slide shows the map that Dr. Sharpe has already presented, but basically, the three main locations where we got the information for the new infiltration maps that were implemented in the TSPA model.

The ratio of infiltration to precipitation ranged from 6 per cent to 21 per cent for these new infiltration maps that we developed. Now, if you're thinking, you're going, well, they've got three new glacial climates, but you've only got four new infiltration maps. What's going on there? And this is TSPA abstraction at its best.

We looked at the overall ranges in those new glacial states, and we said okay, where do we already have existing maps that are comparable that we can use to reduce the overall work load, and so we've chosen, in some cases, we've chosen a map that we had in our earlier climate stages to fit in, maybe one of the low or medium cases for these new glacial states.

So along with the four new infiltration maps, there were four new unsaturated zone flow fields developed, and in terms of the saturated zone impact from these climate changes, the water table elevation was increased by 120 meters for the wetter future climates, which basically is all except for our very first climate state, that zero to 600 year time period.

And based on this increase in the water table, obviously there's a change in the hydraulic gradient in the saturated zone, and so we have some saturated zone flux multipliers that are based on the increase in the infiltration. This was developed utilizing the 3-D saturated zone model. It was based on matching up the 3-D saturated zone model with those new gradients.

So, basically, we've gotten new precipitation and temperature information, and that has literally flowed through to infiltration and updated unsaturated zone flow fields, and through the engineered barrier system seepage and on down to influence the saturated zone.

Now, getting at the second question that was asked about the long-term climate, this slide basically tries to answer the first part of it, which was what is the effect of this climate change on the dose, the nominal dose, and also on the peak dose for that particular case for Rev 0 ICN I. And on this, I guess in your handouts, many of you have a black and white handout, so you may not be able to see this, but the smoothest curve under here, the black curve on this slide, is the nominal case. In the TSPA-SR Rev 0 ICN I, we presented this blue curve, which is both the climate change, as well as the updated neptunium or actinide solubility information.

And the question was asked, well, what if you just take the nominal case and change the climate? And so that's shown here in the red, and you can see, you know, obviously the dose is a little bit more jagged, representing when you have a change in climate. And primarily, you see the effect of flushing out of the unsaturated zone as you get an immediate rise in the water table.

The mean peak dose increases by approximately a factor of two at about 250,000 years. And another thing that's important to note is that first climate change in here has little effect because that's a time when we still have basically just diffusive release out of the waste packages because they're mainly just cracked. There aren't large patches open in the waste packages. So we're not seeing a lot of advective release at that time, and so your additional infiltration doesn't increase the dose that much.

The next slide goes into a little more detail on the neptunium itself, just showing the mean dose rate for neptunium with time, and again, the smoother curve, the black curve, is the base case, TSPA Rev 0 ICN I, and then the red curve shows what happens if you throw in the long-term climate.

Again, the peak dose rate for the neptunium is increased by about the same rate as in the total dose case.

This lower plot shows the same thing, but here's the base case for neptunium, and then what was presented in the TSPA-SR Rev 0 ICN I, which incorporates the secondary phases, or the reduced actinide solubility, and that's the reddish curve. And then on top of that, go ahead and put in the extended climate model, and you get, you know, a similar jagged response.

So, you know, the top curve here is basically what we call a one off, where this one, this analysis was a two off, and so we're trying to clarify what happens with the one off.

The next slide begins my sequence of we haven't done that, we haven't done that yet. The second part of Question 2 was dealing with the igneous intrusion scenario. The analyses were conducted for 50,000 years in the TSPA Rev 0 ICN I, and as we've noted, our first climate change in this extended model is right at 40,000 years. But we were really focusing on 10,000 years, so we weren't too concerned about that climate change.

Another point to bring up is if we were to incorporate the climate change here, it's not expected to affect the combined dose, because at that point, the dose is really dominated by the nominal dose, not by the igneous.

And another point, I guess in your Amargosa meeting in January, you were shown some other igneous results, and these results are weighted by the probability of occurrence. They're not the conditional doses that you saw in your January meeting.

The next slide is regarding the sensitivity analyses and how does this extended model affect the sensitivity analyses. Again, we haven't conducted any additional analyses. As Dr. Boyle mentioned, we are in the process of creating a supplemental model which will have a lot of new information and be able to do the thermal evaluation.

And once that supplemental model is completed, then we intend to do some additional sensitivity analyses. But we don't expect the overall conclusions to change, you know, based on this climate model, just because we've already found out the importance of seepage and advective releases, and while we expect the doses will go up when those climate changes occur, the overall sensitivity of the parameters isn't expected to change.

The next slide shows the same sort of answer. We haven't done any additional analyses in this area, but we intend to once we get the supplemental model finalized. The incorporation of the extended climate into this type of analysis also is expected to obviously increase the dose. If we don't have a waste package and we throw more infiltration in there in a, you know, barrier analysis, then obviously we're going to get increased peak dose. But overall, we don't think it will change the relative importance of the barriers themselves.

So, in summary, the Rev 0, ICN 1 of the TSPA nominal case climate, it assumes constant climate beyond 10,000 years. It has three changes before 10,000 years, but beyond 10,000 years, it's constant.

The extended climate model that we developed has four different climate states, as Dr. Sharpe mentioned, the interglacial, which is the modern day climate; the intermediate/monsoon; the glacial; and then another intermediate, if you're going around the horn. And there are three types of glacial cycles, as mentioned already.

We implemented 45 different climate changes from 10,000 years out to a million years in the TSPA model that had a range of infiltration values, but the start time for each of those changes was fixed.

And incorporating that extended climate model into the TSPA Rev 0 ICN I gave us a factor of two increase in the peak dose at late time. And then, again, in terms of additional sensitivity analyses and multiple barrier analyses, we haven't done any at this time, but we intend to over the summer after we've completed the supplemental model.

I welcome any questions that you might have.

SAGÜÉS: Thank you very much. Any questions? Dr. Bullen?

BULLEN: Bullen, Board.

If you could go back to Slide 16 where you talk about the barrier analysis? I guess the question that I have is you noted in the third bullet that incorporation of the extended climate into the existing barrier importance analysis is expected to have increase in the peak dose, but not change the relative importance of the barriers. And I guess the question that I have deals with the fact that if you have the 120 meter rise in the water table in the 600 year time step, when that first climate change can occur, do the waste packages and drip shield know that the water is down there? Does the water know that the waste packages are there? And in your thermal analysis, do you have the mobilization of more water from the water table up, as opposed to percolation down?

MC NEISH: No, we don't. I mean, we talked about that earlier today. We haven't done that analysis where your thermal calculations see that water table rise.

BULLEN: Okay. I guess the concern that I have, or the question that I have is that at one point long ago, Tom Buschek was actually moving water up with the heat in his heat transfer calculations, and I wondered if those effects would actually be more prevalent or more applicable if you actually have the water table rise closer to the waste packages while they're in the thermal pulse phase. And I guess how will you address that, I guess is the question?

MC NEISH: Well, I hate to sign up for more work, but it sounds like an issue that we need to do an analysis on.

SAGÜÉS: At this point, I would like to recognize the presence of Dr. Ike Winograd of the United States Geological Survey, who was instrumental in establishing the early phases of the concept of Yucca Mountain as a potential repository. And perhaps Dr. Winograd would like to comment on these issues.

WINOGRAD: Ike Winograd, USGS.

The work just presented I had not seen until just now, and Saxon Sharpe faxed her work to me on Friday, which I got to look at on Monday after reading Dick Forester's AMR, and Jerry's work I've just seen.

I have a few comments. First off, this work is an order of magnitude superior to what appeared in the VA. In the VA, you recall, the climate states were interglacial, which with a step function, went up to glacial, which lasted 90 per cent of the time, which then, with a step function, went up to the superpluvial, and then back down, and everyone working in climate knows of course that in the average, we're in some state between these extremes. So this is a major step forward, and I commend all of you for this.

I think Debra Knopman was--said something I was going to say if called on, which is that on those tables, I would round everything to one significant figure as quickly as possible. Tomorrow afternoon, Saxon and I are going to get together and go over some details. The qualifications that can be made, and I think should be made to this new effort, which I commend you for, I think it's a step forward, but recognize that the field of paleo climate is, as we were talking at lunch, the half life of ideas in this field is a few years to a decade perhaps, and if you want to be convinced of this, look at an essay by Richard Kerr in the April 27th issue of Science, that's two issues ago, and showing how thinking flip flopped on the role of the tropics just in the last few years.

