UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

JOINT MEETING OF PERFORMANCE ASSESSMENT AND REPOSITORY PANELS

SUPPLEMENTAL SCIENCE AND PERFORMANCE ANALYSES (SSPA)

June 20, 2001

Crowne Plaza Hotel
4255 South Paradise Road
Las Vegas, Nevada 89109

NWTRB BOARD MEMBERS PRESENT

Dr. Daniel B. Bullen, Meeting Chair
Dr. Alberto A. Sagüés
Dr. Norman Christensen
Dr. Richard R. Parizek
Dr. Jeffrey J. Wong

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Linda Hiatt, Management Analyst
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I N D E X

PAGE NO.

P R O C E E D I N G S

1:00 p.m.

BULLEN: Good afternoon.

My name is Daniel Bullen. I'm a member of the U.S. Nuclear Waste Technical Review Board, and I'm going to serve as Chairman of today's session, because I serve as Chairman of the Board's Panels on Performance Assessment and Repository. It is, indeed, my pleasure to welcome you to this joint meeting of those two panels.

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

In those same amendments in 1987, Congress created the Nuclear Waste Technical Review Board, this Board, as an independent federal agency for reviewing the technical and scientific validity of OCRWM's activities. The Board is required to periodically furnish its findings, as well as its conclusions and recommendations, to Congress and to the Secretary of Energy. We do this in Congressional testimony and through our reports. As you may know, we issued our summary report for last year's activities about two months ago. Copies are available at the tables in the rear. And I also know that you can get copies on our web site if you don't want to carry a copy of it home with you.

As specified in the 1987 Act, the President appoints our Board from a list of nominees submitted by the National Academy of Sciences. The Act requires that the Board be a multi-disciplinary group with areas of expertise covering different aspects of nuclear waste management. We meet as a full Board three or four times a year, usually somewhere in Nevada. Today, however, we are not meeting as a full Board, but rather as a joint meeting of two of the Board's specialized Panels. These are the Panel on Performance Assessment, which focuses on methods of qualifying repository performance, that is, its ability to contain and isolate radioactive waste; and the Panel on the Repository, which focuses on the engineered aspects of the repository.

I want to introduce you to those members of the

Board who are present today, and in doing so, let me remind you that all Board members serve on the Board in a part-time capacity. In my case, I am associate professor of Mechanical Engineering at Iowa State, and I was formerly the coordinator for the Nuclear Engineering Program and a former director of the Nuclear Reactor Laboratory there. My areas of expertise include nuclear waste management, performance assessment modelling, and materials science.

Dr. Norman Christensen--Norm, would you please raise your hand--is about to step down as Dean of the Nicholas School of the Environment at Duke University. He tells me he has three working days left--not that he's counting. He served with distinction in this position for ten years. He will now start a very well deserved sabbatical this summer. His areas of expertise include biology and ecology.

Dr. Paul Craig is not here today. He will join us tomorrow for the last two sessions of the meeting. Paul is thee professor emeritus from the University of California at Davis. He is a physicist by training, and has a special expertise in energy policy issues related to global environmental change.

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

Alberto Sagüés. Dr. 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.

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

Many of you know and have worked with our staff, a number of whom are seated at the side of the room behind the Board members. Bill Barnard--Bill, would you raise your hand? Bill is not here? Bill stepped out. Bill is our Executive Director. Unlike members who serve part-time, the staff serves in a full-time capacity. Here's Bill.

Now I need to add our usual disclaimer so that everybody is clear on the conduct of our meeting, and specifically on what you're going to hear and its significance. Our meetings are spontaneous by design. Those of you who have attended our meetings before, and I know many of you have, know that the members of the Board do not hesitate to speak their minds. 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 are speaking on behalf of themselves. I would like to remind you that this is not a meeting of the full Board. Our Chairman, Dr. Jerry Cohon, and actually five other members are not present today. We view this meeting as an information-gathering meeting. Any Board position that may develop would only be taken after the full Board has had a chance to process all the information. And I understand that we're going to receive a great deal of information in the next day and a half.

The subject of today's meeting is the Department Energy's Supplemental Science and Performance Analyses, or SSPA. This document, which we understand will be issued this summer, contains recent scientific and engineering studies and performance analyses, not reported in previous DOE documents related. All of these analyses are related to the possible repository at Yucca Mountain. This meeting will be divided into three sections. The first section will last until about 3:00 p.m. today, and in it, the DOE and its contractors will address the purpose of the SSPA, describe its scope and content, and summarize the overall results of the report.

The second section, which will start this afternoon after the 3 o'clock break, and continue tomorrow morning until about 11:00 a.m., will address efforts of different technical areas to quantify previously unquantified uncertainties, to incorporate new scientific data and models, and to provide a basis for a comparison of different conceptual models of repository design.

After that, in the third session, we will hear the results of sensitivity tests and performance assessments, which take all of the new information into account. The DOE will also indicate how the SSPA helps address four priority areas identified by the Board as important for a potential site recommendation. And I'd like to remind you of what those four important priority areas are for the Board.

First. Meaningful quantification of conservatisms and uncertainties in the DOE's performance assessments.

Second. Progress in understanding the underlying fundamental processes involved in predicting the rate of waste package corrosion.

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

And, finally, development of multiple lines of evidence to support the safety case for the proposed repository. These lines of evidence should be derived independently of performance assessment and, thus, not be subject to the limitations of performance assessment.

I will chair this afternoon's session. Board Member Alberto Sagüés will chair tomorrow morning's session, and Board Member Norman Christensen will chair tomorrow afternoon's session.

Now, let me say a few things about the opportunities we've provided for public comment and interactions during the meetings. This is something extremely important to the Board. We will try to give the public as many opportunities as possible to comment during our meetings. Today and tomorrow' public comment periods will take place at the end of the presentations. Those wishing to comment should sign the Public Comment Register at the check-in table where Linda Hiatt and Linda Coultry are sitting. Linda, do you want to raise your hands back there? That's where we'd like you to sign in to make public comment. They'll be glad to help you sign in and then ask you to be ready to publicly comment with the time arises.

Now, I have to point out, and I'll remind you again later, that depending on the number of people who sign up, we may have to set a time limit on individual remarks.

As an additional opportunity for questions and continuing something that we've tried successfully before, you can submit written questions to either Linda Hiatt or Linda Coultry during the meeting. If there is time, the Chair of the meeting will ask the question during that meeting. If we don't have time to ask the question during the meeting, we'll raise some of the questions during the public comment period.

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

Now, as Chairman of this afternoon's session, we will see presentations introducing the SSPA. Our first presentations will be made by Steve Brocoum and Bill Boyle of the DOE's Yucca Mountain Site Characterization Office, and they will introduce the SSPA and tell us how it fits into the site recommendation process.

Following that presentation, Rob Howard of Bechtel SAIC will tell us about the Volume 1 of SSPA, the Scientific Basis and Analyses, and Peter Swift of Sandia National Laboratories will tell us about Volume 2 of the SSPA, which are the Performance Analyses.

We will then hear about SSPA work in two technical areas. Bo Bodvarsson of Lawrence Berkley National Laboratory will talk about unsaturated zone and near-field environment coupled process components, and Bob MacKinnon of Sandia National Laboratories will talk about engineered barrier system coupled process components. Following that, we will have the first of two public comment periods.

That brings me to the end of my prepared remarks, and I'd like to actually take the extra time and jump right into the meeting. So our first presentation is by Dr. Steve Brocoum. Dr. Brocoum is the Assistant Manager for the Office of Licensing and Regulatory Compliance and Yucca Mountain, and he will provide us with an update on the interface of SSPA in the site recommendation process. Steve?

BROCOUM: I want to give some introductory comments on the potential site recommendation document structure, how the SSPA fits in, and then Bill is going to get into what the purpose of the SSPA is, and information about that. So we're both speaking at this presentation.

I'm going to do this first bullet here, and potential site recommendation document structure, and Bill is going to do all of these things here.

These are the same diagrams. You know, again, just for information, this is all our documents that we have in our program that we've done in site characterization over the last 15 years. You know, down here is all the science and technology and engineering documents. These documents here are kind of summary documents of all the details down here. They include such things as System Description Documents, the Site Description, Repository Safety Strategy, and so on, a whole bunch of stuff, TSPA and the Supplemental Science and Performance Analyses.

The documents in the black box are the comprehensive statement that will be used by the Secretary, the comprehensive list of information used to make his decision on site recommendation. And so the next viewgraph just takes this box and makes it a little more clear.

We will have the Yucca Mountain Science and Engineering Report, which we issued earlier in the year. We will have the final Environmental Impact Statement. We have asked, we have sent the letter to the NRC and asked them to submit their Sufficiency Comments to us by October 1st. We will, based on any hearings we have on the site recommendation process, we will have a Comment Summary Document. We will have a Site Suitability Evaluation. Prior to that, we will issue a preliminary Site Suitability Evaluation for the public comment process, any other information that the Secretary deems appropriate, including TSLCC and Fee Adequacy Report. And if the State of Nevada submits an Impact Report impacting the site, we have to include that also. So all of these things are required. That's why we have these little bullets here, where in the

Nuclear Waste Policy Act they come from, and the comprehensive statement for the site recommendation.

So that is what is in this box here. If the Secretary decides to recommend the site, he will issue some kind of a document, you know, to the President with all of this either attached or referenced. That's kind of how we see the document structure flowing together.

This tries to show you how the SSPA, the Supplemental Science and Performance Assessment, Volume 15, allows other information. Basically, the SSPA is an extension or an addition or a supplement to the TSPA. It has more or less the same stature in the technical hierarchy as the TSPA. That's why over here, we show the SSPA in the supporting documentation, and we have the TSPA there also.

So, these two volumes are more or less the same technical level within the program.

The TSPA-SR supported the Yucca Mountain Science and Engineering Report, and that's one of the documents, and that's one of the documents up here in the comprehensive statement. And the PSSE, the Preliminary Site Suitability Evaluation, and the Site Suitability Evaluation, also which will evaluate our site against our reg. proposed, today's proposed regulation--we hope to finalize it. Obviously, we have to finalize it for site recommendation. So this is the PSSE, which I think is right there, Site Suitability

Evaluation.

So the TSPA supported both the Yucca Mountain Science and Engineering Report and PSSE. The Supplementary Science and Performance Analyses supports the PSSE. The PSSE is going to evaluate the site over a range of temperatures. So that's kind of how they fit.

The Yucca Mountain Science and Engineering Report supports the specific requirements from the Act, or the repository description, the waste form and packaging, data important to safety, and so on. These are all listed in the Act. The PSSE supports the evaluation against our proposed 10 CFR 963 requirements. So that's kind of how the documents fit together.

The technical documentation, this information down here, includes our TSPA, our Analysis and Modeling Reports and Process Model Reports, our System Description Documents, our Draft Environmental Impact Statement and the Supplement, the Preliminary Site Suitability Evaluation, the Yucca Mountain Site Description, the Preliminary Pre-closure Safety Assessment, and the Supplemental Science and Performance Analyses Report, which is still not complete.

One of the things, and Bill will go into more detail as to what the SSPA does, but I want to make one point here. One of the things that the SSPA evaluates is it evaluates the effects of the thermal operating modes on system performance. In DOE selecting, eventual section of a thermal operating model, we will consider other issues that are not in the SSPA. We will consider design parameters. We will consider preclosure safety, consider economic costs, timeframes for construction, operation, and so on. All these things will have to be considered by the Department before it selects an operating mode.

In the letter to the Board on May 30th from Lake Barrett, we have promised that we will produce an integrated evaluation and comparison of the options prior to the SR decision. That will be the status of where we are at that time. It won't contain any new information, but it will pull together all the information we have produced in a way that will be more understandable to the Board and to the public.

So, that was my introductory statement as to how the documents fit together, and my intent now was to turn it over to Bill, who will now focus on the SSPA.

BULLEN: Steve, we'll just hold questions for you at the end.

BROCOUM: Okay.

BULLEN: And wait until both presentations are done.

Bill, do you want to just go ahead and hop right in, or however you want to do this?

BOYLE: Yes. And if it's okay with your technical people over there, I'd just as soon make the presentation from here, if that's all right.

BULLEN: However you want to do it, that's fine.

BOYLE: Okay. All right, thank you. And thanks for this opportunity.

As Steve has mentioned, I'll focus in on the SSPA, in particular, out of all the things in that pyramid. And the Supplemental Science and Performance Analyses, the purpose was to document new results, and these five bullets come out of the technical work plan for the document. There they are. Rearranging them, if you will, as I talk about it, we have been continuing to do work, so we had some new science and we've had an ongoing effort to quantify uncertainties and conservatism. And we also were specifically looking at the effects of coupled processes over a range of thermal operating modes, and a lot of that material is documented in Volume 1. And in many of the sections of Volume 1, we also summarized multiple lines of evidence to back up this new science, or quantifying the uncertainties.

And then the new data, the new science, the different models, were eventually translated up into TSPA itself for system and sub-system sensitivity analyses, which in turn shed light on the quantification of uncertainties and conservatisms. So the purpose of the report was to capture all these new science models and a new TSPA.

So, the document exists as two separate volumes, Volumes 1 and 2. I always find it easier to talk about Volume 2 first, and it's updated TSPA, a supplemental TSPA model, a higher thermal operating mode TSPA and a low temperature operating mode, full TSPAs for both operating modes, and accompany sensitivity analyses. And those new TSPA calculations are premised upon the information that's in Volume 1.

Now, those tic marks, the three tic marks in Volume 1 correspond to the columns in the table that were presented at the May 8th and 9th Board meeting as to the motivating factors for the new information in Volume 1. And the two tic marks under Volume 2 correspond to the right-most two columns in that table, whether sensitivity analyses were done or whether the new information got into the supplemental model.

Professor Bullen mentioned the Board's four priority areas. Now, the work, which are listed here, these were copied from the Board's website and pasted in here. I think meaningful quantification of conservatisms and uncertainties; progress in understanding corrosion; an evaluation of the base-case repository design in comparison to a low temperature design; and development of the multiple lines of evidence.

And as I've already mentioned, the SSPA has work that relates to these four priority areas, but the document itself is not structured around these four areas explicitly. Like you won't find a chapter that deals, other than with the case of the waste package, that deals with each of these bullets. The document was structured instead, the SSPA is structured like many of our other technical documents, what happens to a drop of water as it moves through the Yucca Mountain system. So it's laid out by typical technical chapters, as you'll see in the following talks during the next two days.

Now, one of the handouts that was made available over there--well, there were two handouts made available. I've already referenced the table, and I'll come back to it. This was available over there. But there was also another document called Roadmap to Draft SSPA Volume 1, Rev. 00E and Volume 2, Rev. 00B. And you can use the table to try and figure out what parts of the SSPA dealt with the Board's four priority areas of concern. But rather than make people do that themselves, that's what the roadmap does. People have looked--the roadmap has the same technical content as the table. It's just been rearranged around the four priority areas of concern.

So if there's somebody that's interested in one of the priority areas more than another, they can go and look at it, and it lists, first of all, there's generally a brief conclusion about what did we learn from the SSPA with respect to the priority area, and then there's a listing of the sections of the SSPA that provide the basis for the conclusions.

So, in these next four slides, I'm going to briefly summarize some of the conclusions that can be drawn from the SSPA with respect to the four priority areas of concern, with the intention that I'll give you some of the conclusions up front here. And during the next day and a half, you'll hear a lot of the details that will back up these.

And, today, in Peter Swift's talk, you'll see the TSPA calculations from which these conclusions can be drawn. But I'd like to draw attention to the--it deals with the nominal performance. You know, Peter will talk some about disruptive events, but these conclusions only deal with nominal performance.

I think you'll see in the SSPA and also over the next day and a half, that supplemental model, the SSPA model, shows significantly wider ranges of doses at any given time, and times to reach given doses.

After the first 10,000 years, the base case model, that is the TSPA Rev. 00 ICN 1, results of last December, that base case model appears to be conservative. That was always the claim of the project, that we had a conservative model by using bounds, and the conservatism is shown in that the magnitude of the dose is less for the supplemental model, and it occurs later in time.

Now, during the period prior to 10,000 years, the supplemental model, the SSPA model, mean results are less than that small number of millirems per year, while the base case model from last December, those results, the dose rate is zero. So, even though the difference between the models is very small, .00006 millirems per year, the base case model from last December appears to be slightly non-conservative, if you will, because its dose was less, and now we have a higher dose, although an exceedingly small one.

But with respect to conservatism, I think people should always keep in mind conservative with respect to what. As defined here, the supplemental model is, you know--or the base case model is conservative for after 10,000 years, but arguably less conservative, although by a small amount. But with respect to the regulation, the 15 millirems per year in the EPA regulation, even before 10,000 years, the results are conservative.

I think over the next day and a half, we'll show you that for the thermal operating mode, the high temperature versus low temperature operating modes, significant differences are observed at sub-system level for some models. Bob MacKinnon and Jim Blink will show that. And also that the system level performance, you'll see this a number of ways, are essentially the same at the TSPA system level for the high and low temperature operating modes.

Over the next day and a half with respect to corrosion processes, you'll see that in Volume 1, there was a framework developed for--Volume 1 of the SSPA--for a conceptual model of long term passive film stability.

We also have new information that improved confidence in parameters and models related to stress corrosion cracking and aging and phase stability. And we've now included model of temperature dependence for general corrosion.

Multiple lines of evidence. Now, this is something that I personally believe that many of the scientists and engineers on the project have always done. But perhaps we just haven't done a very good job of explicitly documenting the multiple lines of evidence. Well, the SSPA has many sections in Volume 1 in an attempt to document the multiple lines of evidence that back up what we're doing. So, multiple lines of evidence were identified for most process and sub-system level models, and you'll find that in Volume 1. And these multiple lines of evidence are independent of the TSPA itself.

So, I'm coming back to the summary table, which had been shown at the last full Board meeting May 8th and 9th, a version of it was shown. It's available as a handout over there on the tables. You'll see that it's from Rev. 00E.

It's not a finished product. It's still a draft.

During the course of the next two days, you'll see variations on this table. Some of the presenters that follow after me have used excerpts from the table, if you will, to suit their own purposes. They may have even added some information to it. But in the end, there will be a final product of this table in the document.

I just want to make sure that everybody understands that SSPA, the first S stands for supplemental. It was never meant to be a stand-alone document for all time. There will be follow-on work. At the May 8th and 9th meeting, you heard about the replan effort that Bechtel SAIC submitted to the Department. And as part of the Department's review, we have requested that those first three bullets be addressed during the rest of the year.

Exercise the supplemental TSPA model to try and get more insight from it that's not already in the SSPA. Data collection and analysis will continue, and we will have an update on what we've learned during the course of the summer. And we will--we're asking for development of guidance on the treatment of uncertainty.

Those first three bullets will have some documentation in the autumn time frame to be referenced in Progress Report 25.

The next two bullets, I think you're aware of both.

The corrosion processes peer review is started. The week on Friday, the International TSPA Peer Review will meet here in Las Vegas, the IAEA NEA Peer Review, and we should have some input from them prior to site recommendation, at least some initial input.

And then the last bullet represents what Abe and I have brought up every time we've talked about our treatment of uncertainties. It's an ongoing process to manage, communicate, assess and analyze uncertainties. It doesn't stop with this effort.

And although I used four bullets to do it, Professor Bullen covered the same technical content, but he lumped it into three areas. After I'm done, you'll hear an overview and introduction of the SSPA from Rob Howard for Volume 1, and Peter Swift for Volume 2. And then over the remainder of today and the beginning of tomorrow, you'll hear a lot of details on the process models, which represents the material in Volume 1. And then you'll hear from Bob Andrews tomorrow as to what got into the total system performance assessment, and you'll hear from Mike Wilson as to the results of the TSPA.

And then that will be followed by four talks, Kevin Coppersmith and Jim Blink and Ardyth Simmons and Jerry Gordon, to address each of the four priority areas and what the SSPA tells us about those four priority areas.

And I believe that's that.

BULLEN: Thank you, Steve. Thank you, Bill.

Questions from the Board? Well, actually I'll lead it off because I do have a couple, and I'm not using Chairman's prerogative now. I'm just asking questions from my perspective.

Steve, you mentioned that the sufficiency requirement for the NRC requested a response by the 1st of October?

BROCOUM: That's correct.

BULLEN: Did you get a response from the NRC as to whether or not you'll actually hear by then?

BROCOUM: We had a manager meeting last week, and they said they will do their best to meet that date, although they pressed us for our dates, which we were not able to give them exact dates for the SSPA and the PSSE, but they said they are working hard to meet the October 1st date.

BULLEN: Bullen, Board.

As a follow-on to that, the comment period for the supplement to the draft environmental impact statement is out. Does that supplement adequately address the modifications and design that are covered in the SSPA?

BROCOUM: I don't know if you can adequately, but I think it bounds the modifications. I think that's what the EIS people would tell you. I don't know if there's any EIS person here. But they bound what's in the SSPA.

BULLEN: Actually, I have one more question for you, Steve, because you mentioned that the selecting of a thermal operating mode for the repository was going to have other parameters, and that you were going to give us an integration of how those parameters might work. But will there actually be criteria or weighting factors or an analysis that describes the actual process of making that decision?

BROCOUM: The report that we'll give you before SR will be a status of where we are at that time. The actual selection will probably occur post-SR and pre-LA, and that will--we haven't laid out exactly how we're going to do that selection, but my guess is it will address a lot of the issues you just asked. In other words, it will have criteria and stuff like this. But we don't have it yet scoped out. When we do, we'll be glad to tell you about it.

BULLEN: Okay. Bullen, Board. One more. I have a question for Bill, too.

You mentioned the International Peer Review for TSPA that's commencing this week, and you've got some very new and exciting or interesting results, it looks like, in the Volumes 1 and 2 of the SSPA coming out. What will the International Peer Review see and how will they respond?

BOYLE: You know, I'm not that involved with it. I think Peter may talk to them on Friday, or there must be somebody in here, or Bob Andrews I know is in the room. I don't know if the SSPA results are going to be presented.

BULLEN: I can defer that question, because I still have a day and a half to get the answer. So I'll wait and maybe there will be a more appropriate time to ask.

But as we see new PA results, and you've incorporated a review from an international panel of eminent experts, I guess the question is what will they see, and how will they be provided the opportunity to give you a response to that?

BOYLE: Yeah, I'd be interested. Like I said, I don't know the answer, but somebody must, and I'll be interested to hear it myself. Is it just the base case?

BROCOUM: Abe is in the field today with some of the members of that panel. But we'll try to talk to Abe and see exactly, because Abe is the point of contact with that group.

BOYLE: But I can tell you my guess is that when this started, the paperwork, you know, they must have been asked to review something specifically, and when they were asked, the SSPA didn't even exist. So my guess is that there's a chance it may focus in on the Rev. 00 ICN-1.

BULLEN: Thank you. Questions from the Board? Dr. Wong?

WONG: I just have one question. The NRC noted some QA problems with your models and data. What impact do you think this has on increasing confidence to this mechanism, or what are you doing about addressing those problems?

BROCOUM: At the management meeting, Nancy Williams from BSC gave a fairly detailed presentation of the four different types of reviews that BSC has undertaken to look at the data. Also, I think we told the NRC at that meeting that we will present an impact analysis on about August 15th as to what impact we have for data that isn't qualified on results we've been getting.

BOYLE: And the concerns that the NRC has expressed were not--they were expressed about other documents, not about the SSPA itself. But we're even, just to make sure that there aren't problems related to the SSPA and other documents, that's why there are these reviews that Nancy Williams described at last week's meeting, vertical reviews to make sure that when documents reference each other, that they're coherent, and also, you know, vertical within a document, but also horizontally across from document to document.

WONG: But the problems with those documents and the data that's contained in those documents don't feed into the SSPA?

BOYLE: I would have to defer to somebody, in particular with respect to the TSPA, was there one of these glitches or discrepancies that the NRC noticed, has it been carried forward into the SSPA TSPA, but I'd have to defer to Peter or

someone else more familiar with all the details of the TSPA.
SWIFT: Do you want me to say something on that?
The short answer to that--
BULLEN: Peter, identify yourself, please.
SWIFT: I'm sorry. Peter Swift, BSC, TSPA Department.
The short answer to that is that no, the problems that were identified in the TSPA Rev. 0 modelling have not been carried forward. We have, in fact, corrected them. Most of them were identified back during the winter, and we worked through them some months ago.
BULLEN: Other questions from the Board? Questions from Board Staff? Dan Metlay?
METLAY: Dan Metlay, Board Staff.
Now, these is a question for Steve. I didn't quite understand how the SSPA is going to fit in or not fit into your preliminary site suitability evaluation. Since the Draft 963 is largely a TSPA reliance regulation, which TSPA are you going to use? The one that's in the TSPA-SR or the one that's going to be published in Volume 2 of the SSPA?
BROCOUM: I think we're using both, because we're trying to look at a range of temperatures, you know, the high end and the low end. So, for the high end, we're depending on more or less TSPA Rev. 0 ICN-1, and for the low end, we're depending on the SSPA, I guess Volume 2.
METLAY: Well, now I'm even more confused. If SSPA is

in fact represents the Department's most current thinking of the state of the art, why wouldn't you use the two TSPAs that are in Volume 2 of the SSPA?

BROCOUM: Because those are not complete TSPAs. Those are just modifications of the original. Am I saying that right, Bill? Extrapolations of the original TSPA.

BOYLE: Yes, you could view it extrapolations, or extensions, but we haven't moved away from the Rev 00, ICN-1. We've just supplemented it, you know, made modifications to it to gain insight. So I would say personally, as the documents that Steve had showed, we're in a sense relying upon all of them to gain insights over a range, the Rev. 00 and then the two that are in the SSPA.

METLAY: Let me just say one thing. I think, though, the results, for example for the high end of the temperature range, though they may not be identical in, say, Volume 2 of the SSPA and the TSPA, will give you similar, or almost identical results and conclusions for the PSSE.

BULLEN: Other questions from Board Staff?

Seeing none, and seeing that we have a little bit of time, we have a question from the audience that I'd like to read. This is from Mr. McGowan. The first question is, "Will the cylindrical drifts in the repository be lined with concrete, and if not, why not? And I can see a finger of blame being pointed at the Board right now, but I'll let you guys answer that.

BOYLE: To the best of my knowledge, we don't have the concrete in there anymore, and I don't even remember why. I know there was the pH issue, the high pH was a concern. But it's not in there now.

BULLEN: So the current base case design basically for ground support has essentially steel sets and rock wall mesh as necessary?

BOYLE: That's my understanding.

BULLEN: Okay. The second question from Mr. McGowan is since DOE requires more than ten months to complete the SSPA, why shouldn't the public comment period extend for ten months, consistent with the provisions of Section 1 of the Fourteenth Amendment, equal access, equal protection?

BROCOUM: I think we opened the comment on the site recommendation process and documents on May 4th, and it's ongoing today, and we haven't announced when that comment period will close. So, the comment period I think opened May 4th. Have I got the date right? I think it's May 4th, and it's going on today, and it will go on until we announce it's going to close. So we haven't announced when it's closing.

BOYLE: A minor clarification. As I heard you read the question, it was stated that the SSPA had taken ten months, which that's--I mean, we have a lot of documents and a lot of acronyms, but the SSPA was created essentially late February, early March. So it hasn't been around for ten months.

BULLEN: Okay. Well, I didn't want to get into an argument about semantics--

BOYLE: No, no.

BULLEN: --of timing. But it's a valid question about comment periods, and I thank Steve for telling us that the comment period is still open.

Any other questions from the Board or staff? If not, the Chairman's prerogative is to forge ahead six minutes early and ask our next presenter, Mr. Rob Howard, who seems to be up and ready in the bullpen here, if he's ready to go. And, Mr. Howard is actually the integration manager in the Science and Analysis organization of the Management and Operations Contractor, BSC, and he's worked on the high-level radioactive waste management program in several areas, including performance assessment, design, data and software qualification and quality assurance. Rob?

HOWARD: I'm going to tell you about the scope, content, and give you a little bit of summary on Volume 1, which is the scientific basis for what Peter Swift is going to talk about with the scope and content of Volume 2. I am actually the responsible manager for the development.

Volume 1 covers the major processes expected to occur at Yucca Mountain, and it supplements the information described in the Analyses and Model Reports and the Process

Model Reports that we put out last year.

The subjects, as Bill Boyle alluded to, are organized in a manner similar to the way information was organized in the Science and Engineering Report. So we organized this document the way our thought processes typically work when we think about these subjects, and I know that can cause some difficulty when we're trying to discuss the Board's four priority issues, but we organized it around our thought processes.

It focuses on the technical work within each process model area. It encompasses uncertainty quantification, updates scientific bases, and analyses of a range of thermal operating modes.

With respect to unquantified uncertainties, specific uncertainties that were not treated explicitly in the analysis, model reports and the process model reports that were summarized in the science and engineering report, we've quantified some of those, including where we had parameter bounds, different conceptual models and assumptions, and in some cases, input parameters that were statistically biased or skewed.

Scientific information updates include new experimental results that we've obtained over the last several months, new conceptual models, new analytical approaches, and the identification and discussion of multiple lines of evidence.

And the thermal operating mode analyses in Volume 1, it includes the process level information regarding thermal dependencies, and how the process responds to thermal inputs, and the impacts of uncertainty on those processes. So, we were looking at the process level information, coupled processes in the rock, coupled processes in the drift.

What the document looks like, there's a big three-ring binder that Bill's holding up, that's what it looks like. That's a lot of information that's produced in a relatively short time. It was shorter than ten months. Sections 3 through 14 include a summary of the conceptual models that were used as a point of departure that were described in the science and engineering report. Sections 3 through 14 include the bulk of the technical information.

The specific content and level of detail in each section is variable, and it depends on a number of factors, including the extent of the analyses that have been performed to date, the amount of new information and data that we've collected in the particular scientific discipline, and the amount of information necessary and required to evaluate the range of thermal operating modes.

There's some process model areas where we didn't have to do a whole lot to address that third issue and, therefore, they're relatively silent on that. And each section contains a summary of information and recommendations for use in Volume 2, if that information was appropriate to be carried forward.

Just to give you a little pictorial of the issues that we do cover, and we will talk about climate and infiltration, unsaturated zone flow, coupled processes within the mountain, coupled processes within the drift. Bo is going to talk about seepage quite a bit, waste package degradation, waste form degradation, mobilization and transport within the EBS, unsaturated zone transport, saturated zone transport, and biosphere, and there's also information that we've developed on disruptive events that will help us evaluate impacts of different repository footprints, along with some other sensitivity information.

This is a modification of the table that Bill was discussing earlier. I'm not going to go through each one of these things. I do want to point out how the information is arranged. We have where the information is contained in Volume 1, what sections are relevant to the particular issue grouped along, you know, the subject areas that we're attempting to address.

I've also, at the request of some commenters, put a "T" in here where we have temperature dependencies in the model. So that gives you some insight into where we've actually used temperature dependencies in the model to evaluate the range of thermal operating modes.

As an example of some things where we used thermal operating mode, we needed the information for thermal operating mode evaluation and may or may not have a "T" in it, if you look at 3-D flow fields, I don't have a "T" there, but Bo's team developed a larger model domain to account for larger footprints. That in and of itself doesn't have a temperature dependency, but we used that information at the process level to evaluate the range of operating modes.