So, it's a very, very tricky thing, but this is a step forward. I'll stop there.

SAGÜÉS: Thank you, Dr. Winograd. Any other questions?

Debra Knopman?

KNOPMAN: Jerry, I'd like to talk a little bit more about the barrier importance analysis, because this is just a continual source of frustration for me. We see the dose curves, and because of the assumed performance of the waste package, we don't see anything, we get no insight for the first 10,000 years of what's going on in the natural system, what might go on in the system if the waste package wasn't there, or maybe, you know, could be in terms of juvenile failures, or whatever.

But have you done any of those runs that took the waste package barrier out so that you could see what would happen in the natural system with the water table rise, and the increased infiltration, if you then were having advective transport out of the repository? Do you know what the differences in dose would be?

MC NEISH: Offhand, I don't know the differences in dose. Maybe Bob does. But we have done those types of analyses in the repository safety strategy. There were a lot of neutralizations where the waste package was not included. We also, in our TSPA document, have some juvenile failure analyses. And as I understand it in the supplemental model, the waste package has early failure, there are some early failures, which will allow us to see the performance.

KNOPMAN: right. I'm asking, though, whether with the refinements in the climate models, whether you've done that analysis?

MC NEISH: Not yet, no.

SAGÜÉS: Okay, Dr. Reiter?

REITER: Jerry, this is Leon Reiter, Staff.

Jerry, reference to Debra Knopman's question, I want to make sure at least I understand something, is that you're going to do the sensitivity analysis. But are you going to include in the sensitivity analysis the effects of the steep hydraulic gradient? From what I understand, although the site recommendation design extends the repository over the steep hydraulic gradient, part of it, the analysis in TSPA did not take that into account, because I assume a little different configuration.

So, when looking at these things, are you going to take into account in the sensitivity analysis the effect of being over part of the steep hydraulic gradient?

MC NEISH: Well, we have to look at where our UZ information comes from. Frankly, I don't know how far north it goes. But if the UZ model that feeds us the flow fields gives us that information, then it will be incorporated.

Otherwise, they're going to have to update their information before we can incorporate it into the TSPA.

REITER: So, you don't know whether you're going to include this or not at this point?

HOWARD: This is Rob Howard, Bechtel Integration Manager.

For the total system calculations, for the dose calculations that Jerry's group does, the answer is no, we are not going to be able to analyze it for this round of analyses. We did not get that information for the larger model domain to Jerry's team early enough to incorporate it into the transport calculations. We do have a description of those implications, though, in Volume I, where in fact you would have, you know, obviously shorter transport distances in the UZ that could affect radionuclide transport out of the repository system. We have not analyzed, as you have noted several times today, the effects of thermal implications on the water table that would be, say, 60 to 70 meters away from the repository, as opposed to 160. Those analyses have not been done. They will not be available.

REITER: You said something will be in Volume I?

HOWARD: In Volume I, we will have a qualitative description of the implications where we actually have built the UZ model out to the north so that we can start to develop the flow fields for it, and we're looking at different repository sections to understand if we have a water table rise, where that water table may or may not intersect the repository horizon.

SAGÜÉS: Dr. Parizek?

PARIZEK: Parizek, Board.

Sort of a general question. On the 10,000 year, you use a constant climate in your summary slide. That's not shown really in figures on Page 12 and 14 which you run out to a million years; right? So I guess for the SR report, are we going to get something different than what we're seeing in those slides?

MC NEISH: I think that 12 and 13 have that, the nominal case includes that constant climate after 10,000 years. So, the black line has the constant climate in there.

PARIZEK: And then maybe a general question for the program, if you put these climate states in and take, say, the site scale groundwater model, or the regional groundwater model, will it explain the paleospring deposit occurrences? If you take this information from climate, throw that into the recharge story for the regional and site scale models, will we get a groundwater discharge point at the paleospring locations? Because, in a sense, it's like a model validation opportunity, and I can't answer that myself as to whether it did that or didn't do that to date, because at one point, we have a deep water table, we ran into a pluvial condition that when we needed to bring the water table up by 100 meters or better, the paleospring deposits suggest that we only need to bring it up a few meters, or tens of meters at most. Do you know whether the program intends to do that to try to calibrate and then validate those two models?

MC NEISH: I don't know. Is there a saturated zone guy here? I don't know.

PARIZEK: It could be a validation opportunity, is why I'm really raising the point here.

MC NEISH: Yes.

SAGÜÉS: One last quick question from Dr. Runnells.

RUNNELLS: Runnells, Board.

A question for Dr. Sharpe. Priscilla Nelson asked about the continuity of the record at Owens Valley, and you said there are unconformities, discontinuities. Are you able to fill those in with the packrat and midden record, or the Devil's Hole record? You mentioned that they're both very robust. So are you able to patch in the holes so you have what you consider to be a more or less continuous record? Or in looking at the overall picture, are there still gaps in trying to put this thing together?

SHARPE: The Owens Lake record doesn't have huge gaps in it. We're not talking about thousands and thousands of years. The dating is a little bit problematic. Essentially, a paper came out after the original paper on Owens Lake with different dates, changing the deposition and, you know, the rate of deposition.

In terms of unconformities in the record, and I think in terms of what Jerry has up here, I think it's minimal. I don't think that it's a significant problem. I mean, the Owens Lake record is really an excellent chronology, over 800,000 years, and any small unconformities I just don't feel are significant that we're missing with that.

A number of different things, proxy data were looked at in the Owens Lake record from the sediments to pollen to the ostracodes and diatoms, and that record is fairly robust. With the piecemealing, you can kind of put in the packrat middens. Those are kind of discrete instances in time, and you can look at those and compare those, also with the paleospring deposits, and you can just start kind of building this record.

So, really, I don't know if I gave the impression that there were these huge unconformities in the Owens Lake record, but, you know, there is just a timing problem, and I really don't think that's significant.

SAGÜÉS: Okay, thank you very much. We are now on break until 3:45 p.m.

(Whereupon, a brief recess was taken.)

SAGÜÉS: Welcome back to the rest of the afternoon session. We're going to have now presentations on performance assessment, the engineered system. And then we're going to have an introduction on the DOE waste package performance peer review, followed by a few additional comments on our issue, and then probably comments.

So, I'm going to go ahead and introduce Mr. Robert Howard and Dr. Robert MacKinnon, who are going to be talking about a series of questions that have to do, as I said, with the engineered system.

Mr. Howard is going to paraphrase his part of the questions at the beginning of the presentation, so there's no need for me to go through those at this time.

So, Mr. Howard?

HOWARD: Thank you.

Okay, good afternoon. I thought I had overcome the effects of my ongoing cold. My name is Rob Howard. I am the Integration Manager for Bechtel SAIC for the Science and Analysis Project, and I'm going to be talking to you about the first question related to the engineered barrier system on the agenda, which has to do with why is it that performance assessment typically analyzes the design condition as opposed to some as built condition. And as I talk to you about it, I'm also going to try to work in some of the progress that we've made in our updates to our models in this area.

Just to remind you, Dr. Boyle went over this chart, or a variation of this chart, earlier this afternoon, and what I'm going to be talking about has to do primarily with the waste package. I'm going to talk a little bit about early failure due to improper heat treatment and how that may play a role in these analyses, temperature dependent general corrosion rate, stress thresholds, fraction of surface-breaking flaws, and distribution of crack exponent. You're going to see the results of some of that work in these calculations that I'm going to show you.

So, generically, why doesn't performance assessment always consider possible differences between the EBS components as designed versus how they might be built at sometime in the future? Well, we assume that the repository is going to be constructed, operated and closed according to the design.

We assume that design is going to meet the applicable quality assurance requirements and quality controls. That includes those requirements for design control and inspection and testing, so that we can confirm that the as built condition does in fact conform to the design.

Any deviations from the design are going to be subject to regulatory review and reevaluation. Larry Trautner mentioned a little bit about this in his presentation earlier. We have things called operating specifications or technical specifications. In reactor power world, you have 5059 evaluations for changes, testing and experiments. We'll have similar regulations imposed on us if we find the site suitable and go for a license application, so that any of those changes in the design would have to be reevaluated.

We have requirements for a performance confirmation program to confirm the design parameters, and that performance confirmation program goes on during the operations, and if we were ever to build, to construct and operate this facility, prior to closure, we'd have to reassess the performance of the system in the as built condition as part of the requirements for closing the system, so that we understand those.