Bo is going to go through a lot of the details on the coupled effects on UZ flow and seepage into the drifts, and coupled effects on seepage, and he's got temperature dependencies.

Bob MacKinnon is going to be talking about performance of the engineered barrier system this afternoon. We've got temperature dependencies in those models, as well. And when I put a temperature dependency, like for the evolution of the in-drift chemical environment, the temperature dependency might not be a direct temperature dependency, but we might have a temperature dependency that's related to pH, for example, and the pH varies as the temperature varies, and that is what I've included in there. So that you can actually see a difference in some parameter. It may not be a temperature that you're seeing a difference in, but some other parameter that's driving the system that we look at.

We do have information on local chemical environments on the surface of the drip shield, and we've got temperature dependencies in there. Aging and phase stability, I believe Joon Lee and Greg Gdowski are going to be talking about some of the waste package issues tomorrow.

You'll note that I do have an "X" in here for stress corrosion cracking, where we actually looked at, you know, is there a temperature dependency that we could find for stress corrosion cracking, and that's discussed in Volume 1, but we couldn't find the temperature dependency, so I didn't put a "T" there.

We'll talking about cladding degradation. Pat Brady will be talking about that tomorrow, as well as other waste form issues, and there's temperature dependencies in there. Bob MacKinnon might be touching on some of these EBS transport issues as well.

Tomorrow, Jim Houseworth will be talking about UZ transport and the work that we've done in the SSPA Volume 1 with respect to UZ transport. Bruce Robinson is going to be talking about SZ flow and transport and some additional work we've done in that area. Not in these three particular columns, but there is information that we are putting in the SSPA trying to gain some additional insights with respect to 40 CFR 197. That regulation was issued a week or two ago, and we're taking a look at that, and we're seeing if we can provide a couple of additional insights with respect to what the implications are of that regulation, and how we might go about evaluating system performance.

Biosphere, you're not going to hear a talk on biosphere this week, but we did do some additional work in that area to address unquantified uncertainties. There's also going to be some additional work included in Section 13 to take a look at the 40 CFR 197 implications there as well.

Disruptive events. We looked at volcanism and seismic activity. We do have some updates in the scientific information there. Again, we're not going to go into a whole lot of detail in the next couple of days. Our disruptive events team is working on a technical exchange with the NRC this week, so they're tied up doing that.

What have we learned? Someone said it would be a shame if I presented all this material and couldn't come up with any conclusions about what it is we learned. So I thought about it a little while, and one of the things we learned is that the quantification of uncertainties has improved our understanding of both conservatisms and the non-conservatisms in our process model representations. Bill gave you a little bit of a hint about some of those issues in his discussion this afternoon.

Reduction of uncertainties can come from operating at either end of the thermal range, and it depends on the model of interest, and in some cases, it may even depend on the time frame you're looking at.

The post-closure impacts of a range of thermal operating modes and a variety of operating mode configurations can be evaluated by selecting appropriate thermal initial conditions for the model representations. And Jim Blink is going to be talking a little bit about that tomorrow afternoon. He'll have some thermal curves he can show you where we looked at a couple of different operational configurations to satisfy ourselves that we could represent the thermal implications of the operating mode by just selecting the initial conditions. There were some questions about whether or not we were going to be able to get at some meaningful answers there, and I think we've convinced ourselves that we made the mark. So that's a useful piece of information, at least from my respect.

Waste package degradation evaluations with respect to thermal operating mode must consider thermal dependencies and the local chemical environment. It's not just a temperature, hot, cold, warm, cool issue. It's the associated processes that go along with it. It's not just a temperature issue. And Greg and Jim will be talking about some of that tomorrow.

Capturing multiple lines of evidence is a useful exercise in improving our understanding of repository performance. I agree with Bill. We do that as scientists and engineers. We have been doing that. We haven't done a very good job at all of articulating it, either to ourselves or to other people, but it is part of our thought process, and this aspect of the document development was good to do. It was a healthy exercise for us.

The Supplemental Science and Performance Analysis is not the end of the story. It provides a point of reference for continuing work, and Bill touched on that. It is, you know, where we are today. There's a lot of information in Volume 1. I can tell you that putting the document together and seeing the results was more than I expected in more than one way. There's a lot of good information in there. I'm satisfied with the document. I believe that we've gone a long way towards working on the issues that the Board has identified, as well as issues that we've identified ourselves. It's a good piece of work, and I'm looking forward to getting it off of my desk.

BULLEN: Thank you. Everybody is studying very diligently, so again with my Chairman's prerogative, I get to jump in.

I also had a copy of that 1300 page document. Unfortunately, I just carry around the pictures. So I have the table figures at the end of it, and I have a question about some of the consistencies associated with the models. Specifically, harkening back to a couple weeks ago, there was an introduction to the Waste Package Materials Peer Review that's being done. And taking a look at, for example, predicted temperatures and relative humidities in the repository as a function of time, during the preclosure ventilation period, for a number of the figures, including some figures that are in here, there is an increase in relative humidity during the ventilation period. And I guess the question that I have is why? And is that an inconsistency in the models, or is there some impact that I don't understand that may actually be happening as you dry the rock out?

HOWARD: Dr. Blink, will you help me with this question? I don't want to botch it.

BLINK: Jim Blink, LLNL. Dan, would you say one more time what you observed and what figure maybe?

BULLEN: In a few of the figures here where they take a look at temperature versus time, and then they also plot coherent with that, relative humidity, whether it be for the high temperature or the low temperature operating modes. During the first 50 years, the relative humidity appears to be increasing. And I guess the question is why? Because as the empirical observation of being in the mountain when you blow air through it, it's pretty dry, and so why would I expect the relative humidity to be any different in a ventilated repository that's operating, waste packages or not, than what I see in nature now? Why does relative humidity go up during the 50 years, is the question?

BLINK: The relative humidity is initially set at 30, 35 percent, something like that, by the ventilation air. It then drops as the temperature goes up, because the denominator of the relative humidity equation increases. And then it slowly rises as water comes out of the rock and vaporizes into the stream. That's a relatively small effect, and the humidity at the exit of the ventilation stream, considering the higher temperature, is still a lower number than the inlet.

BULLEN: Okay.

BLINK: So I don't think it's going up.

BULLEN: Well, that's exactly what I would expect. But maybe we could talk about specific figures some other time. But it's just one of those anomalous representations that is in here that makes one wonder how self-consistent all the modelling is.

BLINK: Show me the figure at the break.

BULLEN: We'll talk about that a little bit later. But it's just, you know, one of the issues that you see where, and a great deal of work has been done and I've really got to compliment you on putting together 1300 pages that's hard to get through. But I guess the point that I want to make is that you built good models, you've identified where the thermal, additional thermal dependencies are. The next logical follow-on question is are there data to support the models with the thermal dependencies, and if so, did you have it, or do you need to get more? And you notice that this is just a point in time here, or a point of reference for continuing work. Does that continuing work include the requirement for additional data, and how are you going about getting it?

HOWARD: Well, yeah, the work plans do include going about getting additional data, and Bill touched upon that. That's one of the more important aspects of our plan, that the data that we're going after, or data that we need to support our analyses, and in fact, you know, once we digest all of this information, because quite frankly we haven't finished digesting all the information, we'll be looking at that for what additional data needs we need to support these models. It isn't the be all and end all. I mean, science does have to be backed up by the experiments.

BULLEN: Bullen, Board, again. Then do you think you'll have sufficient data to satisfy the NRC's sufficiency requirement, or will the data, forthcoming data be necessary to support it? I ask either of you. Rob, if you want to take a shot at that?

BROCOUM: We're on record with a letter to the NRC I think in November or October of '99 on what they should base their sufficiency on. That letter had attached to it a table of all the documents that we think they should use. All of those documents have been delivered to the NRC. So, from where we started in '99, you know, we think we met that commitment we made to the NRC at that time.

BULLEN: Bullen, Board, then a quick follow-on question. That implies that the SSPA was not one of the documents that was to be the basis for the sufficiency requirement?

BROCOUM: That is correct. That was in '99. We hadn't even got a--on the SSPA at that time. But the NRC was pretty clear at the management meeting last week they would like to see the SSPA, and any other technical document, prior to their making a sufficiency--so they will obviously review the SSPA.

BULLEN: Thank you.

BROCOUM: I don't want to speak out of turn for the NRC.

BULLEN: That's fine. I'm just trying to finalize it.

Other questions from the Board? Dr. Parizek?

PARIZEK: Parizek, Board. On Page 13, the middle bullen, I probably need some examples of this. It seems like you could take reductions of uncertainty either with the warm or cold repository, in which case the uncertainty issue just falls out. You gain ground either way. There would be other reasons then for picking the repository design other than temperature, because of uncertainty. Right? Can you elaborate on what's meant there?

HOWARD: Yeah. I won't elaborate too much because I don't want to steal anybody else's thunder. Bo Bodvarsson is going to be talking about some seepage models where the thermal dependencies he looked at are in the higher temperature operating modes, we didn't see any seepage. When you go to the lower temperature operating modes, you do see seepage early on.

And then, you know, with respect to the lower temperature operating modes, you're looking at reducing the uncertainty in the rates of corrosion for general corrosion if you, you know, take into account the Arrhenius relationships that we have with the temperature dependency for the general corrosion rates. Those are, I guess, two examples on either side that I would point to that say it can be dependent. It's a useful piece of information for us. I mean, it's not, you know, I'm not prepared to say, well, what would you do with this?

Is either one of those a determining decision factor for selecting an output mode? I would say absolutely not. There's a lot of other things that have to be considered before you can go there, and Steve outlined some of those. But the performance implications, I know that there were thoughts that if you go cooler, that you're going to actually reduce uncertainties. And in some cases, yes, that's true. That's not true for all cases.

PARIZEK: It just helps clarify the benefits for either design.

HOWARD: Right.

PARIZEK: Either hot or cold.

HOWARD: Yes.

PARIZEK: And then kind of weighs in with other variables that come into play.

HOWARD: Right.

PARIZEK: On Page 14, the middle bullet again, on the multiple lines of evidence, did you get different input from scientists versus engineers in this process of going through your multiples of evidence? From a geological point of view, we see all sorts of things they do in the field, and we're always weighing, you know, the benefits of some observations over others, and so on. But from an engineering point of view, did you get similar kind of input from the engineering community or from the materials people? You either have metals or you don't have metals, or either they corrode or they don't corrode?

HOWARD: I'm an engineer by training, and I certainly don't think that kind of bipolar, if you will, I mean I don't think it's an either or process, and the engineers that I work with on the project don't think of it in those terms. They do think of, you know, well, what is it we've done in the past that can inform us about the future. I mean, there's a great book by Henry Patrowski called "Design Paradigms" that talks about failures and errors in judgment in engineering history and, you know, it's one of those books that engineers should be reading.

And, you know, I think a lot of engineers on the project, including myself, have read it several times. It talks about, you know, when you're building something, you design something, you go back to previous designs to inform that. I mean, you don't--you could start cold, but that's usually not how engineers approach problems, just like scientists approach problems, they build on what's done in the past to inform what it is they're going to do in the future. The thought process is there. It's just not articulated, and that was what we're doing now.

PARIZEK: Thank you. That helps.

BULLEN: Other questions from the Board? Dr. Wong?

WONG: Jeff Wong, Board.

Going back to your previous slide, if I can, just in relationship to the reduction of uncertainty. Did the reduction or your feeling that there was a reduction actually come about because you had new information or new data or better model, or did the reduction come about because you simply used a narrower band of input parameters?

HOWARD: I don't think it came because we used a narrower band of input parameters. And I'm going to ask Bo maybe to touch on it when he hits some of his topics later today. But I think for the most part it came from, you know, we pushed these models. We pushed our thinking on them, where we said we were going to try to be conservative in the past, we pushed it and we looked at new data off the project and pushed the input parameters, not trying to, you know, get narrower, but really look at, you know, what was available in the world to us to address the issues.

So I think it has more to do with the development of the thinking and the models and exercising the models for the operating modes than it had to do with just trying to squeeze the band. I don't know if that answers directly your question. I don't think it does, but I'm having difficulty with that.

WONG: You know, did you actually use new data, or did you have a better understanding of the data that you had so you were able to constrain that data, or constrain the model? I'm trying to get at how you achieved your reduction of uncertainties.

HOWARD: I'll let you ask that question for the guys that developed the detailed process models.

WONG: All right. Fair enough.

HOWARD: I'll exercise my management prerogative.

BULLEN: Other questions from the Board? Questions from Board Staff? Carl DiBella?

DI BELLA: Yeah, this is Carl DiBella, Staff.

Could you go to your Page 8, which was a table? I have a question I'm not sure whether it is about the headings of the table or about the entries of the table. But if you look down on the row about halfway down, there is an entry that says General Corrosion Rate of Alloy 22: Uncertainty/Variability partition.

HOWARD: Yes.

DI BELLA: And then if you look at the column that's labelled Cooler Thermal Operating Mode Analysis, there's no "X" or "T" in that box. Now, can you explain why there's no "X" or "T" in that box? It might have something to do with the heading, it might mean something else. It would seem to me that you would do this analysis.

HOWARD: Yeah. And I'll ask Greg Gdowski or Joon Lee to correct me if I get this wrong, but what we were doing with this uncertainty and variability partitioning, the uncertainty exercise that we were doing there is associated with implementing the waste package degradation model. There's a module in there called galcium variance partitioning, and we had questions regarding what are the impacts when we make the split between 100 percent uncertainty or 100 percent variability in the corrosion rates, or, you know, 50/50, or 75/25, and we were just testing that module and the conceptualization of it and the reasonableness of the results with respect to that. It wasn't a temperature exercise. It was a model implementation of the code exercise for this galcium variance partition.

DI BELLA: So I'm reading it correctly. You did not do an analysis of that for the cooler temperature mode of the partition?

HOWARD: That's correct.

BULLEN: Other questions from Staff?

This is Bullen, Board. I've got one more Board question that I neglected to ask.

You alluded to 40 CFR 197, and its implications on the required changes in the performance assessments that will be necessary for SR. Are those changes merely moving the site boundary, or volumes of water, or what do you project those changes to be and how difficult will it be to address them?

HOWARD: Mike Voegele, sitting in the back there, do you want to help me with this question?

VOEGELE: This is Michael Voegele.

There are actually about three things in the final rule that are subtly different from what was in the proposed rule. One was the site boundary, as you noted. One was the amount of water for the groundwater protection standard. And one was a slightly different interpretation of the human intrusion scenario.

We are looking to understand how the PA calculations that we have done to date are impacted by those changes in the rule, and we're intending to address them in these documents.

BULLEN: Bullen, Board.

So those types of calculations are going to be extremely difficult, or do you think it will be something you can get done in time for SR?

VOEGELE: In the documents that we're working on right now, and you have to understand there's another set of documents before the SR, what we're trying to do is make an assessment of whether or not--how big the differences would be. We're doing scaling type calculations rather than fully rerunning the PA calculations. We'll have to look to running the PA calculations fully for the next set of documents. But there will be an assessment based on analytical investigations as to how big the differences are between what we've done and what we will eventually have to do to demonstrate compliance with the standard.

BULLEN: Thank you.

Now, we still have about five minutes left, and unfortunately I have six questions from the audience, and so

I'm going to just defer a couple of them, but I will ask a few from each of the people who submitted them.

The first one is actually from Mrs. Devlin, who looks through all the documents and wonders in these studies where are the evaluations of microbiological influence corrosion on the new stuff, meaning Josephinite and the other materials. And is there a place we could direct her with respect to the SSPA analyses of MIC, or are there other supplemental works that we should be looking for?

HOWARD: Yeah, in Section 6.3, we do have discussions on microbial growth and biological growth within the drift. The impacts on corrosion, our models for that haven't changed much over the last ten years--or excuse me--over the last year or two. We have in the corrosion model, an enhancement factor for MIC. The comment said something about Josephinite. We didn't do any calculations for corrosion of Josephinite.

BULLEN: Bullen, Board.

Josephinite was one of the analogues.

HOWARD: Analog, yes.

BULLEN: And I'm not familiar with any of those studies. But that was 6.3 of Volume 1 or Volume 2?

HOWARD: Of Volume 1.

BULLEN: Volume 1. Thank you. That helps me a lot, and maybe Mrs. Devlin can chase that down.

When will this be available for the public again? End of June, early July? Meaning I can't give away my copy today?

HOWARD: Right. A couple weeks or so.

BOYLE: But to let the public know, as I showed with the table that's available as a handout, it is a draft. It's not done yet. When it's done, as with all our documents, it will be available on our website. If anybody wants a hard copy, just let us know, and they can carry this around just as Rob does if they want it.

BULLEN: Thank you. Another questioner asked why these presentations are cursory and generalized rather than incisively detailed.

HOWARD: Okay. I could give him the easy answer, and it's like the Board set the agenda.

BULLEN: I think you're doing the overview, Rob. Right? This Bullen, Board, again.

HOWARD: This presentation that I made and the presentation that Bill made, and in fact the one that Peter made, are meant to be overview introductory type presentations. We've got a lot of detailed information that we're going to go when we get down at the process level. So, my only suggestion is to wait.

BULLEN: This is Bullen, Board, because that's actually a very important differentiation between a full Board meeting and a panel meeting. In a panel meeting, we do hope that we ask the detailed questions and we get down to the real basis of the science, and sometimes it may be as though the Board is droning on and on about a specific topic, but this is our one opportunity to actually delve very deeply into the science.

Sometimes at the full Board meetings, we specifically don't ask DOE to be as technically detailed, and we're looking forward to the next day and a half to being exactly that.

I do have one more question that I want to ask before the last minute is up. And let me apologize to the other questions that were submitted. If you would like these back to ask them during the public comment time, I would more than welcome them, or I will read them during the public comment time, if you'd like that.

Thank you, Mr. McGowan, because that's a couple of the questions that I'm missing. The other question was actually for Steve Brocoum, which says, "Regarding the problems found in TSPA-SR, Rv 0, ICN-1 by the NRS, and Dr. Swift's comments that the DOE knew of these problems in the winter, why hasn't an ICN been issued to cover these problems? Does DOE still plan to use this document, given the numerous deficiencies, as its basis for SR? If not, what will be used as a basis for SR, and when will the public have access to those documents?

BROCOUM: Multi-part question. I think as they were developing and finalizing, and I'll look to Bob Andrews to help me here, as they were finalizing the TSPA-SR, they have an errata file, and that errata file had 30 or 40 items on it that they knew of errors. And at that time, the analysts went through each of those and decided whether it was significant or not, and at that time, they decided it was not significant to address at that time the issue of the TSPA-SR.

The NRC found eight errors, I believe, that they informed us about around May 4th, I guess it was, and some of those duplicated the ones they have in the errata file. Some did not. Some were new ones. All those have been reviewed as to their significance on the results. I think it's been determined they don't have much significance on the results. However, they are errors.

So, all of that is being looked at, and I think there has been some CARs--Bob, do you want to talk about the CARs that have been developed?

CLARK: This is Bob Clark. I'm QA director. Bob Clark, DOE.

Actually, the results of these NRC identified issues, no CARs got issued.

BULLEN: Bullen, Board. Are CARs corrective action reports?

CLARK: Corrective action report.

BULLEN: Thank you.

CLARK: That's a significant deficiency adverse to the QA program. But a few deficiency reports, which are deficiencies but not significant deficiencies, have been issued. And one of those, in NRC's face, they expect, and me as the director of Quality Assurance expects for our processes that when deficiencies get identified, there's a discipline process by which to identify them and take care of them. That was not done by the analysts themselves. On their own, they kind of said this is a no, never mind, and didn't enter into the process. So we wrote a deficiency report to get to the root cause of why you did that, and to preclude any recurrence from that, such that everybody knows you identify a deficiency, you enter it into the system.

BROCOUM: And, finally, I think again, I said earlier, that we told the NRC we'd provide them a report of the impacts for the data that's not qualified in our information by August 15th. The NRC has asked for that and we've promised to give them that. So I think that answers the question.

BULLEN: Thank you. And I will indeed ask the remaining questions during the public comment period. But now I'd like to move on to the next item on the agenda. Thank you, Rob.

Our next presentation which was scheduled for right now is by Dr. Peter Swift. Peter shares the management responsibilities in BSC of the Total System Performance Assessment Group for the Potential Repository at Yucca Mountain. He works along with Jerry McNeish at Duke Engineering and Services, with an emphasis on the direction of technical analyses. He's also the manager of Total System Performance Assessment Department at Sandia, and Peter will talk to us about Volume 2 of the SSPA.

Peter?

SWIFT: First of all, I want to acknowledge a bunch of other people up here other than myself. Jerry McNeish is here, and the entire TSPA Department. Any big analysis like this, takes dozens of people to put it together, and in fact, the whole science project from the M&O has worked on this.

Another point I want to make, which I think both Bill and Rob have said, is that this is work in progress we're reporting on here, very much so with Volume 2. We're still in internal review on most of this, so I ask for some patience on this. You may get some answers like, well, we're still analyzing that one, and those will be true statements if I say them.

All right, what I'm going to try and cover here, the purpose, scope and content of SSPA, Volume 2, the relationship between Volumes 1 and 2, including a few words on how we got from what's in Volume 1 to what's in Volume 2, some context for the interpretation of the result in Volume 2. Basically, what I'm going to do there is give you some pointers to later talks. I will show the summary results. And if you want to skip ahead to the back of the packet, they're there. We've got basically the new performance assessment, supplemental model results there. But the technical basis for them and the details of those results, put them off for later. I'll answer what questions I can, but I'm not expert on a lot of it.

This repeats something that was in Rob Howard's presentation. That's good, because Volumes 1 and 2 are doing the same thing. SSPA Volume 2 documents analyses that provide insight into the effects on TSPA of three types of information that were not addressed back in Rev. 0 TSPA.

The first one, the uncertainties that were not fully quantified. That's the unquantified uncertainties work where we used conservative assumptions, some bounding assumptions, simplifications.

Second, additional scientific information. Now, the research of the project has moved on since a year ago when we were putting together the Rev. 0 models, so we've updated to bring that information forward.

And, finally, the effects of alternative thermal operating conditions. This is a request from the Board and others that we do this. So we've included these analyses in

Volume 2.

Just to offer a definition here of what we mean by alternative thermal operating conditions, we've examined these effects in Volume 2 by evaluating two repository operating modes. We picked two, HTOM and LTOM, high temperature operating mode and low temperature operating mode. HTOM is essentially the same as what we analyzed in the TSPA Rev. 0 base case in terms of design assumptions for some of the models that have been updated. But the design is essentially the same as what was in Rev. 0.

The LTOM design, and I'm hoping Jim Blink will cover this tomorrow, basically, this is a design that uses longer ventilation periods and changed package spacing to ensure an outer surface of the waste package below 85 degrees C. average temperature.

There are two main types of analyses in Volume 2. This slide talks about the first type. The next one talks about the other one. First, sensitivity analyses that we did using the Rev. 0 model, the same model you saw last winter where we've done one-off analyses in which all the models and parameters are identical to those in Rev. 0 except the case we're looking at, the one component we're analyzing, we changed that one, held everything else the same. It's still a probabilistic analysis. We sampled, did multiple realizations, but everything else is the same as it was in

Rev. 0 except the new assumptions or inputs.

We used these analyses to examine the effects of unquantified uncertainty and new information. You saw some of these in January at the meeting in Amargosa Valley. Results of this type of analysis are directly comparable to the Rev. 0 results. It's truly a one-off comparison. What would Rev. 0 have been like if we had changed this one thing? But these one-off analyses don't give us the insights into the coupled effects of all the uncertainties taken together, and also they were not particularly useful for looking at thermal effects, because the Rev. 0 model was not all that sensitive to thermal outputting conditions. So we've gone through these one-off sensitivity analyses.

Then we built a supplemental TSPA model. A full system-level analysis incorporates the major uncertainties from the unquantified uncertainty work, important updates from new information, and that gave us a model that we felt was sensitive to the alternative thermal operating modes. So we used this new supplemental model to do full performance assessments for alternative thermal operating modes.

The system-level results show the overall effect of the unquantified uncertainties and new information. And the last point here again, I said this already, but later presentations will give you the details of the model changes and the system and subsystem results.

How do we get from Volume 1 to Volume 2? This graphic here, the big document here is supposed to be Volume 1, which as Bob showed you isn't very big. And the significant changes from this were forwarded on to Volume 2, where we first ran these unquantified uncertainty analyses, the one-off analyses. They appear actually in Chapter 3 of Volume 2, and I'll give you the table of contents in the next slide. And then only a portion of that information was forwarded on into the supplemental model.

Basically, the decision as to what from Volume 1 made it all the way to the supplemental model here has two main components. First of all, did we see a significant impact in these one-off analyses? And if we didn't, and we were pretty confident that that was a robust conclusion, those are things we did not include forward.

There were also some places where we got guidance from Volume 1 that based on their own internal analyses, they being the authors and experts in Volume 1, they concluded this wasn't essential to be carried forward.

Leave the slide up here. I'll come back to this in a minute when I have a table up here. This is the outline. When you get Volume 2, it's a much shorter document. There's a very draft copy of it sitting in front of me there. Introduction, these are bullets, Chapter 1, 2, 3, 4 and 5. One is just a brief introduction. Two is our methodology.

Three, these are the one-off analyses. Every analysis reported in Chapter 3 is a one-off comparison to the Rev. 0 model. Then we go through it for system level nominal scenario, subsystem level evaluations. These will be the components of the nominal scenario, and a section on disruptive performance, primarily volcanism.

Chapter 4 has the updated supplemental TSPA model, with system level results, subsystem analysis from those results, and the igneous disruption scenario. And then a very brief Chapter 5, which is the summary and the major conclusions.

This table here is actually the same information as on the table that Rob showed, and on this table, it was updated from what was handed out a month ago. There's a copy of it over here. All Rob and I did to produce our versions of the table was to edit this to focus on our talks. So Rob knocked off the two right-hand columns of this paper table. I left them on. These are the ones that apply to Volume 2, here and here.

I'm not going to work down through this. This is here for reference, and basically so you can figure out what questions you want to ask the later speakers about cases where we ran, using the Rev. 0 model, we ran a one-off calculation for all of these, and these were the things that were carried forward, and we should be able to find their impacts for you in the supplemental model. And you can see there are some things where the new thermal hydrologic model, we didn't do a one-off. It wasn't suitable for that. It went directly into the updated model. There are other cases where we actually ran sensitivity analyses, and then on further guidance from subject matter experts on Alloy 22, we did not include that particular model into the new updated PA.

Just more of this. There are three pages of it here, and I'm not going to go through these. I want to get to the results here.

Okay, this is just an example of the types of one-off analyses. What does a one-off analysis mean? There are two codes up here. These are both mean annual dose histories. The black one is the TSPR Rev. 0 nominal mean for a million years, and the red one here is everything else being held the same in the Rev. 0 model, except in this case, we've used the extended climate model, just to show you what Rev. 0 would have looked like had we used this climate model instead of the one we did.

This figure will come back up again tomorrow in Mike Wilson's talk, I think. But I show it here basically just as an example of what a one-off comparison looks like. And these are means. The full distributions of the results, the full horsetail plots are available.

This is a supplemental TSPA model prepared to Rev. 0. This is the Chapter 4 type of results that we show in Volume 2. And the black curve, again this is the million year mean from Rev. 0. It was the same curve you saw in the previous slide.

Now, these are means from the new supplemental model. The red is the high temperature operating mode, HTOM, and the "L" is the low temperature operating mode. And I think the next plot will show you the horsetails, show the full distributions that go into the two new results.

This is more than just up there as an example. These are fairly important results, so it's worth mentioning, since first order observation is here. I think we can come back to them tomorrow when you've seen more detail. But some things sort of jump right out at you. The first things, in Rev. 0, we had zero dose prior to 10,000 years, and now we have a small number here. By the way, it's quite a small number. That comes from about 23 percent of our realizations out of the 300 realizations shown here, about 23 percent of them had one or two waste packages showing an early failure. This is due to a reconsideration and expansion of these uncertainties in our treatment of initial defects and welds due to possible improper heat treatment.

Slower waste package corrosion delays the main rise in dose is the next thing that just sort of jumps out at you.

Here is the main slope of it here in Rev. 0. We've pushed it out now beyond 100,000 years before doses start to decline. What this is telling us is that the bulk of the waste package failures are now occurring much later in the supplemental model than they did previously.

And peak dose has gone down. It was here. Now it's basically out here at the end of the simulation, a million years, and it's gone down considerably. A major driver there, the lower solubility limits, neptunium, for example.

The final point, at the system level, the thermal effects are pretty minor. You know, you've got to look hard to see the difference between HTOM and LTOM. It's there, and we're working on understanding why the differences are there. But the first order observation is they look pretty darned similar. You get out past tens of thousands of years, and the system is not all that sensitive to the operating temperature in the early time.

This just shows the full series of realizations. I'm not going to go over this more. 300 realizations for high temperature and low temperature, and the results.

This is the new results for our igneous disruption model. I want to first of all say something that I think some previous speaker mentioned, Rob or Bill. The project's team on igneous activity is in a technical exchange with the

NRC tomorrow, and won't be here tomorrow. That includes me. I'll be over at the NRC meeting. So if we have questions about this, now is a good time to ask them. I will try and field what I can today.

Again, the black curve here is the mean from Rev. 0. These are probability weighted doses now. The mean curve from Rev. 0, in Rev. 0, we only ran it 50,000 years. We've added computational power since then, and now it can run out to 100,000 years. The blue and red overlay perfectly during the first part of the curve. That's why you only see blue here. And at later times, you start to see some differences between the high temperature and low temperature modes.

The first order results here that are worth noting, well, first of all, something has happened. The eruptive doses have gone up by a factor of about 20 from here to here. The curve from this point over in Rev. 0 is largely dominated by ash fall doses from an eruption, and this part of the curve in Rev. 0 was dominated by groundwater doses from damaged packages. While the eruptive dose has gone up, the groundwater dose has gone down.

And the next point--maybe this was a point I should have noted first--the overall peak dose is about the same here and here, but it shifted much earlier in time. It now seems to be coming from the ash fall. And this is something that I think is sort of a good confirming sort of result. We do see effects here of climate change now in the groundwater. This has got our new climate change model. We in fact have a glacial climate starting at 38,000 years, and here it is. You can see it.

And there's more information here on the slide. I can go through that if there are questions. But basically, yes, we do understand quite a lot about why these doses have changed and what the drivers were on that. But I'm going to try and stay on schedule, so I'll field questions if it comes up. These are again the full suite of plots from the igneous cases, and there's not a lot of information there.

And I'll summarize. What's in Volume 2? Two main things; a set of one-off analyses, lots and lots of figures that show comparisons of new information to Rev. 0, you know, one-off mode where each plot tells you how Rev. 0 would have been different if this were the only thing we changed. That's Chapter 3. Chapter 4, the supplemental TSPA with the updated models that compare performance at high temperature and low temperature operating modes.