Any deviations in the design that are significant, in other words, if they change performance implications, have to be corrected. We document the generic rationale in our systems level FEPS analysis, our features, events and processes. We have a features, events and processes screening argument for improper or inadequate design, if you will. So that's generically how we address this issue.

Now, for the specific questions, I'll just kind of remind everyone that one of the problems with the waste package, or one of the threats to waste package performance is stress corrosion cracking. So if we have residual stresses in the waste package, we're going to be prone to that kind of failure mode.

Our current mitigation approach is to solution anneal and quench the as-fabricated waste package in the shop, and then after we load the waste package with fuel, we'll do a local induction annealing and laser peening to induce compressive stresses on the final closure welds.

Just to keep everybody oriented, we've got a 25 millimeter thick outer lid. We're proposing to induction anneal that weld. The 10 millimeter thick inner lid that's part of the design, we're planning to use laser peening as a process for that.

We do have a weld in the 316 nuclear grade steel, but we don't have any performance accredit for the 316 structural shell at this point.

Well, what if induction annealing or laser peening cannot be demonstrated at the commercial scale? That's kind of the heart of the question. We've got some options. We could use a single closure lid design, and I'll show you an analysis of what that looks like right now. You could use two lids with the same stress mitigation technique. So if we couldn't demonstrate laser peening, we could use two solution annealed lids might be an option, or we can develop one of these other low residual stress welding processes.

I've got two cases that I want to show you for a single lid design, in other words, what happens if we can't demonstrate either laser peening or induction annealing and we want to go on with fabrication anyway with one lid.

For the purposes of these analyses, I use thermal inputs that were from the higher end of the thermal operating mode just so that we would have a comparison for both cases. We used the updated temperature dependent general corrosion model for both cases. That was work that we were doing as part of our unquantified uncertainties, and work that's also necessary for the thermal evaluation. So having a temperature dependent general corrosion rate in our waste package model is very important for us for being able to tell the differences in thermal operating modes. And that's something I'm pretty excited about being able to actually show performance implications on that. We've got updated treatment of weld flaws.

Threshold stress uncertainties for stress corrosion crack initiation has been updated. That's work that was going on as part of our unquantified uncertainties. We've got new data in our stress corrosion crack growth exponent. That's our repassivation slope in our stress corrosion cracking model, and that was updated based on new scientific information. And an updated probability for improper heat treatment is being considered, which actually leads to early waste package failures, and that's included in this scenario.

Okay, what I have here is a set of curves generated from our waste package degradation model, and also I'm showing the 95th and the mean for both cases. So, this blue line here is the mean cumulative distribution function, or failure rate for a waste package design that only considers a laser peened lid.

The first failures are actually occurring in about 1,500 years. This does not include early failure mechanisms from improper heat treatment. And for the case where we only have an induction annealed lid, you get waste package failure, demonstrated by the red line here, and the fire failure is around 3,500 years. And that does not include the improper heat treatment as well.

And why it occurs different, in general keep in mind that we use, for the laser peened lid, we only assumed we still had a 10 millimeter lid. That's what we had stress profiles for, for these analyses, so that's what we used.

Both of these curves, quite interestingly, the dominant failure mode with all the updates to the models is general corrosion. It's not stress corrosion cracking. That's an important result of the updates to the new models. The general corrosion failure mode is the dominant failure mode in these analyses, although for the laser peened lid, the first breach was due to a stress corrosion crack.

I've got results, mean value results of both cases, and you actually have the hundred realization dose curves in the backup slides. But this black line is the TSPA Rev 0 ICN I base case. This first blue line is a waste package with only the 10 millimeter laser peened lid, and you're getting doses early here because of the thinness of the barrier itself. But you don't have the early breach due to improper heat treatment.

The reason why we don't have the early breach due to improper heat treatment in this case is that we believe that that failure mode is most likely going to be caused by the induction annealing process. So the induction annealing process, you might have the waste package closure lid being heated up beyond the thermal range, or cooled down, or quenched faster, and that's going to create some phase stability problems and aging problems in the waste package lid.

If we don't have an induction annealed lid, you don't have that problem. For this green line, that represents the case where we just have the induction annealed lid, and that does include the early failures. And then when you combine all these effects into the updated model, the dose results that you would get, and that includes the early failures, is shown in this red curve here. And that peaks about 2 millirem per year. The peak for the induction annealed lid only is about 87 millirem per year, and for the laser peened lid only is about 97 millirems per year, and they all occur in the out years.

Doses at early times, just like in the TSPA, base case are due primarily to technetium and iodine. At later times, these doses are due primarily to neptunium-237 and colloidally transported plutonium.

Now, Bob MacKinnon is going to present some information that may show how these results might change at earlier times with some sorption properties that he's included in the invert model.

Part B of the question was related to drip shield performance, and what if the drip shield doesn't perform as expected, and I'm going to show one of Dr. Knopman's favorite analyses, these barrier degradation analyses. There's also just for you a neutralization analyses in the backup slides. These were calculations that we did for TSPA-SR Rev 0 ICN I. Since they do have the waste package performance in them, as in the prior models, you don't see that much difference in the doses.

If we had included those early waste package failures due to improper heat treatment, these results may give us a little bit different insight. But we just did not do those calculations. I apologize for that.

Any questions?

SAGÜÉS: Debra?

KNOPMAN: On Slide 10, I assume these dose rates have not been weighted by their probability of occurrence, as you do with the igneous intrusion scenario; right?

HOWARD: That's correct. These are nominal.

KNOPMAN: Could you just--okay, they're nominal.

HOWARD: Yes, these are just nominal, so I don't include any igneous in there.

KNOPMAN: Right. Okay. But can you give us, give the Board some idea of what you would expect to be the probability of occurrence?

HOWARD: For the--

KNOPMAN: For each one of those different model runs.

HOWARD: Okay. Well, I can't do it for the hypothetical cases. With the one waste package design, that's just not part of our repertoire, if you will.

For the early waste package failures, the probability of occurrence that you see, and you can see it better in the hundred realization dose results that are in the backup slides, so you've got about a 77 per cent chance of zero waste packages failing by improper heat treatment. You've got about a 20 per cent chance of one waste package failing by improper heat treatment. And you've got about a 3 per cent chance of two waste packages failing by improper heat treatment. And when you see how it's implemented in the

TSPA for the hundred realizations, you'll see that those results are based on zero, one or two waste packages failing at early times, in early years.

So, does that kind of answer your question? I can't do it for the--

KNOPMAN: Yes. So, another way of saying this is that the probability of failure, at least through this mode, for greater than five packages is about as likely as having some kind of igneous intrusion.

HOWARD: I'd have to think about that.

COHON: Wait a minute. This is Cohon, Board.

Is this number of packages failed by a certain year or in a certain period?

HOWARD: Yes, this is at time zero. The number of waste packages failed at any given time is what you saw in the CDFs.

KNOPMAN: Which is quite low also.

HOWARD: Yes, ma'am, it is.

KNOPMAN: So it's different failure modes at different times. But at time zero--

COHON: I have to go back and take a first probability course. But the way I read this is that there's zero probability that six packages would have failed, since the probability is one in five that five do?

HOWARD: Now, I think you're reading it backwards.

There's some small, not non-zero probability, that more than five waste packages fail, but it's small.

COHON: In other words, it's less than .00001?

HOWARD: Right, by this early failure mechanism. We're not saying the waste packages don't fail.

COHON: No, no.

HOWARD: Okay. Any other questions?

SAGÜÉS: Yes. Can we go to Number 5, please? I just wanted to know, that sketch is more or less to scale, is that correct?

HOWARD: Yes, I believe it's more or less to scale.

SAGÜÉS: So the little lid against which the induction and annealed weld is made is about, say, one-eighth of an inch, between one-eighth of an inch and a quarter of an inch, or so; right?

HOWARD: Yes.

SAGÜÉS: Okay. And then there is that large cut between the section and the outer--

HOWARD: Right here?

SAGÜÉS: Yes. Underneath that, there is that--

HOWARD: Yes.

SAGÜÉS: Okay. I just wanted to make sure I got that understood. If you go to Figure 9, if I understand correctly, if you take out the outer lid, you have a first failure by general corrosion after about 1,500 years, you said?

HOWARD: Yes, actually, in this case, the first failure is by stress corrosion cracking. But the dominant failure mode here is general corrosion. And you can see that on the backup slides I have a slide, failure modes by--failure by first crack and failure by first patch, which is by general corrosion. And this tracks along the general corrosion profile, so it is dominated by general corrosion.

SAGÜÉS: And the other one in the red curve, that's 3,500 years, that is not by stress corrosion, that's by general corrosion?

HOWARD: That's failing by general corrosion.

SAGÜÉS: And is that a new estimate, like based on new estimates of generalized corrosion rate, or is it just as it was before?

HOWARD: Well, no, this implements the general corrosion rate that's temperature dependent. So, we've got a Arrhenius relationship to general corrosion rate. That's what it looks like. The bottom line is that general corrosion rates decrease by about three orders of magnitude as the temperature decreases from 125 to 60 degrees C. So it's not the same general corrosion rate that you have seen in the past. It's been modified to incorporate temperature effects.

SAGÜÉS: And the rationale for that Arrhenius relationship comes from what dataset? From the laboratory?

HOWARD: Yes, it comes from the data that's being generated out at University of Virginia. I think the activation energy was 66 kilojoules per mole, or something like that.

SAGÜÉS: I see. Okay. On the other hand, the current data indicated a much less severe temperature dependence, was it?

HOWARD: I'm sorry?

SAGÜÉS: The data from the corrosion test coupons at Lawrence Livermore, that kind of data suggested a much lower activation energy?

HOWARD: Yes, it did. This is somewhat conservative. I guess I probably need to get Jerry Gordon out here to help me better quantify the difference between those.

SAGÜÉS: Because in this particular case, we seem to have a strong temperature dependence.

HOWARD: Yes, we do.

SAGÜÉS: But in the other data, such as the other temperature dependence, was a lot less than that, in which case it wouldn't be conservative. It would be the other way around, presumably.

HOWARD: I don't know if it would be less conservative or not than having no temperature dependence. I'd have to think about that. Jerry, can you answer that?

GORDON: Jerry Gordon, Yucca Mountain Project.

The data are from potentiostatic tests rather than weight loss. So it's a much steeper dependency.

SAGÜÉS: And which data was chosen for the overall-- which type of evidence is going to be used for the final calculations, this one here, or the one that came from the tests?

GORDON: Well, the University of Virginia tests were done in unbuffered sodium chloride. So they're very conservative. We're currently generating data in more relevant environments, and we'll use them as soon as we get the data.

SAGÜÉS: Okay. So this will be like provisional estimates; is that right?

GORDON: That's correct.

HOWARD: This is the function that's going to be used in the SSPA analysis. This will be what we use. And when we do the evaluation for the high temperature operating mode and the low temperature operating mode, we're going to use this function.

SAGÜÉS: Okay. On the other hand, you can measure corrosion rate to 95 degrees centigrade, and then you measure it at the lower temperature based on the corrosion coupon tests. And you observe a relatively small temperate dependence. Then if you get the high temperature rate and you try to find out what the low temperate rate is going to be, then you don't gain a lot by going to a low temperature. But with this kind of an estimate, you gain a lot by going to a lower temperature; right? So that's what I was saying, that that is not necessarily conservative. That would be the other way around. Is that right or am I wrong?

GORDON: It's a much steeper slope.

SAGÜÉS: Right.

GORDON: Potentiostatic data.

SAGÜÉS: Okay. But what I was trying to say is that that's not necessarily a more conservative estimate. It just simply--a lot faster as you cool down, and that could give you a more optimistic estimate if you're trying to use high temperature data to extrapolate to low temperate behavior; right?

GORDON: It could, yes.

SAGÜÉS: Okay. I'll have to look at that then. Thank you.

DI BELLA: Carl Di Bella, Board Staff.

Could you put up Slide 10 again? The blue curve, for example, that's for no outer lid whatsoever. And the question the Board asked was what happens if these treatment techniques don't work? Well, what would happen is that you would get failure, at least in the early time, by stress corrosion cracking.

Now, could you explain how you get from a stress corrosion cracking type of failure to no outer lid whatsoever, or are you simply just presenting that as a bounding case?

HOWARD: Yeah, I'm simply presenting that as a, I won't say bounding case, but a case to answer the question what if we can't demonstrate one or the other of these techniques commercially, and we only went to one lid design. These failures here are not stress corrosion cracking failures at early times. These are early failures due to improper heat treatment.

Did that help, Carl?

DI BELLA: Thank you.

HOWARD: I wasn't trying to make any grandiose claims about the analysis.

SAGÜÉS: We have one more question from Leon Reiter.

REITER: Two short questions. I just want to make sure I understand that. The reason for reduced peak dose in the blue and the green curves is because you just distributed the releases over time?

HOWARD: Yes, that is primarily what it is saying.

REITER: Okay.

HOWARD: You don't have, if you recall in the TSPA Rev 0 base case, all of the waste packages failed by one mode or another somewhere between 100,000 and a million years, and you don't have that for this case.

REITER: Okay. The second question is do these curves and the curve after on the drip shield take into account the new climate scenario?

HOWARD: No, sir, they do not. And once we get these models incorporated into the updated analyses, along with what Jerry showed you earlier, we'll run the total system model with a high temperature and a low temperature case with the climate scenarios and these updates.

REITER: Because you might get--at this point then, the peak, the glacial peak that occurs at 38 to 40,000 years might have a real effect on this.

HOWARD: Yes, it might. I mean, that's part of the exciting part of this analysis.

REITER: I think it's a good example of what Dr. Cohon was talking about before about interactions. If you look at something just by itself, it's hard to determine what the impact is. It's sometimes necessary to look at a bunch of different factors.

HOWARD: Yes, he's absolutely right, and I'd also say that Dr. Boyle was right, too, that unfortunately some of our analysts, you know, like myself, are so close to it that sometimes we second guess ourselves too fast, and we don't get to those answers.

No questions from Bullen, Board?

SAGÜÉS: We're going to have one more questions from Dr.

Craig, and then we're going to have to go to the presentation.

CRAIG: Craig, Board.

I'd like to understand that blue curve somewhat better. You know, we've had a long interest in juvenile failures and what happens, and this isn't quite a juvenile failure, but it's something pretty close. And if you look at that blue curve, you see that on the time span well below 10,000 years, you're getting up to doses that look like they're violating whatever standard happens to be set. I don't know whether it's 10, 15, 20 or 30, or even 50 MR per year, but anyway, it's up there to the place where it's violating standards. And if that's what happens, that's very interesting. That seems to be the situation where you leave off a lid, and some how or another, the material comes out, and you're going to tell us how it comes out, and this is presumably using your distribution that you showed, so it's one or two, probably one or two failures.

HOWARD: Just one.

CRAIG: Okay. You understand my question and the confusion?

HOWARD: Yes.

CRAIG: So I'll leave it with you to answer then.

HOWARD: Yes, the one or two failures that you're looking at are this curve here. So for the early failures at time zero--

CRAIG: Well, how many cans have no outer lid in the blue curve?

HOWARD: All of them.

CRAIG: All of them? Okay.

HOWARD: Yes.

CRAIG: So that's a lot.

HOWARD: Yes, that's about 11,000, 12,000. That's not something that we would do.

CRAIG: Yeah, that would probably be noticed.

HOWARD: Right. Yes, we might somehow figure out a way not to put the lids on a couple of them, but I don't think we would--

SAGÜÉS: We're going to have to proceed with Dr. MacKinnon's presentation and question groups two and three.

MACKINNON: Good afternoon. I'm Robert MacKinnon. I'm the EBS Department Manager on the project.

Before I begin, I want to clarify one item, though. On the agenda, it indicates that I've been promoted to a Lawrence Livermore National Laboratory Staff Member. However, that's not true. I'm still a member of Sandia National Laboratory.

I guess if we stick to the schedule, I have eight questions that I need to address in five minutes. That's going to be difficult, so what I'm going to do is briefly state the question, provide a brief direct answer, and then provide some high level basis for that answer.

The questions are separated into two groups. The first group deals with issues related to local environmental effects, and the performance of the drip shield. The second group of questions relate to FEPs, their dependence on the thermal operating conditions, and the postclosure evolution of the engineered barrier system.

This slide simply shows a cross-linking between the questions and the various topics that are addressed in the supplemental analyses that Bill Boyle talked about earlier this afternoon.