And detailed discussions in later presentations, component by component we're going to go through the quantification of uncertainties and new information, i.e. how are the models different, what are those two alternative thermal operating modes we talk about, how this new information gets integrated into the TSPA. Bob Andrews will talk some about that, for example, Kevin Coppersmith. Detailed results of the TSPA, Mike Wilson will talk tomorrow on that. And what do we learn from these uncertainty analyses? I think several people will come back to that during the course of the meeting. I'm going to stop there.

BULLEN: Thank you, Peter. We have questions from Board members, starting with Dr. Sagüés, and then Dr. Christensen.

SAGÜÉS: Thank you very much. I guess I would like to start with Number 13. This picture is probably I think one of the most dramatic--clearly, for the initial low doses at the beginning, there is like an order of magnitude improvement in the time scale of releases.

Now, first, is this is a result primarily then of changes in the way in which the contribution of the engineered barrier has been evaluated?

SWIFT: Yes, I would say the first order, the largest change here is in the waste package performance, and I'll let the waste package team address that tomorrow, putting temperature dependency into the corrosion model, for example, so at later times when it's cooler, corrosion is slower. That accounts for a big part of this.

We have not finished analyzing this. I suspect we'll see a fair amount of benefit here from--improvement in performance from the changes in the treatment of the diffusive transport from the EBS into the UZ. And Jim

Houseworth has something to say about that. This is an analysis in progress. We haven't finished taking these plots apart to see what's driving it. We're quite confident that the big driver out here, a big driver, is the changes in the solubility models.

SAGÜÉS: And this is solubility inside the waste package?

SWIFT: Yes, solubility in the in-drift environment, in the package and in the in-drift.

There's one other thing I should have mentioned. The spikiness here is the climate change. That shows up very nicely, but it's presumably not a major factor.

SAGÜÉS: Right. But also, most of the, and if I can concentrate again on the sloping portion of the curve there, do I understand correctly that the curve that results from the introduction of temperature dependence on the way in which corrosion brakes of waste packages are evaluated, is that correct?

SWIFT: That's my understanding. Is there someone here from the waste package group that would like to speak on that? Greg or Joon Lee?

ANDREWS: This is Bob Andrews. There will be presentations tomorrow morning, you know, unfortunately, they're tomorrow morning, which will describe the changes in the waste package degradation modelling. One aspect, and you're absolutely correct, is the thermal dependency aspect. Another very important aspect is new information on the stress states, and the stress state uncertainty, and the yield strength at the welds.

If you'll remember from the Rev. 0 analyses, most of the initial failures up to 50,000, 60,000 years were failures at the welds, at the two welds of the Alloy 22 which had been stress mitigated. Virtually, those changes in stress states and stress state uncertainty eliminated that failure mechanism as well. So it's a combined effect of the thermal dependency of the general corrosion rate and the stress state and stress state uncertainty at the welds.

But, you know, any more detail than that, probably the people tomorrow, Greg and Joon, will probably be better to answer it.

SAGÜÉS: Okay. Let me then close by saying that if that effect, and I see a shift and I also see it in Volume 2 in some of the initial information, a similar shift, that seems to be almost like simply the result of a correction on the corrosion rates assigned to the uniform dissolution. If that is the case, I do understand furthermore that that correction was due to a relatively--to the introduction and consideration of the--of a relatively small series of experiments performed in a very short time in the laboratory to evaluate activation energies for corrosion rate evaluations. If that is what appears to be, and I guess that tomorrow we're going to hear more about it, then a few days worth of experiments appear to have changed that in a very remarkable fashion. I wanted to leave you with that.

BULLEN: Norm Christensen?

CHRISTENSEN: I think I've answered my own question.

BULLEN: Okay. That's fine. Richard Parizek?

PARIZEK: Parizek, Board.

For both Volumes 1 and 2, what's the cutoff date for data? There's always a time when you have to kind of stop in order to draw one of these documents together. Can you help me with that?

HOWARD: What's today's date?

PARIZEK: I mean, in the case of Dan's abbreviated summary of Volume 1, something was printed. You know, there's a big fat volume. So it must have cut off today. I mean, it's ongoing. I mean, in order to issue that volume in its working form, there must have been a drop dead date for putting new stuff in?

SWIFT: Sure. But actually Rob's answer is the right one, which is this is work in progress. These results have not been through DOE review yet.

PARIZEK: Okay. So you're going to be adding?

SWIFT: If new information came up sufficient that we actually had to do something about it, this will be a decision that has to be made.

In general, these model changes were finalized in early to mid April. There were changes in the waste package corrosion model up until early May.

PARIZEK: So really it's dynamic, and the next version we see will include whatever else comes up this summer and early fall, I assume.

SWIFT: It's my personal belief, my hope, this is what you'll see in the document.

PARIZEK: Relating to Figure 13--

SWIFT: That's what I want to show in the document.

PARIZEK: --you offered the possibility that we'll learn perhaps tomorrow why the blue and red curves mimic each other and don't separate, or would have thought that maybe the hot versus colder design might have given you bigger differences.

SWIFT: You'll see some system results in plots of things like humidity, where you can certainly easily distinguish between the two models. But by the time you get, you know, all the way to the dose at 20 kilometers--and, by the way, these were calculated at 20 kilometers, not the NRC's 18, or the EPA's 18 kilometers--anyway, the point is when you get to final dose, they do look quite similar.

PARIZEK: Then for the Rev. 0, or the base case, climate didn't seem to show up in that one? You don't get the excursions that we see in the red and the blue after 100,000 years.

SWIFT: Those?

PARIZEK: Well, you don't have them in the base case, the black line.

SWIFT: Right. That's because this base case was done with a constant climate model after 10,000 years.

PARIZEK: Okay. And solubility didn't enter into that one?

SWIFT: Well, no, actually if you go back to the Rev. 0 work, this was the curve we showed as a so-called baseline in Rev. 0 last winter. We did show the effect of including the model we had available then, as of last fall, for secondary phase effects and solubility, and that showed a lower curve. And then we put the version of the climate model, which is essentially the same as the climate model we have now, we put that on also, and we saw an effect then last fall that looked not unlike at least out here, spiky and roughly an order of magnitude below the black curve that you see here.

So, I'm not sure if that answers your question. The reason this is smooth is the constant climate.

PARIZEK: Okay. And just from a general reaction, I mean, it's more realistic to me to see the red and the blue than it is to see the black, because it's hard to imagine 11,000 or 700 units flawless. So, from a credibility point of view, I mean, it just raises your expectation to prove that in a convincing way. So it seems to make more sense to me to see it this way. That's neither here nor there for the moment.

As far as the igneous effects--

SWIFT: That's slide 14?

PARIZEK: Yes, Slide 14--15, I guess 15 shows some of the difficulties there. Now, if this whole thing has to be dealt with and you can't quite live with it, what's the engineering solution to that problem if you really have to kind of reduce the risk and the uncertainty? Is it backfilling the repository? Would we be back to that, because we had drip shields and we had--you know, there's always an add-on in order to kind of address a problem. If something like that is not going to be considered acceptable, what's the solution to that problem? You won't be here tomorrow is the reason I'm asking you today.

SWIFT: Right. These results, both old and new updated models, are calculated assuming that the access ramps and mains were backfilled, but the drifts were not, the emplacement drifts were not. What that does is it limits interconnection from one drift to the other. Damage is limited to the drifts that are actually crossed by an intrusion.

For the portion of the release that is dominated by the effects of damaged packages within a drift, the packages that are damaged but not actually erupted, yes, backfill would I believe reduce that. But as presently modelled, perhaps not all that much. Most of the damage occurs when the package is quite close to the point of intrusion.

This dose out here is driven by material that's actually erupted, our hypothetical conduit to the surface, and I don't actually see in here alternatives that would do much there. Radically changing package spacing, spreading the packages out enormously might reduce that. But there is still I believe some conservatism in that, perhaps particularly in the treatment of the air mass loading, the dustiness in the atmosphere following the event. We calculate these assuming that the air is as dusty forever after the event as it is in the first decade, and most of this dose comes from inhalation of the suspended particles.

If we were to put a time dependent dose conversion factor in that accommodated stabilization of soil, we haven't done that, I expect you would see that come down. I don't know how much.

PARIZEK: That would be a more realistic case really.

SWIFT: Yes, there's more realism there, but that's not an engineering answer to your question.

PARIZEK: Yes, that's a different one. But it means that that analysis could be made, could be a little bit more realistic, in other words.

SWIFT: Yeah.

PARIZEK: One other question with regard to the one-off analyses. Are there times when you take one thing out at a time where you miss the interaction between two variables, or the rest of the variables that are still in the mix? I think the Board has kind of explored this in the past, but it's sort of like the human performance. If you pull out organs one by one from me, you'll get a sense of what the organ's value was. But there may be times when there's kind of a, you know, two things react in a way that's delayed in a way that surprises you, but may kind of confuse the issue. I see value in the one-off method, but are there times that it misleads you somewhat as to how the interaction of that variable affects the others that remain, or several others that remain?

SWIFT: Well, I share your concern. I have more faith in the full system analysis than I do in the one-off comparison for just that reason. It's difficult to know what you've missed when you've only changed one component. If you knew that was the only component that was going to be changed, that would be fine. But we don't know that.

So, yeah, the value of doing them is that it gives you insight into what might matter in the next one. But go ahead and look at the combination models. That's where the answer is.

PARIZEK: Which is really what the total system analysis does for you.

SWIFT: Yeah.

PARIZEK: Thank you.

BULLEN: Bullen, Board.

Just a quick question on Number 13, if you'd go back to that?

You made a comparison between the peak doses, and I guess the question I have is have you done calculations beyond a million years to determine if that's actually the peak?

SWIFT: No.

BULLEN: And do you think it is? Or your crystal ball doesn't tell you anything right now?

SWIFT: My crystal ball doesn't answer that one.

BULLEN: Okay. Well, it might be something that would be interesting to know, because the Board--the Yucca Mountain Standard, the National Academy, is one of the things that drove you to go to peak doses, and so I just was curious as to whether or not you thought you might be there.

We have a couple questions from Board Staff. John Pye?

PYE: John Pye, Staff. Could you clarify how the project defines low temperature operating mode? On Slide 4, the second bullet read waste package temperatures below 85 degrees centigrade. You added the word average. In reviewing the SSPA, in order to assess the results, I see, for example, a string of waste packages from center to the edge of the repository. I see two-thirds of them below 85 degrees. The rest almost approach 96 degrees centigrade. So, how does the project interpret the 85 degrees centigrade criteria?

SWIFT: I'm going to actually pass that question. I didn't see a nod there.

HOWARD: John, it's the average of the waste packages. It's average waste package surface temperature. Yeah, we do have some cases where the peak waste package surface temperature of some waste packages goes above 85 degrees C. We were doing the analyses to look at, you know, the implications of performance for hot versus cold, not set a design not to exceed constraint. So, we thought that having some waste packages that had peak temperatures above 85 was acceptable for the analyses that we were doing. But we were looking at the overall performance of the system, not a fixed temperature limit of a not to exceed per waste package. It wasn't a design constraint. These were post-closure thermal analyses. Does that help?

PYE: Well, you have an average, but do you have a range in mind?

HOWARD: I think that's something that we're going to have to look at as we move forward, you know, what is that range, and what is the window of susceptibility. I mean, you'll see I think Dr. Blink will have some graphs of what we think that window is, and what it means. But it's not to be interpreted as a strict temperature only issue for us. I mean, that's one of the things that I said in my conclusion. It's not an on-off switch. Things don't suddenly go south as soon as you hit 86 degrees. That's not what we're doing.

BULLEN: Bullen, Board. I had one more question from a Board member. Jeff Wong, did you have a question? Oh, two more questions.

WONG: Yes, I have a quick question. Back on 14? I got Volume 2 this morning at 8:30, so I really didn't have enough time to read the whole thing. But I'm trying to under this 95th percentile, 50th and 5. I suppose that there's just the 95th percentile and 5th percentile of all runs. That's not the confidence interval around the mean; is that true?

SWIFT: Yes, exactly. The percentile is simply the percentiles at that time of the realizations that were calculated. So if there were 300 realizations at this time here along the 95th, you know, 95 percent of them would be below that.

WONG: Right. Now, help me understand why--I have two questions. One, in the beginning, that period sort of around 1,000 years, why is it that the 95th percentile, you know, is actually lower than the mean, and in the end, toward the million years, it looks like the 95th percent and the 5th percentile are converging. So that means that would indicate to me that your uncertainty is decreasing, and just intuitively, I would think as you go out a million years, you would know less, you'd be more uncertain in terms of predicting performance. But why is it it appears that it's converging?

SWIFT: At early times, the mean exceeds the 95th because there are relatively few realizations contributing to that mean. It's a strongly skewed distribution, a lot of low numbers, some fairly high ones. So it's quite possible to have a skewed distribution in which the mean is driven by a handful of realizations that are large numbers, and most of them are low numbers, or even zeros. So that's what is happening here at early times. And you see that in other runs also. It's not an uncommon result in large calculations that produce a skewed distribution in outcomes.

I'm sorry I don't have the plot here from TSPA Rev. 0, the same thing. I think that may explain some of this apparent convergence of the summary measures out here. In TSPA Rev. 0, we saw the same thing, although it happened much earlier. A large broad band here, and it had gotten quite narrow by the time it was out there. And what was happening there was that uncertainty in the results was indeed going down because more and more packages had failed. At an intermediate time, you have some packages producing a dose, because they have breached, and others not producing a dose, and you have a broad range from zero to non-zero.

At later and later times, potentially all the packages are going to contribute. It's still not all of them in these results. There are still 12 percent or so that are not contributing, but most of the packages are now contributing to the dose. So our summary measures are starting to converge. And, yes, there is actually less uncertainty in the outcome the later and later you go.

WONG: Thank you.

BULLEN: Final question in this session will be from Norm Christensen.

CHRISTENSEN: Maybe this is more along the lines of a comment than a question. But it has to do with Slide 15. I just wonder what happens when you extend the time frame. You've truncated the time frame here at 100,000 years. What happens to peak dose in these two? Is there a reason for not extending that out? I'm just curious as to what it looks like in that longer time frame. You clearly have higher doses than the base case.

SWIFT: Sure. Sure. That is the answer to the question as to why we haven't run it out longer, is that for the Rev. 0 work, we picked this 50,000 year point because in our Rev.

0 model, the nominal doses were considerably higher already by about 40,000 years, and this is a very computationally intensive calculation. That's the straight answer in the Rev. 0 work.

For this work, basically the same reason. We have the computational power now to go out to 100,000 years. We have not gone further than that with that. There is no reason to believe it would continue to go up. Each individual event produces sort of a pulse of a groundwater dose. It's not like the nominal scenario where packages continue to fail on and on through time. Here, we're getting a bunch of them all failing at one time from an igneous event. 100,000 years later, you're not contributing much to the dose. It's the newer events that are doing it. So, basically, the conclusion that you reach a plateau out here is a logical one.

CHRISTENSEN: This is more of a presentation comment. But a quick glance by somebody not thinking about this carefully might conclude that the best thing that could happen out there would be a volcanic eruption, if in fact one assumes that the baseline curve flattens out there at about 10 to the minus 1, and continues on that line. So, it would be nice to have some sense of that.

SWIFT: These do not include the doses that would have occurred from nominal performance. These are simply the doses from an eruption.

BULLEN: Thank you very much, and my thanks to the speakers. My apologies to the members of the public who asked questions. I will ask those during the public comment period. And to the two staff members who didn't get to ask the questions at all, that's just too bad.

We will now take a ten minute break, and reconvene at 3:15.

(Whereupon, a brief recess was taken.)

BULLEN: In the interest of giving Dr. Bodvarsson enough time to go through his many viewgraphs, not that Bo ever brings too many, I would like to begin the session.

Our next presentation is by Dr. Bo Bodvarsson from Lawrence Berkley National Laboratory. And Bo is going to be the first of our specific issues people after the overview, and I'm thinking Bo is talking about--is it unsaturated? Yes, UZ flow and near-field environment thermally driven coupled process components.