Well, the first question asks to what extent does TSPA account for local environmental effects when we have a stand-alone or continuous drip shield? The answer to that question is that we do account for local thermal effects, mainly radiation and conduction. We do account for variable waste package spacing. We do not distinguish between a stand-alone or coupled drip shield configuration.

Out of our multi-scale TH model, we developed approximately 6,000 unique thermal hydrologic environmental conditions, or approximately 6,000 waste packages. That information is abstracted and used in the corrosion modeling and in the EBS models. Our local processes use average thermal hydrologic conditions in our calculations.

We presently make the assumption that the gaseous phase conditions in the air gap between the waste package and the drip shield are well mixed with the drift environment. In other words, we treat those environments in the same way. We currently have some work ongoing to further strengthen the technical basis for that assumption.

There's also one source of variability and uncertainty in our multi-scale calculations that we do not account for, and that is the axial movement of gas in the drift due to thermal gradients, in other words, natural convection.

This slide is a conceptualization of natural convection. This slide shows a hot package adjacent to a cool package, and because of thermal gradients in the direction from the hot package to the cool package, we get axial flow. This hot air flows in the direction of the cool package and descends along the drift wall, and returns along the invert to the hot package, and the loop is completed.

A similar loop takes place in the gap between the waste package and the drip shield.

I'm going to briefly show some analyses that we initiated in March, three dimensional, thermal Navier Stokes calculations to quantify natural convection and do pretest predictions for natural convection experiments that are planned to begin later this year.

This is an idealized calculation. It simply shows two waste packages, a hot package next to a cool package. The temperature of the hot package is approximately 80 degrees C. The cool package is approximately 60 degrees C.

Now, the orientation has been reversed on this slide. This is the hot package over here, and the cool package. They're timed 300 years, we've solved for a steady state flow field inside the drift, and we've released a tracer at the end of the package. So the tracer is following the flow path up towards the cooler region near the crown of the drift, and it's beginning to turn over and move towards the package like in the conceptualization that I showed you previously.

The tracer is above the cooler package and descends down along the drift wall, and then returns and is caught in this convective flow path.

Now, one thing I want to point out here is that we're talking about relatively short times on this quarter scale drift test. Here is a shot at 1,000 seconds. So we're talking about reasonably high velocities on the order of a tenth to a quarter of a meter per second. What this slide shows is that in a thousand seconds for that steady state flow field at 300 years, we have almost complete mixing.

In our thermal hydrologic models, we do make the assumption that we get complete mixing. That is one of the reasons we do not include axial flow in our multi-scale models.

Well, this question asks what is the potential for significant temperature differences between adjacent waste packages and drip shields, i.e. cold traps? The potential for significant temperature differences is high, and the potential for having cold traps is also relatively high. However, we, based on our analyses, we have concluded that cold traps themselves will not significantly impact performance. Cold traps can impact performance in two ways. One, it can put water on the package.

We account for water on the package by introducing dust on all packages at the time of postclosure. This is a hydroscopic dust, sodium nitrate. The deliquescence point is rather low, and when that critical LH, corresponding to the deliquescence point is reached, that is when corrosion is initiated. We think we've bounded that process.

The other effect that may occur due to cold traps is enhanced advector flow into failed packages. And based on our FEPs analyses, we have concluded that the magnitude of extra dripping is expected to be small.

However, I do want to point out that our analyses, there are uncertainties in those analyses. We still feel that our conclusion will remain as it is, but we do need to further investigate the issue of natural convection and cold trap effects, and we currently have an NRC KTI agreement to do so, and we have initiated those analyses in March. And like I said, we have a convection, core scale convection test initiated this year also.

We plan to use the information that we learn from the modeling and the experiments to help us interpret the observations that have been made at the ECRB.

What is the potential for formation of thin or thick films on the surface of the waste package? The potential is quite high. We will get films forming on the surface of the waste package. We also will get films forming on the inside of the waste package. We think we've bounded the effects of films forming on the exterior of the package.

In our supplemental analyses, we've looked at packages that are failed due to stress corrosion cracking. We allow water to enter those packages through the gas phase and sorb onto the interior components of the waste package. In this water film, radionuclides are allowed to diffuse from the source and be released from the waste package. In the TSPA-SR analysis, we assume that the radionuclides were released from the source and were right at the waste package wall instantaneously.

Now, we implemented this in-package diffusion model only in CS&F waste packages, and it shows that the impact is not real significant. It's main impact is that it delays doses by about 2,000 years. This is the base case TSPA-SR. This is the case where the impacts diffusion model is implemented in CS&F packages only.

What is the potential for dripping to occur under the drip shield? Our analyses show that there is potential for condensate to form. In our FEPs analyses, we have screened this process out, and we believe that the contribution of condensate to mobilization of radionuclides is not significant. We did recently implement a condensation model in the TSPA model, and I'll show you those results here next.

This slide simply shows that for the base case, there is no impact on dose, and this is primarily because the waste packages fail late and the evaporation rates are relatively low at these times. But in the backup slides, I believe it's Slide 39, that we present results where we have presented juvenile failure results for the condensation model, and the impact on doses is not significant.

Now, we considered a total of 88 FEPs in our EBS analyses. Several of these FEPs are concerned with postclosure drip shield performance. This question is do current drip shield models adequately characterize and bound drip shield performance? Yes, we believe they do, based on our current understanding and our current models.

Now, again, I want to emphasize that our FEPs analyses, we need to strengthen the technical basis for several of these FEPs, and we're in the process of doing so.

This slide shows the specific FEPs that were evaluated for postclosure drip shield performance, and they include thermal expansion in the drip shield, floor heave, rock fall, seismic response, and emplacement pallet failure.

Now, again, we're further strengthening the arguments in these analyses, but the process we've used is the FEPs analysis process. At the subsystem level, a decision has been made whether or not to carry that model forward through the total system performance assessment calculations. These processes have been screened out in the current analyses. And when these analyses were done, the attempt was made to bound various processes. As I said, we recognize there are uncertainties in these analyses, and we're continuing to evaluate them.

Our drip shield flux model and waste package flux model, the models that we use to calculate seepage penetration through the drip shield and waste package, are highly conservative in the TSPA-SR model. We essentially make the assumption that all of the seepage that enters the drift falls on the crown of the drip shield.

We also make the basic assumption that all of the corrosion patches that form on a drip shield line right up on the crown of the drip shield, whether they're on the right side of the drip shield or on the left side of the drip shield.

We also ignore seepage evaporation. So what we've done is we've reduced the conservatism in this drip shield and waste package flux model by assuming that all the seepage falls on the upper surface of the drip shield. We do make the assumption that any seepage that penetrates the drip shield will contact the waste package, but it contacts the upper surface of the waste package. And we do take credit now for seepage evaporation at the drip shield.

These are results for comparing the base case calculation with a case where we've removed all of the drip shields in the repository. We're taking credit for seepage evaporation, and we've neutralized the waste packages. Every waste package has a failure patch in the waste package, and this was so that we could examine. Obviously, if we look at the effects after 10,000 years, evaporation, seepage is not that significant.

So we see that for the case where we've neutralized both the drip shield and the waste packages, we do get some impact on dose. Peak dose in 10,000 years is reduced by a factor of approximately two.

Now, this shows you the effect of the new drip shield and waste package flux models. They're still relatively conservative, but they do reduce our doses in

100,000 years. This is the effect shown here compared to the base case, and then the base case with the new models implemented.

If the potential repository were operating in a cooler thermal mode, which FEPs previously screened out would be included, and vice versa? Well, to really answer this question, we would have to do some analyses, but I can give you my best educated guess. We considered 23 near-field EBS and Waste Package FEPs that are directly related to thermal conditions. In other words, if we were operating in ambient thermal conditions, 22 of those FEPs wouldn't even have to be considered. The only reason we'd have to consider one of them is that it has a combination of thermal and non-thermal processes in it.

Nine of these 23 FEPs are excluded from TSPA-SR. We feel that even at the low thermal operating mode, none of the nine excluded FEPs would need to be included. If anything, it goes the other way.

However, and this statement I think is maybe a little strong, in that none of the 14 included FEPs would be excluded for lower thermal operating mode conditions. And I can think of one case, and the reason I want to qualify this statement is that one of the FEPs is condensation in the regions around the drift, and thermal reflux. In TSPA-SR, we account for thermal reflux. But in the case with the lower temperature operating mode, we will likely not form a condensate around the drift, so we will not have thermal reflux. So we could possibly exclude that FEP. However, we would not do that. We would go ahead and implement our models. It would just turn out that we would get zero thermal reflux.

This question deals with if the structural steel corrodes and the drip shield may misalign, the waste package may fall off the pedestal and roll over and touch the drip shield, how does this impact performance?

We currently in the TSPA-SR model, we set the waste package right on the floor. So we don't take credit for the pallet.

We do have two FEPs that have been considered; thermal stresses due to differential thermal expansion in the waste package. In other words, we've looked at uneven temperatures on the surface of the waste package, and how does that impact thermal expansion. We have concluded that the effects are not significant.

We've also got a FEP that looks at material interfaces, and in particular, if the waste package is adjacent to the drip shield. Our conclusion there is also that the effect is not significant.

Now, again, there are uncertainties in this analysis, but all of these analyses are documented in our

FEPs process, so they are there for anyone to evaluate. But we have gone through an orderly process to decide on which processes we carry on in the postclosure period.

Have the corrosion products of the EBS and materials, such as ground support, been considered in postclosure EBS performance? Again, we've relied on FEPs analyses to exclude a couple of processes. Degradation of cementitious materials in the drift. Our conclusion is that right now, we do have grouted rock bolts, that the grout will be sufficiently carbonated that the seepage that contacts the grout, it will not experience large increases, or significant increases in pH. Interactions with corrosion products have been also screened out, primarily because most of the ground support system in that will be gone in the first thousand years.

In-drift sorption. We have screened that out simply by saying that we're conservative by not including it. Well, we have recently developed a model to include sorption and the invert. We have over 20,000 kilograms of potential corrosion products per waste package. That's a substantial amount of corrosion products.

This shows you what the effect of considering the corrosion products and sorption in the invert, and you can see that we've got a substantial delay in time of arrival. The peak doses will not change, but the arrival times will certainly be delayed.

That's the last set of results I have to present, and I'll end the presentation there.

SAGÜÉS: Thank you very much. Dr. Nelson?

NELSON: Nelson, Board. I didn't think I was going to be the first one, though. I'll try not to be too obscure.

I have a continuing question that really has to do with heat transfer and moisture and how all the different ways of heat transfer are modelled, and whether there's a model that includes moisture and all possibilities for heat transfer, considering the rock and ventilation, be it natural or forced, and whether there's a model that takes into account the moisture mass balance and the energy balance in trying to understand what goes on during the thermal pulse, and the continuation of natural ventilation in a drift.

And I'm thinking that I'm not prepared really to believe that condensation is not a concern, because I don't think we're really sure what's going on in the ECRB. So I'm really not sure. So can you tell me that you've got a model that you're actually very confident of can do all of the different kinds of heat transfer and consider energy and mass balance in a coherent manner to predict when and where condensation will occur?

MACKINNON: I'll try to answer that question.

First of all, condensation is a concern, no doubt about it. We do think, though, that its impact on performance will not be significant, and we plan on, we've got the quarter scale drift test, conduction test, we've initiated these three dimensional thermal Navier Stokes analyses, and the ECRB observations. We're going to use this information to get a better understanding of the effects of condensation, and hopefully to validate our conclusion.

Now, our multi-scale thermal hydrologic model definitely does an energy balance and mass balance. It does not include axial flow or natural convection. Our assumption there is that the environment in the drift has substantial mixing in it that the gas phase conditions, moisture concentrations and temperatures are relatively uniform.

These experiments and the analyses that we have ongoing we hope will confirm that.

Forced ventilation, our model does not include forced ventilation. What we do is we simply we have a FEPs that addresses this specific issue. We remove the thermal energy from the system that would be removed by ventilation in our power input to the thermal hydrologic model.

Now, during ventilation, a substantial amount of moisture would be removed. We do not account for that moisture removal. In fact, we keep the system wet, and we think that tends to be on the conservative side, and we're relatively certain about that, but we need to provide a better technical basis for our treatment of the effects of forced ventilation.

Does that answer your question?

NELSON: I'm not absolutely sure. It seems like it's being parsed out into certain parts and then pieced back together, and I'm wondering if there is a coherent overall code that might be envisioned that would do more than what the various pieces of the question that you're dealing with. My experience when working with geology and geotechnical is that it's the little local things that will almost always be the surprises that will develop local conditions to be not what you thought on the average. So I'm just not comforted by this discussion here that there's been a way of really trying to capture whether those kinds of things can be important, particularly regarding condensation.

MACKINNON: I guess maybe I should summarize it like this. We are doing different analyses to look at various issues, natural convection and condensation. These processes are not in our current model. If indeed we determine through our analyses and our experiments that these processes are important, i.e. significant to performance, we're going to have to account for them in some way in our thermal hydrologic model.

NELSON: Okay.

SAGÜÉS: We are behind unfortunately. We have two quick questions by two Board members who are known by being brief. One of them is Dr. Bullen.

BULLEN: Bullen, Board.

Could we go to Slide 17 first? This is your adaptation of what the heave and collapse of the invert might look like and how a drip shield might actually be degraded. And if you go to Slide 18, which is the next one, you talk about mechanisms that are responsible for thermal expansion, floor heave, rock fall, seismic response, and emplacement pallet failure. I guess what I don't see is the degradation of the invert there. With the corrosion of the carbon steel that's down there, wouldn't you expect that in a few hundred years, that's going to be gone, and that that would be the primary mechanism for floor heave? And yet these have all been screened out because of minor structural response to the performance of the drip shield, but really I'm not worried about the drip shield performance. I'm worried about the drip shield acting as a focusing agent on the waste package.

So have you analyze the impact of these kinds of responses to the performance of the waste package, or is that something that you haven't done?

MACKINNON: These processes address these mechanisms and their effect on the drip shield movement. It has been concluded that in our rock fall analyses, floor heave analyses, anything that would contribute, any processes that would contributed to deformation of the drip shield, or movement of the drip shield, those processes are not significant enough to impact the performance of the drip shield, primarily because the way the drip shield is designed, the drip shield, the overlapping drip shields, they have 600 millimeters of overlap.

In addition to that, there is a lip on the top of the drip shield that's 5 centimeters high. That lip will prevent any axial flow. The overlap is, based on our calculations, is long enough to prevent separation for any kind of movement.

BULLEN: Bullen, Board.

A follow-on question to that. How many drip shields do you have to emplace, and since you're doing it remotely, what's the guarantee that they're all going to be done perfectly? And what are the probabilities for error in drip shield emplacement, and that impact on failure? I mean, you've got a whole bunch of drip shields to put in.

MACKINNON: Well, I'm going to have to defer to Rob Howard's presentation, which basically concluded that we will ensure that drip shields are emplaced according to design requirements.

BULLEN: Okay. So there is no human error probability that's built into that? I mean, I'm working remotely from the surface, emplacing this thing remotely, watching a camera that may or may not be working as well as I might like 125 years from now when I'm closing this thing. I guess I just wondered about the human error scenario, and whether or not you've evaluated that before you take a look at it.

MACKINNON: We haven't evaluated it.

BULLEN: Okay. I have to stop asking questions now.

SAGÜÉS: Yes, we have to stop. Thank you very much.

I am very pleased right now to introduce Professor Joe Payer. Joe will describe briefly the Department of Energy's new materials peer review, which he chairs. And Joe has his BS and BSD from Ohio State, which is one of the best known centers for materials science and corrosion research in the United States and the world. He's been in the materials science and engineering department of Case Western Reserve University for 16 years, and served as department chair for several years. And many of you will remember that Joe was the materials science representative on the panel that formed the peer review of the TSPA-BA. So, Joe, go ahead, please.

PAYER: Thank you, Alberto.

We can just go right to the next slide here. This Peer Panel Review is just underway. The organization and the beginning of getting us up under contract and going started in the March/April time frame. We are a peer panel to look at waste package materials performance. We were put in place by DOE's request to Bechtel SAIC, and our report recommendations will go to DOE.

The overall objectives of what we're going to do are to review the current bases for predicting long-term performance. We're interested in both the high nickel alloy 22 and also titanium alloy represented by Grade 7, and we will be looking at the ongoing and experimental plan as well as the performance information.

The intent is, and the goal is, to increase confidence in long-term performance projections by conducting this exercise.