Thank you for putting that up so I could get the title right. Bo, it's all yours.

BODVARSSON: Thank you. Can everybody hear me okay?

Okay, like Dan said, my name is Bo Bodvarsson, Lawrence Berkley Lab. I'm going to talk about both the near field and the UZ, unsaturated zone, activities that we have been doing over the last few months, and talk mainly about thermally driven coupled processes components.

We have a lot of participants, both from the unsaturated zone and from the near field, from various organizations, including LBL, Livermore, Sandia, and others.

These are some of the other participants.

The main objective of this presentation is to talk about some of the recent advances in the UZ and near field studies since TSPA-SR in terms of unquantified uncertainties, and also in terms of a lot of work we did to examine the range of thermal operating modes. Then describe resolution of these uncertainties, and then also describe the use of multiple lines of evidence.

There are two main things I'm going to discuss. One is UZ flow. That means the three-dimensional flow fields in the unsaturated zone, the effect of coupled processes on UZ flow, the effect of various parameters and processes on the UZ flow, as well as coupled processes on a mountain scale. The other one is seepage. I'm going to talk about seepage in terms of ambient seepage, the effect of various modification improvements we have made in seepage models, and then talk about the effects of coupled processes on seepage, including TH, THM and THC effects.

So, first, this discusses the UZ flow, and we talk about the unquantified uncertainties. And there are two areas here I'm going to concentrate on. One is lateral flow in the PTn, and the second one is expanded 3-D flow fields. And this is in response to the footprint, because if you change the thermal operating mode, you're going to change the footprint. And we examined the 3-D flow fields and the effects of the footprint on the 3-D flow fields.

Then we take the UZ flow and investigate the effect of thermally driven coupled processes on a mountain scale, because this is UZ flow, in terms of thermal hydrologic effects, including the effect of lithophysae on thermal properties, that is, the big holes and how they effect thermal conductivity and incapacity, and examine the range of thermal operating modes, the high and the low, and the effect of thermal hydrology on flow.

We continue with the effect of thermal hydrologic chemical effects on the UZ flow. This is a new model that has been developed recently that addresses various processes on a mountain scale, including alteration in the PTn in the vitrophyre, in the zeolitic rocks, as well as large scale mountain effects in the repository.

Finally, with respect to this first component, UZ flow, we talk about thermal hydrological mechanical effects, and this is another new model that addresses multi-phase flow and calculates stress, the effect of stress on permeability, and consequently, the effect of permeability on the three-dimensional flow fields.

The next slide is about seepage. We go through the same thing we just went through for UZ flow. We talk about unquantified uncertainties. And here, we concentrate on a new seepage model for the lower lithophysal. We talk about flow focusing and how we have improved our formulation and theoretical basis for flow focusing. And we talk about drift degradation and how we have improved our analysis in terms of the drift degradation.

Then we go into again the thermally driven coupled processes on a drift scale now, because this refers to seepage, not on a mountain scale anymore. We talk about TH, THC and THM models, including lithophysal properties. We examined the range of thermal operating modes for all of these models, and then we talked about THM, a fully coupled THM continuum model that we haven't had before. This is what we're going to talk about in general.

Then we're going to discuss uncertainty during the talk in terms of uncertainties in conceptual models, parameters and input data, and how we reduce these uncertainties through analysis of new data, improved experiments, sensitivity analyses, all multiple lines of evidence.

In terms of multiple lines of evidence, we gain confidence throughout this talk with various examples, including natural analogues, laboratory and field experiments, detailed sensitivity studies using process models, and then comparison with alternative approaches, different approaches, different modelling approaches, experimental approaches, whatever.

After all of this, we start with the first topic, unquantified uncertainties on UZ flow. The first topic regards lateral flow in the PTn. In the mid 1980s when Montazer and Wilson did their conceptual model of lateral flow in the PTn, they thought it would be significant. Since then, various model studies have difference in their conclusions regarding lateral flow.

Now our conclusion is that this is highly dependent on the numerical model, and you have to use a very fine gridding to actually catch lateral flow. And we believe that this is a very significant effect to the extent that for infiltration rates that we have currently, 5 to 10 millimeters per year, you have lateral flow on the order of 25 to 30 percent that goes into faults due to lateral flow in the PTn and, therefore, does not go through the repository horizon. Therefore, this is good. This is positive for performance because it leads to reduced percolation flux, therefore, reduced seepage.

Currently, we are not taking this into account in the TSPA because this is a conservative assumption, but perhaps we will in future TSPAs.

And I'm going to put this viewgraph up here that I'm going to refer to from time to time. It basically shows a little variability of what Peter and Rob showed. All these things I'm going to talk about, what is in TSPA currently, future plans, and comments. So you can look as we go. This is in, this is not in, and why not.

Now, I'm going to talk a little bit about 3-D flow fields. Since our interest is to investigate what happens to various temperature ranges, we also have to consider not only the temperature, but also all the parts of the models. And, similarly, if you have lower temperature operating modes, you're going to have to spread the base further apart and, therefore, the repository footprint is going to be larger, and we have to investigate how does that effect our overall dose and overall impact.

Thus, what we show here is simply the repository boundary as we had it in our Rev. 00 approach. And then here we have an extension to the south in case we need additional footprint because of the lower temperature operating modes. We did some extensive studies and evaluation of this in the reports, the SSPA report, and did three-dimensional simulation studies, and basically conclude that for the 3-D flow fields, this is a fairly small effect.

However, you'll note here in the lower one, that the lower lithophysal is predominant repository rock in the upper part here, and in the lower part, the lower non-lithophysal rock becomes very important. And, therefore, this may have an effect on seepage.

With respect to UZ flow under ambient conditions, we are looking at various multiple lines of evidence, including certainly Rainier mesa that all of you are familiar with, and also percolation flux studies that we have been conducting over many years using geochemical and temperature data that agree very well with our current estimates of percolation flux, as well as infiltration.

Now, after we talked about the ambient effect on UZ flow, we want to talk about coupled processes effects on UZ flow in terms of TH, THC and THM.

This is a result for a TH mountain scale model that we have been developing over quite a few years. The reason for this model are various. Number one, we want to look at how much does the temperature in the PTn increase because of alteration potential in the PTn. How much does the temperature in the zeolitic rock below the repository increase because of perhaps reduced sorption if the temperature goes too high? What happens to the perched water? Does it boil off at the various thermal conditions, et cetera, et cetera. So there are various reasons for doing this.

This slide here shows a given location in the repository, and shows the time evolution of temperature. So, in the beginning, you just simply have the geothermal gradient. Then you start to heat and you get this profile, this profile, and you get hotter and hotter. This is for the high temperature case.

And you see you get boiling conditions in this case close to the repository, the temperature exceeding 100 degrees, and less temperature everywhere else. When we use this model and compare with and without lithophysal cavities, we find two things. Number one, the most important thing is this does not have significant effect on our 3-D flow fields. We, therefore, don't have to include the thermal effects in terms of TH on 3-D flow fields, because they are very similar for both the high temperature and the low temperature case.

Second, and of course importantly is that we get the boiling zone and dry-out close to the drift. That has implication for other aspects, such as seepage.

For the lower temperature case, you only get up to from 70 or 80 degrees or a little higher close to the drift, and you see everywhere temperatures are less. Again, you have less vaporization and condensation. Some of it occurs because when you increase temperature, the partial pressure of water in the gas space is going to increase and, therefore, you vaporize. But it has very little effect on the mountain scale three-dimensional flow.

So, again, we use this model over all temperature ranges now to screen out the effects of thermal hydrology on the 3-D flow fields. So it's independent on the temperature we use.

now we go into THC mountain scale coupled processes, and that means we have the chemistry, we have the temperature, we have the hydrology, and now we have the chemistry. That adds further complications, potential for alteration in the PTn, potential for alterations in the zeolitic rocks, perched water vaporization, alteration in the basal vitrophyre, mineral deposition dissolution, et cetera, et cetera.

This is the newly developed model of THC mountain scale effects, and this specific model looks at changes in porosity and mineralogy in a cross section over the entire mountain, vertical extent from the ground surface, all the way to the water table, and the lateral extent all the way through the repository.

The conclusion we reach for this model, and we run it for thousands and thousands of years, and this happens to be the results after some 5,000 years, is the basically the fracture porosity changes, consistent with our drift scale modelling previously, are small. The fracture porosity only changes by about 1 percent of the initial value. And since the fracture porosity is about 1 percent to start with, you only go from .01 to .0099 all the way. So it's a very minor effect, and does not significantly affect either flow or transport.

We get, however, other effects in localized areas of the repository, and you see here a little deposition of amorphous silica in the northern part of the repository close to a fault. Here, we have more vaporization occurring, and the vaporization causes mineralization of amorphous silica in this region here. And we have gas convection also occurring in this region. This is a very localized effect and does not significantly affect the repository performance.

The major effect with respect to mineral changes on a mountain scale occur in the zeolitic rocks down here below, where temperature, just truly temperate effect causes zeolites to dissolve to form feldspars. And this increases the matrix porosity somewhat of the zeolites that may cause increased permeability, and perhaps that would lead to better sorption characteristics of the zeolites. But, again, these changes are fairly minor.

However, all of these changes are not expected to be very reversible, because they're very slow kinetics, so they may stay in the system for tens of thousands of years.

Same model. Again, this is the high temperature case, the higher temperature case, and since we find very little significant changes for the higher temperature, obviously you're not going to get a lot of changes for the lower temperature either. So now we look at the water and gas chemistry, which is extremely important of course for water seepage and chemistry going into the drifts, and the gas chemistry in the drifts and its effect on the corrosion rates, et cetera.

You have three slides here. First, the top one here shows basically the pH changes after some 1,500 years. You see the CO_2 changes after 1,500 years. And down here at the bottom, you see the chloride, total chlorides in the water.

If you take the middle one first, it's easier to explain that one. Obviously, for the high temperature case, when you start to boil, CO_2 doesn't like to be in the liquid phase, temperature rises. It wants to go in the gas phase. It degasses, goes out through the matrix blocks into the fractures, and you will get an area that is pretty much depleted in in CO_2, or any mass structure of CO_2 is basically gone. It goes and boils off and you have here a region after 1,500 years which is low in CO_2. Because of that, you also get low pH, because the CO_2 is related to the pH, because you have a reaction in the bicarbonates, that interacts with the H plus ion, which is the pH in the ion, and you get basically water and CO_2.

So you get somewhat lower pHs. But if you look at the range, the pHs are very similar, on the order of 7.5 to 8.5, or something like that.

At the bottom here, again, you remember that we have the gas convection and we actually have the evaporation and boiling. That's reflected in the chlorides. You have a little higher chloride content in the water here, because a little bit more has boiled off.

Finally, THM on a mountain scale. This, again, is a new model where we looked at the cross-section. This shows the stress situation, or the changes in stress, and you see here in the repository horizon in the middle non-lithophysal, you have thermally induced increases in stress, some four to five megapascals, which is equivalent to some 40 to 50 bars.

What that says to you is the following. When you look at this table here, you get the ratios of initial permeabilities, the vertical permeabilities, and initial permeability, the horizontal permeabilities, and you see in the repository area, and throughout the TSw, the changes are very small, less than an order of magnitude, and general decrease in permeabilities. And there is more decrease in permeabilities of vertical fractures than horizontal fractures, just simply because the stress on horizontal fractures due to the lithostatic load is more than the stress on vertical fractures and, therefore, it takes less changes in the thermal stress to alter the permeabilities. Still, this is a minor factor on the overall flow and transport.

Mountain scale thermally driven coupled processes multiple lines of evidence. We of course use the single heater test and drift-scale test to verify and validate our studies. We have a large history and a dataset of alteration mineralogy, isotopes, geochemistry and fluid inclusion data that will help up with the THC history of the tuffs. And we have experiments that were conducted in the g-tunnel in Rainier Mesa to verify some of the THM results that we have.

Now, we have finished with UZ flow. Overall conclusion, the improvements that we have made in terms of the PTn model will improve performance. The expansion of the repository footprint doesn't seem to have an effect on the 3-D flow feels, and all the coupled processes effects, TH, THC and THM, are more important locally. Our conclusion, therefore, is you do not have to consider these in the 3-D flow fields.

Now, seepage. Seepage is a local phenomenon and is a drift scale phenomenon, and we want to look at unquantified uncertainties with respect to seepage. The lower lithophysal model, the flow focusing factor, and the drift degradation model first, and then we go into coupled processes.

This is a slide from Stefan Finsterle that shows data from the systematic testing where we actually put a lot of water above the drift, and there's a lot of noise in the measurements of the water going into it because of the balance. This is what we get in terms of seepage. And you see here seepage is generally about five units versus 30 units of applied water. That means one-sixth seep on the average.

This is very high flow rates, much higher than the ambient conditions in the mountain, because we can't test under ambient conditions because that's too low a flow rate, obviously.

We did multiple scenarios. These are multi-color simulations here that Stefan did, multiple realizations, with different results because it has different permeability structures and different calibration structures. That results in a mean value, which is given here, and this is basically the data that he obtains with the seepage data that was very good.

The good things about this is the following. The lower lithophysal rocks show much less seepage than the middle non-lithophysal rocks, even though those middle non-lithophysal rocks didn't show a lot of seepage. But still, the small permeability fracture structure in the lower lithophysal rock seems to increase the capillary barrier capacity of that rock, which is very positive for performance.

The second important thing here for performance issues in TSPA is that Stefan provides an uncertainty band that Mike Wilson and people at Sandia and elsewhere can use to sample this distribution to show a range of uncertainty in the simulations of seepage for the lower lithophysal.

This is included in TSPA, which is shown right here. Included in TSPA. And as you see here, most of these thermally driven effects on the 3-D flow fields are not included, because we don't need them. They're used to screen out the flow fields.

This is multiple lines of evidence developed by John Stuckless of the Survey. He gets to travel to various parts of the country, which is nice, various parts of the world. This happens to be in Egypt, where he looked at some tombs, and this is 3,000 to 3,500 years old, very, very old, and basically what he reports, they are very well preserved. You see no seepage, just some areas of spallation and plaster from the walls.

Flow focusing. Second item on unquantified uncertainty. Flow focusing basically refers to the fact that we don't know where the discrete flow paths are in the mountain. We think flow coming from the PTn is fairly uniform because of the porous nature of the PTn. But then it develops into some kind of weeps or preferred flow paths. This model uses a heterogeneous permeability field to determine at the bottom of the repository, after flowing through 150 meters of Topopah Spring fractures, how many of these weeps come out of here, and how much is the flow focusing.

In TSPA-SR Rev. 0, we used flow focusing up to 60 times. That means each one of these weeps might have flow rate 60 times the average, which is a lot, because that affects seepage, increases seepage suddenly at those locations.

What we find interestingly when we do this exercise, and that's summarized in this graph here, this is the frequency versus normalized flux graph. This is one. That means that on the average, 14 percent of the flow paths coming out here have a flux equal to the average flux that is applied uniformly on top. Only 2 percent have 2 1/2 times that. So this clearly showed that for these conditions and this study, you are nowhere close to 60 times that's used in the TSPA, and you are more close to three, four or five times, that's the maximum flow focusing factor. This has been incorporated into TSPA.

Drift degradation effects on seepage. Dwayne Kicker, in his EPSA MR, did a more drastic realistic evaluation of possible rock falls and possible changes in the geometry of a drift. If you change the geometry of a drift and make a big hole in the ceiling, that may significantly affect the seepage, one would expect.

These results show the results of our simulation regarding seepage. This is percolation flux, which is the flux through the whole mountain, and this is the seepage rate for fracture. The base case is shown here, that is, without the changes in the drift, and these are the most drastic possible results, and 75 percentile of the most drastic results of the drift changes.