There will be two reporting periods, an interim report in the September of this year time frame, and a final report scheduled. I've got a little more detail later on the scheduling, but that will be in the February of 2002 time frame.

There are five sub-issues or sub-topics within that overall materials performance, waste package performance. One is the assessment of the potential degradation modes. And this will be a review and an analysis of the types of degradation modes that could impair the waste packages, look at that issue one more time to see if all of the potential show stoppers have been considered.

More specifically, we will be looking at the long-term performance of passive materials. In several of the presentations today, the importance of that has come through loud and clear. These alloys, both the high nickel alloy and the titanium, depend upon a passive film for their corrosion resistance. If that passive film remains stable, then 10,000 year lives are clearly believable and credible.

The question is what happens to that passive film over those long time periods, however. And so we'll be looking at that specifically.

Giving credit where credit is due, one of the Board members, particularly chairing this session, has been really the banner carrier on this particular issue, and much of this response is to look at that very important question.

If you ask any corrosion materials scientist person how does something corrode, what's the corrosion rate, their very first response will be in what? What's it exposed to? And so you can't really conduct this issue without looking at the composition of the waters in contact with the waste package surface. So, again, that will be a topic that we will be looking at.

The two most likely failure modes, corrosion failure modes, that have to be dealt with for these materials are crevice corrosion and pitting, which occurs in localized areas, and stress corrosion cracking. And one of the charges to this peer panel, and one of the things we'll be looking at and commenting on is what is the state of our understanding of these processes, and what is the control of these processes.

We will be doing this under the standard peer review criteria, which are listed here. The important point is the focus of this peer panel is on reviewing the understanding and the technical basis for long-term performance.

We will also be reviewing the experimental plan and how that fits into performance assessment. But what is the understanding and technical basis at the process level, the chemistry, the mechanics, the materials science that's underway.

This is a list of the peer panel. Myself. Dr. John Beavers is a vice-president at CC Technologies, a contract research organization that does primarily corrosion research. Tom Devine is Department Chair of Materials Science at University of California, Berkeley, has a long experience in passive film structure and composition. Gerald Frankel is a professor and director of the Fontana Corrosion Center at Ohio State University, an international expert in the area of crevice corrosion and localized corrosion. Russ Jones at Batelle-Northwest Laboratories again is internationally recognized for his expertise and research and performance in environmental cracking, physical metallurgy, things of that sort. Rob Kelly is a professor at the University of Virginia and, again, recognized for his expertise in localized corrosion and monitoring. Ron Latanision is a professor of materials science and engineering, with also an appointment in nuclear engineering at MIT. Ron is the director of the Uleg Laboratory. And I can report to you that I'm delighted that we were able to get these kinds of people willing to serve and apply on this committee.

This is a committee that does represent corrosion science, and so forth, very well. And you can see the different areas that are represented by this group.

In addition to that seven person peer panel, we will have a group of what are called subject matter experts. These are people that are going to look at more specific areas for us, prepare some written input, also ask commenters and dialogue, and they will represent both U.S. and international interests and perspectives in the area of passivity, localized corrosion, geochemistry, hydrogeology, physical metallurgy, and so forth, some of the very important issues that impact on our study.

The meetings and interactions, we will have an open meeting, a series of open meetings. The introductory meeting will be held in Las Vegas on May 23rd. We will have an interim report where we present the interim findings of our study in the September time frame, again in Las Vegas, and there will be a final report meeting. All three of those meetings will be open meetings. They will be primarily set to present the findings to the DOE and BSC.

This introductory meeting will be primarily introduction of the panel and project presentations to the panel, but then the rest will be for us to present our results out.

We will be meeting as sub-groups, working groups with the subject matter experts and peer panels. Those will typically be, or will be closed working sessions. We will be interacting with the project people and other people working on the program. The reports will be delivered to DOE.

February and April was an organization, putting the panel in place. May, the major event will be our introductory meeting May 23rd. We'll be conducting our analysis throughout the summer. September 10th is the tentative date for our reporting of the interim results, and that's, not by accident, planned to be in conjunction with your Board meeting September 11th and 12th in Las Vegas. Many of us have to travel, so we thought it would be nice if we could have one trip rather than several.

We'll complete the peer review. There will be a final report in the February time frame, and then the contractor does the evaluation of our peer review subsequent to that.

Thank you. I just wanted to let you know a little bit about how we're structured and how we're organized. This is in response to the DOE and the project's recognition and your recognition that performance of the waste packages is a critical issue here. The corrosion of these packages, can you say with confidence that a waste package at Yucca Mountain might last 10,000 years. That's what we plan to address.

Thank you.

SAGÜÉS: Thank you very much, Joe. We have time for a couple of questions here from the Board. Dr. Craig?

CRAIG: Joe, as you know, this Board makes a very big deal out of open meetings. Could you explain why you've elected to run your meetings, the technical substance of the meetings as closed rather than open meetings?

PAYER: Primarily logistics, Paul. I think we're going to be going around as sub-groups. I don't imagine our full group will get together as a full group any time other than those three meetings we called out here. Again, it's not by desire; it's just the logistics of busy people and major schedules. So two or three of us will be going to Livermore to sit and talk to the folks about composition of water on the waste package service. Three or four of us might be meeting in Columbus to talk about stress corrosion cracking. That's the reason. It's strictly reality of the logistics.

One of the things I did not mention, I was remiss to mention that, we do plan, however, to generate, both the subject matter experts and panel members, brief write-ups, call them white papers or critical reviews or whatever. We intend to post those on the web. It's not going to be a public website, but if you say, you know, you're technically interested in this area and you want to look at that and comment on that, then you're welcome to join us. That's the way we're going to try to get some of the openness and a wider dialogue of this.

SAGÜÉS: Thank you very much, Joe.

I'm going to make a brief announcement on a corrosion related activity that the Board will conduct before turning the meeting over to Dr. Cohon. And this is a planned international workshop, long-term extrapolation of passive behavior.

For some time now, the Board has emphasized the importance of issues related to predicting long-term waste package performance. This continues to this day. In fact, progress in understanding the underlying fundamental processes involved in predicting the rate of waste package corrosion is one of the areas that Dr. Cohon enumerated this morning. Those areas are, in the Board's opinion, should be essential parts of any such recommendation.

In the past two to three years, the Board's concern related to predicting waste package performance has focused on two areas. First, the resistance of Alloy-22, which is the material selected for the waste packages, to well established amounts of corrosion. Second, once that resistance has been established, the more difficult issue is extrapolation and performance over extremely long times.

The Board's concern boils down to this. The exposed surface of Alloy-22 is reactive. Alloy-22 derives its remarkable corrosion resistance from a tenacious, virtually impervious, but very thin layer of compounds on itself called the passive layer.

Now, humankind has essentially 100 years of experience with metals protected by such passive layers. Alloy-22 itself has been commercially available for only about 20 years, and yet based on this brief experience, we are now extrapolating the performance of the waste package for tens of thousands or hundreds of thousands of years into the future.

Now, because we believe this issues are so important, some members of the Board have discussed holding a workshop that would focus on long-term passive layer integrity, and on challenging experts to identify possible mechanisms affecting it.

Now, we're aware of the DOE's peer review that Joe Payer just presented, and we wanted to wait to make sure that our efforts would not duplicate those of the panel. As it turns out for various practical reasons, it appears that DOE's peer review panel will be quite formal, as has just been shown.

Also, the agenda for the peer review panel is quite broad, as Joe just showed. Consequently, an informal Board workshop with a very focused agenda, should complement the efforts of the DOE peer review panel pretty well.

What we're planning is essentially a round table meeting, and that would be almost like a brainstorming session. Since our workshop will be confined to the narrow topic of long-term passive layer integrity, it will be just a day or a day and a half in length. It will start with a presentation or two to give everyone a common basis of knowledge, continue with brief presentations by participants, and then be followed by a round table discussion of questions furnished before the meeting.

We plan to invite a total of about a dozen, maybe 15 experts, from around the world in fields like corrosion or electrochemistry to participate. We fully expect that some of the DOE peer review members or their subject matter experts also would participate. Naturally, it will be an open meeting, and the Board will share the results of our workshop with the DOE peer review panel.

Right now, we are thinking about Thursday, July the 19th, and maybe the next day, at a location to be determined.

However, at this point, we haven't yet invited any candidates. So, the date or the venue may change. We will keep everyone posted on our progress in finalizing plans for the workshop, and the details will be posted on our website as soon as they are identified or developed.