What we conclude from here is that there is no impact pretty much on the seepage threshold. All the graphs go pretty much to the same seepage threshold, 500 or 1,000. But there are some differences in the seepage, but these are very, very small effects. So overall, we conclude, as before, that the changes in the drift geometry do not have significant effects on seepage based on Dwayne Kicker's analysis.

Multiple lines of evidence. Seepage and seepage barriers, both natural and man-made underground openings give us all kinds of opportunities to verify our experimental and model results. We have archeological cave sites, Egyptian tombs, lithophysal cavities where the Survey is looking at the calcites at the bottom lithophysal cavities, excavated tunnels at Rainier Mesa, ongoing seepage tests at Yucca Mountain. We believe that all this information and all this wealth of information supports our notion from all of our models that seepage is very little at Yucca Mountain, and only occurs under specific conditions and very high flow rates, or percolation flux rates.

The flow focusing that we just discussed, and fast flow paths, some of the Chlorine-36 bomb pulse signals, or other fast flow path indicators after we have resolved them through the comparative studies that are ongoing now, will give us confidence in these results.

Final topic, thermally driven coupled processes effects on seepage. We going to talk about TH, THC and THM models in terms of lithophysal cavities, in terms of thermal operating modes, and in terms of this fully coupled model.

We're going to start off with the TH drift scale for a high temperate mode. The main concern here, as we have discussed in many of the NWTRB meetings, are that when you boil water, it condenses over the drifts, and it's going to go back and seep into the openings and cause, of course major problems like that.

We have done lots of studies, both at Livermore and at Berkley and at Sandia, all three locations, to look at this problem from various points of view, including very rigorous numerical simulations with heterogeneous flow fields and analytical expressions and approximations, and others.

What we are showing you here on the right side are the flux of water 5 meters above the drift, just for comparison. You remember we had a lot of discussion about this 5 meters above the drift issue that we used before in past TSPAs. And you will see just what we showed you before. You're going to get a lot of fluxes for the high temperature case 5 meters above the drift, because of the capillary suction that attracts water towards the drifts.

The bottom thing here shows, though, for a 5 meter drift that has a radius of 2 1/2 meters, none of that water seeps into the drifts. All of our simulation studies for the high temperature case show no seepage of water going into the drifts. Cliff Ho of Sandia did a very conservative episodic flow study in addition to these numerical studies, and showed that there was potential for some seepage under the high temperature regime, and that is what we conservatively use in the TSPA. So it's included in TSPA, but very little seepage occurs.

Of course, that is because during the high temperature regime, you boil off a lot of the water in the matrix. This happens to be the matrix and the fractures, and you get this big dry-out zone, and whatever water tries to get through here, boils up and goes away again.

Now, let's look at the low temperature case. In the low temperature case, the same processes do not occur. We do not get boiling around the drifts. The matrix block saturations remain practically intact at 80 percent saturation.

However, we get some dry-out in the fractures below the drifts simply because when you increase temperature, the solubility of--you know, the partial pressure of water in the gas space increase, just as a fraction of temperature. That causes just simply drainage in the fracture system below here, as well as the shadow zone effect, because the shadow zone makes water want to go around here.

So, you see here a large, fairly large zone here after 500 years of dry rocks in the fractures below the drifts. Jim Houseworth in his talk tomorrow about transport will give you indications for transport issues because of this dry-out zone in the fractures.

When we do seepage studies, however, with the low temperature, we see almost no impact on seepage. It's as if it's ambient with the low temperature case, which makes sense because water is basically close to the drift. Temperature of water is a little higher, but the seepage potential is pretty much the same as in the ambient case.

We directly used, therefore, the ambient seepage model for TSPA calculations, and this is slightly conservative because the seepage under those conditions is a little bit less than in the ambient case.

So, to conclude or summarize this effect, because this is fairly important, we have done multiple simulations of various locations with different approximations, with heterogeneity, and this confirms to us that seepage is going to be much less during the thermal period for the high temperature case, which is good for performance, because it's going to boil off, less seepage.

For the low temperature case, it will not help with the seepage issue, and you get pretty much the same seepage as in the ambient case.

Now we're going to look at THC coupled processes at the drift scale. We're going to look at two things now, precipitation dissolution in the fracture system, and effect on permeability; secondly, the chemistry of water and gases entering the drifts for both the low and the high temperature operating modes.

This show a permeability reduction after about 20,000 years of simulations in the drift scale. Here's the drift. Here's our model. This is the heterogeneous permeability field that Eric Sonenthal, which is in the audience, applied. This is the permeability changes after 20,000 years of simulations. You see localized effects here that are related to the low permeability areas.

So, basically, there are very small changes that have practically no impact on performance. The permeabilities in the small permeability areas go a little bit down, and that's about it. The impact on performance is very, very low.

This shows the chemistry for the high and the low temperature case where we compare those two. We show here the CO_2 concentrations, show here the temperature. We show here the saturations. This is up close to the crown of the drift.

Starting with the temperature, this is the temperature. This is the temperature for the low temperature case. We ventilate for 300 years. After that, the temperature goes up to some 70, 80 degrees at the crown. Ambient stays the same, obviously.

For the high temperature case, you ventilate for 50 years. The temperature goes up to some 85, 90 degrees. Then you quit ventilating, and it goes up to some 150 degrees at the drift crown, and then starts to go down with time.

Obviously, in this case, nothing happens in terms of CO_2--concentrations or positive pressures, until you start boiling. You have a little changed reduction in the CO_2 content of the fractures here in the beginning, just because we dry out the fracture with the increase in the temperatures. Then what happens is at this point here, we quit ventilating, and then you start boiling the rock matrix, and all the CO_2 goes into the gas phase, into the fractures, and that increases the CO_2 content for a limited time, some 50 years. After that, you boil some more, there's no more CO_2, it all gets diluted, and the concentration of CO_2 goes practically to zero, because you continue to boil, CO_2 is gone, and you practically have pure water steam there, no CO_2 present.

After that, when you start to cool down some more, you start to have the effect of, of course, the infiltration carrying some CO_2 with it in the liquid phase, and you start to build up the CO_2 from there. This is also shown in the saturation. Nothing much happens in the ambient case. This is climate change here after 600 years and 2,000 years, and then you have the dry-out period and the high temperature regime.

Now, if you look at some other components, the pH, you look at the chlorides and you look at the fluorides, fluorides is very important for corrosion of the waste packages, this is water coming in through the drifts, suddenly if you look at the chlorides here, when you start to boil, when you start to heat up some of the water, the fractures dry out and, therefore, the concentration of chlorides increases slightly. The same thing with fluorides, it increases slightly, but then you also have the dissolution of fluoride from the rock, which also increases it slightly.

The pH drops down here pretty much in the beginning to just the temperature effect, degassing of CO_2, so it doesn't stay up 8.2 or 8.3 like the ambient case, it drops down because you start to degas CO_2 right away, and the reaction of the bicarbonates and all of that, reduces the pH. And then you have very similar effects in the low and the high temperature, except of course during the dry-out period, you can't define pH because there's no liquid phase present. There is only steam present.

Now, finally, this is the last topic, we go into the THM effects on the drift scale, and the effects on seepage. As all of you know, there are several important things with respect to THM effects. The first one, of course, is the normal stress changes. You basically heat up the rock mass, you have expansion of the rock mass. You decrease the aperture because of expansion of the rock mass into the fractures. That's the normal stress, the normal displacement and reduction in permeability due to heat.

The other one, of course, is the sheer effects that Steve Blair of Livermore has been concerned with where you actually have kind of like a rapid sheer that actually increases the permeability because you're kind of rubbing the fractures together and generally results in increased apertures. Therefore, you might increase permeabilities there.

These are some of the results from Steve Blair of his work using a discrete fracture element model, and he concludes that he gets fairly large permeability increases close to the drift due to sheer, about one order of magnitude increase in permeability, and of course it makes sense as close to the drifts, because that's where you expect to see most of the stress changes. Away from the drift, he has lower permeability changes, mostly reductions in permeability.

We also have done a continuum model rather than the discrete fracture model, and that is close to the drift. This is actually a calibration of this model versus the niche data, because we know that when we drill out the niches, you increase the permeability on here at the top of the drift, which is shown here, which is the red thing, increased permeability, and you actually decrease the horizontal permeabilities and increase vertical permeabilities at the site of the drifts. This is measured in the niche data, and this is what we use to calibrate the continuum model.

We also calibrated the continuum model against the displacements from the drift scale thermal tests and permeability changes due to air K in the drift scale thermal tests. But I'm not going to go into detail with that. I'm just going to show you the results, which is shown in the next slide.

These are results after only about ten years. What you see after ten years, you see generally a permeability reduction closest to the drifts, and away from the drift going all the way close to the middle of the pillars, both for the high temperature and low temperature case. The results are fairly similar. Permeabilities generally go down, except for the sheer effect close to the drift that Steve Blair was worried about, and the changes are only less than an order of magnitude for this model, which is based on calibration against the niche data and the drift scale test data.

When you go to 1,000 years, some of these effects continue, but these are reversible effects. The sheer effects on permeability are not reversible. You will continue to have the increased small level of permeability close to the drift, but the normal stress caused by the temperature, after it cools down, it's going to open up the fractures again and become reversible.

After 1,000 years, in this lower lithophysal, you see only a permeability reduction of a factor of two in the lower lithophysal, which is very, very small and has no impact on seepage. In the middle non-lithophysal, you have more of an effect, about an order of magnitude. And why is that? The reason is two-fold.

Number one, the permeability of the middle non-lithophysal is lower than the lower lithophysal and, therefore, you have smaller apertures of the fractures and, therefore, smaller changes and stresses, and smaller aperture change causes more effect on permeability, A.

B, since this is shallower, the total stress overburden is less and, therefore, you need less thermal stress to make permeability changes. But still, these are about an order of magnitude changes in permeability reduction, and has very, very little effect, we believe, on TSPA effects.

We may consider, considering both the--the effects, to run it through TSPA, the next iteration of TSPA, just to make sure that we are not wrong when we think that this is not very important.

So, we have gained confidence through the niche studies and drift scale studies. Multiple lines of evidence include the NTS THM experiments, underground testing at Stripa, and geothermal analogues.

Multiple lines of evidence for this whole discussion of seepage. TH, drainage of water outside an above-boiling region of rock is corroborated by the drift scale test observations. Modelling has been verified through simulation of tuff dissolution and fracture precipitation experiments that have been done both at Livermore and at Berkley. Active geothermal and fossil hydrothermal systems, they have verified all the processes that we have talked about, all the minerals that we have seen deposited, et cetera. The heated block at Rainier Mesa has shown very similar effects on excavation and stress and temperature changes. That's what we are seeing in our simulations. And the Stripa experiment has provided some information about the single fracture closure on permeability.

And then finally, to conclude, very generically, we have developed a lot of analysis and models for unsaturated zone flow and seepage under ambient conditions, near field effects, coupled processes models in both mountain scale and drift scale to address important issues raised by the Nuclear Waste Technical Review Board, as well as our own program, to evaluate the effects of coupled processes on three-dimensional flow, seepage and other areas.

We have gained confidence through the different conceptualizations and different approaches that compliment each other in many ways. We have evaluated unquantified uncertainties in flow and seepage. We have used new information to refine quantified uncertainty. We have broadened the conceptual basis with multiple lines of evidence. And we have extended the thermally driven coupled process simulation over a range of operating modes from some 80 degrees centigrade, to 150 or high degrees centigrade close to the drift wall. And we have developed new models to examine mountain scale effects, including THC and THM models.

That summarizes my talk.

BULLEN: Thank you, Bo. Questions from the Board? Parizek?

PARIZEK: Parizek, Board.

At page 13, I guess you showed an extension in the event of increasing the footprint size, and you went south. I think at some briefings, we've seen a footprint going north, and there was also a possibility of a footprint going west, I guess in the Jet Ridge to the west of a fault.

Is there any reason why you went south, or is that current thinking?

BODVARSSON: Well, a couple of answers to that. You're actually right. The repository footprint was extended about a year ago to the north by some several hundred meters, about 500 meters, or something like that, if I remember correctly. The design has considered options of going down to the south. There are other options that we could have considered also, but this was one option that was on the books I think six months ago to nine months ago, and we decided to go with that one. And we were concerned about the quality of the rock here to the south. We were concerned about the available data to the south here, and we were concerned about the geological information and flow and transport information. That's one of the reasons.

HOWARD: Just to confirm what Bo has said, the footprint that extends to the south, there's actually a layout of that in the Science and Engineering Report that shows that expansion area. There's other footprints that you've seen.

There are probably footprints associated with the EIS calculations where we also went to--looked at areas in the east and the west. So there is real estate out there we've looked at. This is what we asked Bo to look at for this particular study, was going to the south. That was the current think, but it certainly will change as we--

PARIZEK: We had some observations of going north, and whether or not the one level rise associated with the pluvial conditions could cause maybe saturation. There's that issue if you do go north, you had this question of adequate separation between the footprint in that direction. I didn't know whether that was a reason that maybe it's been dropped out of this diagram.

BODVARSSON: You mean the steep hydraulic gradient?

PARIZEK: Right.

BODVARSSON: We looked at that in great detail, and I think some of that is in the report, and it basically concludes that under the conditions we assume, and diverse climate conditions, we are still some 100 meters of distance between the repository and the potential water table rise in the north.

PARIZEK: That would be the other northern extension. Now, to the south, we also had I think Chlorine 36 and also a concern about tritium. So the probability distribution to the south would be well known, or not so well known? Is that an area where you really have much data?

BODVARSSON: No, you're right. There are just a few boreholes to the south. The block itself is not as well intact as this block here. There are some faults around here, and there's also we think it might be a little bit more fractured there. So the question arises also what is the Chlorine 36, we didn't see any in the south, but we believe there's some tritium there, though. What does that mean? My belief still is that it's possible flow paths--be at whatever, are rather immaterial. This is less than 1 percent for the total flow. We're always going to have flow, fast flow paths, everywhere. With respect to dose, it doesn't do anything for you.

PARIZEK: Okay. You showed I think on Page 19, there was some cement development around the faults to the north, I guess it was that middle diagram.

BODVARSSON: This one here?

PARIZEK: Yes. Is that cementing enough to make any difference to permeability of the fault zone, or that's again a general conclusion that it's trivial?

BODVARSSON: General conclusion is that this is not sufficient to cause any impact on the repository performance.

PARIZEK: Then you have a figure which shows the water distribution going off the PTn and going down through fault zones.

BODVARSSON: Yeah, that's Slide Number 8, is it? No, 11? Yeah, 11.

PARIZEK: There's another diagram, a cross-section diagram that shows perched water below, and the PTn is shedding water off the PTn through the fault zones.

BODVARSSON: You've got to name the number, Dick.

PARIZEK: This diagram was actually looking at some other effects, but previous presentations, you've had the PTn redistribute water, and you alluded to that again today.

BODVARSSON: Yeah.

PARIZEK: Now, if that's been going on through time, particularly in pluvials, shouldn't we find more secondary minerals in the fault zones, the main drains, the vertical drains, than what's seen there? Or is there plenty of calcite in the fault zones to accommodate that increased flow? But you're saying you're distributing the moisture from the PTn, it's going to shed off into the faults and move vertically downward. But we should see some evidence of that through geological time.

BODVARSSON: Certainly if you believe, and I have no reason not to believe studies by Jim Paces and these guys that think that the calcite deposition is a direct function of the total accumulation of flow over long periods of time. So if you would have more flow through the faults, you can argue you see more calcite.

PARIZEK: Right. And do they find that on those principal faults. Your principal drains, you've labelled them here pretty much.