COHON: Thank you very much, Alberto, and thank you for your duty as chair. And our thanks to all the speakers.

That concludes the scheduled portion of our meeting. We'll turn now to the public comment period. We have with us Douglas Schneider, who's from Representative Shelley Berkley's office. Representative Berkley is from the State of Nevada. And he has a statement he wants to read.

Mr. Schneider?

SCHNEIDER: I would like to thank the U.S. Nuclear Waste Technical Review Board for allowing me the opportunity to address the Department of Energy's proposal to store high-level nuclear waste at Yucca Mountain in Nevada. This issue is critical to me because my district is located 90 miles southeast of Yucca Mountain, and it is my constituents who would be the most affected by the Yucca Mountain Plan.

In 1983, President Reagan signed into law the nuclear Waste Policy Act. The new law began with a reasonable scientific approach. The country would search all over the nation looking for geological formations which were capable of containing the radioactivity of high-level nuclear waste. The new law would also consider three sites to provide regional equity to the burden of storing the waste. One site would be in the northeastern part of the country, one site would be in the southeastern United States, and one site would be in the west. These three sites would be studied, and then presented to the President of the United States for a decision.

Since then, politics has had more to say about the siting of the high-level nuclear waste repository than science. After members of Congress from the northeast opposed placing the dump in the northeast, the Department of Energy unilaterally decided to take them off the list. When placing the dump in the southeastern part of the country came up as a campaign issue in the 1984 Presidential elections, President Reagan unilaterally decided to take them off the list.

Then in 1987, the so-called "Screw Nevada" bill was passed into law. This bill made the most political of decisions, the designation of one site, Yucca Mountain, as the only site, excluding any other region in the country from consideration. Thus began the erosion of credibility of the so-called scientific findings of suitability of Yucca Mountain.

More than a decade has gone by since the 1987 amendments to the Nuclear Waste Policy Act, and the scientific evidence against Yucca Mountain continues to grow. Yucca Mountain is located in an earthquake and volcanic eruption zone. As recently as last month, there was so much moisture at the proposed site that electrical test equipment was shorted out. It is widely known that ground water will corrode the waste storage containers, and release the deadly toxins into the environment.

Scientific evidence against the proposed Yucca Mountain site is plentiful, but just like the "Screw Nevada" bill, each time legitimate arguments are raised, standards for Yucca Mountain are changed. Regarding the current situation with groundwater and personal radiation dose standards, the goalposts have again been moved. The Environmental Protection Agency set a groundwater standard of no greater than 4 millirems, and a personal radiation dose standard of 15 millirems per year at 18 kilometers, for the first 10,000 years of waste disposal. Despite the fact that the personal dose radiation standards are significantly weaker than similar sites around the country, the Nuclear Regulatory Commission has still asked the EPA to rewrite these standards to allow an even higher dose of radiation. The NRC knows full well that without reduced standards, Yucca Mountain can never be found suitable. So again, the rules must change.

On three separate occasions, the State of Nevada has demonstrated, using DOE's own data, that the site should be disqualified under both the EPA standard and DOE's own internal site screening regulation. And each time, the DOE or Congress has changed regulations to ensure that Yucca Mountain would not be disqualified, regardless of the health and safety consequences to Nevadans.

In fact, the DOE has found the geology at Yucca Mountain so poorly serves the need of a repository, that over 95 per cent of the waste isolation capability would have to be provided by metal waste containers, and other so-called engineered barriers around the waste. When this project started, the idea was to find a site capable of containing the radiation entirely through its natural geologic features. That standard has since been lowered from 100 per cent to 5 per cent.

Aside from the earthquakes and the potential for volcanic eruption, an aquifer flows beneath the mountain, with water moving so rapidly that even with all engineered barriers, radiation will unavoidably escape the repository and contaminate our water table. This fact is underscored by the U.S. Geological Survey report entitled "Flooding in the Amargosa River drainage basin," February 23rd and 24th, 1998, Southern Nevada and Eastern California, including the Nevada Test Site.

This document, which I would like to include with my statement, details two floods; one in 1995 and one in 1998 that would have had severe repercussions on the proposed repository. Most notable is the conclusion that both the 1995 and 1998 floods indicate that the Amargosa River, the contributing stream flow from one or more among Beattie, Forty Mile or Topopah Washes has the potential to transport dissolved and particulate material well beyond the boundary of the Nevada Test Site and the Yucca Mountain area during periods of moderate to severe stream flow. Yet, once again, in clear English, scientific evidence condemns the Yucca Mountain plan.

In addition to the mounting scientific evidence against Yucca Mountain, there are also ongoing General Accounting Office investigations into mismanagement by senior staff, and a review of the Inspector General's report on bias at the DOE.

The first issue was brought to my attention by an anonymous letter I received at my office from an individual who appears to be highly knowledgeable about the Yucca Mountain Nuclear Waste Site Characterization Project. The letter reflects a high level of expertise and first hand knowledge. It is alarming to say the least. Among the allegations are the lack of oversight in relation to the continually escalating lifetime costs for storing nuclear waste at the mountain, unnecessary travel abroad by senior level managers, lack of expertise and technical background of those in charge of the project, and an adversarial relationship between managers of the project and this very body, the Nuclear Waste Technical Review Board. The General Accounting Office is still in the process of investigating these very serious charges.

As for the second issue, as you are likely aware by now, the Inspector General has found that there were several statements in the Draft Overview and a note which was attached to one version of the Overview, that "could be viewed as suggesting a premature conclusion regarding the suitability of Yucca Mountain." Of particular concern to me is the section of the IG's report that states, "Based on correspondence received by the Office of the Inspector General, it is fair to observe that, at least in some quarters, public confidence in the DOE evaluation of Yucca Mountain has eroded." The IG also noted disincentives at DOE for Yucca Mountain employees to question assumptions, or to, in any way, "rock the boat."

The Inspector General's report serves to underscore what Nevadans have been saying since the origin of the "Screw Nevada" bill. Politics plays the leading role in determining the fate of the Yucca Mountain project.

It is pointless to discuss how we can restore the public confidence into this doomed project. The American public has seen behind the curtain, and we cannot erase from our memory what we have seen, a tainted process, driven by politics, with questionable scientific merit. The further we investigate Yucca Mountain, the more money we spend, the more obvious it becomes that Yucca Mountain is not the answer.

I again request that federal agencies change their course, and stop trying to fit a square peg in a round hole. Instead of trying to change the rules to keep this proposed plan alive, they should immediately begin the decommissioning of the Yucca Mountain Project.

Thank you very much.

COHON: Do you have the attachment for us?

Thank you, Mr. Schneider. No one else signed up for public comment, but does anybody care to comment at this time? Is that Judy's hand I see? This is Judy Treichel. Please come on up.

TREICHEL: This is very short. My entire speech is on one post-it.

What I want to request is that at the very beginning of this meeting, you addressed two questions, and one was whether or not you believed that it was the right time for a site recommendation to be made, or something like that. And you didn't say yes and you didn't say no, but I would ask that you would consider no, because the last presentation that was given was about the peer review that will not be done until early next year regarding the metal that's so important for the disposal casks. And it seems to me that that peer review should be finished because of the importance of that disposal cask.

We also, as I spoke before, we don't have any rules, and I've gotten several reasons for that since I made my first public comment, but none of them actually tell me when we're actually definitely in the site recommendation phase. So until that can be taken care of and there are some rules, I would think that would justify you answering no.

And just to point out sort of problems that the public has where we're trying to do it all and we don't have specialists that we send to each meeting, the very important meeting on May 23rd, which Dr. Payer talked about was the beginning of this very important peer review, is also the day that the NRC will be here to tell the public in Southern Nevada how licensing works. I think that's premature, but they want to do that. So that, once again, causes us to have a real problem.

And I would ask that that peer review panel also check into other work that's going on. I know that the State has made quite an investment of time and effort and money into also looking at this metal, and I would think that would be a good thing to include in what they're looking at.

Thank you.

COHON: Thank you, Judy.

Does anybody else care to comment?

(No response.)

COHON: Seeing no hands, our thanks again to our public commenters, to all of our speakers today, and to our two colleagues who chaired.

We stand adjourned until tomorrow at 8 o'clock in this room. Thank you very much.

(Whereupon, the meeting was adjourned, to be reconvened at 8:00 a.m. on May 9, 2001.)