BODVARSSON: I don't remember. Does anybody--

PARIZEK: We've got millions of years to do it, in other words. Every pluvial through time should have given you that increased flux.

BODVARSSON: Right. I personally believe when you take a look at permeabilities in the faults, we have various data that show that lateral permeabilities in faults is some thousands of darcies, huge. Well, there's certainly no calcite to bother you there. When you look at the temperature signature in the dataset also, they also show that there seems to be preferential flow paths down the fault. That indicates--because you get lower temperatures around the faults. That seems to suggest that is flow focusing near fault is actually real also. But all the faults that I have seen have significant permeabilities to them. That means that if there's a lot of calcite there, it's not sealing well.

BULLEN: Sagüés, Board?

SAGÜÉS: I have a couple of questions. The first one has to do with the multiple lines of evidence in Number 28. I wanted to know do you make any tests--or make any quantitative analysis of these issues. For example, say take the example of the Egyptian tombs, is there like, for example, a quantitative comparison, say, if the precipitation rate was so much, and characteristics of the rock was so much, and so on, that there would be some kind of a methodical printing of that, or is it just qualitative indication that--give us a good picture of something--

BODVARSSON: My recollection, and others here can correct me if I'm wrong, is the following. In John Stuckless' report, he visited various tombs and tunnels in different areas. I know many of them he documented precipitation. He documented the rock types. He documented various things that could be used to quantify the situation and make sure it's consistent with our seepage models. But we haven't done that. So the available, to summarize, the available information is in reports, but we haven't used our seepage model on those sites.

Does anybody--well, all of you guys know more about this than I do, so why doesn't one of you speak up? If you don't speak up, I'll pick one. I think that's right.

SAGÜÉS: All right, I guess that we're going to leave it at that. And I have another question on Number 16. When I saw that picture, I think that it's the vast effect that the repository heat will have on the mountain. The temperature gets increased dramatically over hundreds of meters. And I know, of course, that most of these models analyze effects on a local basis certainly, and so on, but how about things like, for example, accommodating thermal expansion on a global basis. Is that incorporated in any of these models?

BODVARSSON: Yeah. That's in the THM model, the thermal hydrological mechanical model. If you go down a few slides? Next. Go through all this. Next. This one. This model, which is a THM model on a mountain scale, and is also on a drift scale, what that does is exactly that, it takes into account the expansion coefficients, the thermal stress associated with the thermal expansion of the rocks. Then it extends the rock into the fractures and calculates the effect of permeability due to this expansion on a global scale over the entire mountain.

SAGÜÉS: How well validated are these models? Are there analogues, for example, from places which you have volcanic intrusion, or something like that? Have these things been used in other systems in a manner that would be considered satisfactory, or is this something like a one of a kind kind of analysis?

BODVARSSON: That's a very good question. My person bias with respect to that is the following. I have a plot that I did years ago, and it's published in a journal article that shows fractured geothermal system and permeability as a function of temperature. Okay? Going from 100 degrees centigrade to 300 degrees centigrade, large temperature range. It shows a pure correlation of reduction in temperature and reduction in permeability over about one order of magnitude over that temperature range for 30 geothermal systems worldwide. Okay?

That, to me, is lots of evidence, at least to myself, that temperature does cause expansion. Temperature does decrease permeability because of expansion. The normal component of stress going into the fractures is probably more important than the sheer component, because generally volume decreases. But the overall conclusion, one order of magnitude over from 100 to 300 degrees is not very important, but it really verifies these results.

SAGÜÉS: Again, now going one step further into the complications, say effects of this kind of massive temperature increase on biological issues, is there anything that can grow inside the mountain? I mean, we're talking here about a system that is very large, in which you take the temperature up like 10, 20, 30 degrees centigrade for a couple thousand years. What has happened in addition to what all of this--considerations. Has anyone looked at that?

BODVARSSON: I am certainly not the right person to answer that. I have one flower in my apartment and it's almost dying. Can anybody here help us?

BULLEN: Bullen, Board.

Why don't we defer and ask if we can get that one answered on the side. I'd like to let Norm ask one, and then I've got one, and then Richard gets the last question, and then we have to move on.

Norm, go ahead.

CHRISTENSEN: Bo, is there empirical evidence to support the drift shadow? I'm particularly interested in whether some of the analogues support the notion of the drift shadow, and particularly the magnitude that it seems to show up in the models.

BODVARSSON: We are in the process now of identifying these analogues for drift shadows, and there's no question in my mind that every dry tunnel that John Stuckless has seen will have a drift shadow, because of course if water only goes around, you will see a drift shadow. Will it have effect on performance, as our models show, can only be verified by actually experiment. I am totally with you there. There has been--we owe it to DOE, our report by the end of this fiscal year with our shadow zone studies that Jim Houseworth and others have been doing, where we are going to outline potential analog sites and testing avenues to pursue this issue. At this time, I couldn't tell you the most promising one. In two or three months, we will have that report to the Department of Energy.

CHRISTENSEN: There will be something on this?

BODVARSSON: Yes.

BULLEN: Bullen, Board. I've got a note from Parizek, and I just have a couple of quick questions, and then I may defer to John Pye, who asked a question that I'm not sure I can exactly repeat.

If you look at Figures 16 and 17, I have a little bit of question about the high temperature operating mode and the low temperature operating mode and the definition thereof. If you look at the previous one, 16, the high temperature operating mode which you have here, it looks like a 1.35 kilowatt per meter load with 50 years of ventilation?

BODVARSSON: Yeah.

BULLEN: And the low is the same line load with 300 years of ventilation?

BODVARSSON: Yeah. 80 percent ventilation.

BULLEN: Yeah. Why do you need a bigger footprint? It's the same line load.

BODVARSSON: It's the same line load. Why do we need the bigger footprint? I don't remember that. Do you remember that, Rob?

BLINK: Jim Blink, Livermore. The loan load for the low temperature operating mode is 1.13 kilowatts per meter. That's a typo.

BULLEN: Okay. So that's just a typo? Okay, never mind.

John Pye, would you please ask your last question?

And I'll give you the last--

PYE: It's the THM analysis of the data I see associated with the dataset based on reverse modelling. There is a sparsity I think of thermal mechanical data. So how do you combine the datasets that we have on the constitutive basis, and what uncertainty, is that introduced into the models?

BODVARSSON: I guess if I understand correctly, John, you are concerned about the limited data for thermal mechanical properties, and how can we trust the models if you have such limited data?

PYE: Yes.

BODVARSSON: My answer is the following. We have an extremely important and good test going, which is the drift scale test, that has heated a huge amount of rock over--a huge volume of rock over years and years and years. We have a very effective test in the single heater test that also heated a large volume of rock. Both of these tests, we have permeability changes, air permeability, permeabilities of fractures that change by less than an order of magnitude, a factor of two generally. We also see that they are reversible in the single heater test, and we expect the same in the drift scale test.

So all experimental tests that we have verify what we have seen in the model. We then use geothermal analogues, the data I was just discussing with this gentleman there, that shows the geothermal analogues, the effect to be within an order of magnitude. And, therefore, we think we have a fairly good basis for what we have concluded so far.

Do we ever have enough data? No.

PYE: What about the constitutive basis between thermal hydrological and mechanical properties, how do you pull those together to combine datasets?

BODVARSSON: Well, to me, the best way--you can do constitutive basis by various means. One is theoretical development, one is laboratory experiments, and the third one is actually looking at the data in the field through a large scale test, or through geothermal analogues over thousands and thousands of years. The theoretical basis I put down here. Lab experiments I put here. Field experiments and geothermal analogues I put way up here. So I think since we rely on this up here, we have a much better foundation for any theoretical work or lab work.

BULLEN: Thank you, Bo. I'm going to have to ask that we call the end of this question session and move on to our final presentation of the day, so that we can allow time for public comment.

The final presentation is by Robert MacKinnon of Sandia National Laboratory. He is the manager of the Engineered Barrier System Department in the Science and Analysis organization for the Manager and Operations

Contractor of Bechtel SAIC. Bob?

MACKINNON: Good afternoon. I'm Robert MacKinnon. I'm with Sandia National Laboratories. I'm the EBS Department Manager.

Before I begin my presentation, I'd like to acknowledge the EBS staff and the hard work and long hours that they put into completing their components of the SSPA during the past several months. And, in particular, I would like to acknowledge some of the lead authors for the various sections, in particular, Jim Blink for Section 5 on the in-drift thermal hydrology; Jim Nowak and Darren Jolle on Section 6 on the in-drift chemical environment; John Case for Section 8 on the EBS flow; and Mike Gross, Section 10 for EBS transport.

The outline of my presentation is as follows. First, I will give an overview of EBS system environments, basically outline key processes in the EBS environment, and relate those to work that we've completed for the SSPA. I'll then describe the individual EBS process components, which include the thermal hydrologic environment, the chemical environment and flow and transport.

My goal here will be to summarize the new model improvements in the supplemental TSPA that quantify important uncertainties. I'll then summarize unquantified uncertainties that we have evaluated. I'll provide some illustrative results and the effects of thermal operating mode on model output parameters. I'll outline multiple lines of evidence. And during the course of my presentation, I'll refer to some ongoing work when it's relevant, and then I'll conclude the presentation with a summary.

This slide or table, portion of the table that's extracted from the table that Rob Howard presented in his presentation, the red font denotes sections and topics that I will discuss this afternoon. Section 5 refer to the thermal hydrology section. Section 6 will be the EBS chemical environment section of my talk. Section 8 are discussed in the EBS flow section. And Section 10 covers the EBS transport processes.

There are a number of important processes in the EBS environment. We've done a substantial amount of work the past three or four months quantifying uncertainties, improving our technical basis and our understanding of these processes. These include the thermal loading and ventilation that determine the heat energy that's input into the EBS and the surrounding host rock. We've made a number of model improvements on our multi-scale model. We are currently doing pretest ventilation predictions for quarter scale ventilation tests that will be initiated here this summer.

We've done some work in natural convection. Natural convection arises from different temperature differences between EBS components. We're currently doing three-dimensional computational fluid dynamics analyses and pretest predictions in support of convection experiments, quarter scale, that we have planned to initiate this summer also.

The incoming water and gas compositions into the EBS provide the boundary conditions for chemical environment. Those compositions are uncertain and those are provided to us by the near-field environment. Dr. Bodvarsson addressed some of the issues and uncertainties associated with the incoming water. In the supplemental TSPA, we do now account for those uncertainties in the incoming compositions, and we propagate those through the in-drift chemistry model.

The chemical interactions inside the EBS determine the chemical conditions in the invert that control the solubility of radionuclides and the stability of colloids. We've made improvements in that model, particularly in our database, that allow us to calculate chemical conditions for concentrated solutions.

Water flow through the EBS is primarily controlled by drip shield and waste package degradation. We've improved our models for calculating flux through the drip shields and waste packages. We've reduced conservatism there and incorporated some key uncertainties.

Waste form degradation is a source term for radionuclide transport. We've made a number of improvements in the area of transport, particularly inside the package. We now account for radionuclide diffusion in the package. We also account for radionuclide sorption onto in-package corrosion products.

Rock fall is another process that impacts the environment. I will not discuss this issue further in my presentation, but we have completed a number of sensitivity analyses, in-drift degradation for the SSPA, and they're documented in Section 5.

This slide summarizes the key inputs to our thermal hydrologic model, and the key outputs. We get inputs from the unsaturated zone, subsurface design, waste package design. The key outputs from this model are relative humidity and temperatures at the waste package surfaces, drip shield surfaces and in the invert. Water flow rate, water saturation, and water evaporation in the invert, we use those for our chemistry calculations in the invert. Water evaporation rate at the drip shield/waste package, we now include evaporation of seepage when it hits the drip shield. We provide percolation flux to the unsaturated zone flow seepage model.

This slide summarizes the differences between the TSPA-SR thermal hydrologic analysis, and the analyses that we completed for the SSPA. We completed a high temperature operating mode and a low temperature operating mode analysis.

The main difference between our thermal hydrologic model for the TSPA-SR and the supplemental model is that we've updated the estimate of thermal conductivity for the lithophysal hydrogeologic units.

In addition, we've addressed several uncertainties that have been identified and evaluated using our submodels from the TH multi-scale model.

This slide summarizes some of the key uncertainties that we have evaluated. We've categorized them into three classes; model uncertainties, process uncertainties and input data uncertainties. I will not discuss most of these in the presentation this afternoon. But I will show you some results, particularly the sensitivity thermal hydrologic performance to thermal conductivity.

I apologize for the orientation of this slide. This is an attempt to show sensitivity of in-drift TH performance to various uncertainties, and Jim Blink in his talk tomorrow afternoon will go into more detail on this slide. I just want to focus in on a couple of uncertainties. What we have on the axis here is variation in peak temperature from the high temperature and low temperature base case. So, right here is zero degrees. At zero degrees, that indicates the peak drip shield temperature calculated and the low temperature and the high temperature operating mode.

Let's look at thermal conductivity. We varied thermal conductivity from this maximum value for the wet thermal K of 2.01 watts per meter K, up to 1.13 watts per meter K. The peak temperature on the drip shield varied by almost 80 degrees. That's in the high temperature operating mode as indicated by the red bar. The low temperature operating mode is indicated by the blue bar, and you can see that the peak temperatures calculated with our multi-scale model are less sensitive in the lower temperature operating mode than they are in the high temperature operating mode.

And you can look across the slide and you can see that the two significant uncertainties are lithophysal porosity and thermal conductivity, and their effects are almost--are essentially the same. That's because the primary effect of varying lithophysal porosity is on the thermal conductivity.

This slide shows the impact of uncertainty on drip shield surface temperature for the high temperature and low temperature operating modes. On each plot, on the Y axis, we have temperature; on the X axis, we have time. This shows that the dependence on thermal conductivity, we reach a peak temperature shortly after closure. The temperatures decline down to ambient temperatures at around 100,000 years. You can see that our temperatures, as I indicated on the previous slide, are about an 80 degrees swing here when we vary thermal conductivity.

On the low temperature operating mode, and please note the scale here is different on these two plots, but the swing here in temperature is approximately 15 degrees. So you can see that the impact of thermal conductivity uncertainty on drip shield temperature is more significant in the high temperature operating mode as compared to the low temperature operating mode.

And the same sort of conclusions can be made with respect to relative humidities within the drift at various locations.

This slide shows comparison between the different operating modes, waste package temperature sensitivity, the location and waste package type. As you can see, the temperatures are much higher obviously in the high temperature operating mode as compared to the low temperature operating mode. This slide also shows the separate curve is waste package temperatures calculated in regions in the center of the drift. This is near the edge of the drift. You can see we have substantial variability in waste package surface temperatures across the repository. We also have variability in waste package temperatures for the lower temperature operating mode, although the variability is less significant.

Some conclusions could be made about the waste package types, where we've got the PWR waste package, top curve, high level waste package is the lower curve.

This slide shows relative humidity sensitivity to location. The high temperature operating mode is represented by the red curves here, and you can see that for the entire post-closure period when we compare the two operating modes, that the high temperature operating mode has a tendency to have higher relative humidities on the waste package surfaces.

Also, I'd like to point out here that it's not indicated on the slide that the lower curve, up until about 900,000 years, is for the center of the repository. This curve actually, it's not very well indicated on this figure, but it actually crosses, and this curve up here represents the center of the repository. So the center of the repository is represented by this curve as it crosses over.

The outside of the repository is represented here, and it crosses over and follows this trajectory here. So, basically, in the high temperature operating mode, the relative humidities are lower in the center of the repository, and then that switches at later times, where the relative humidities are actually lower at the edges of the repository than at the center of the repository. And this is due to the fact that as temperatures elevate, heat transfer due to radiation and convection is more effective, decreasing the temperature differences between the drift walls and the waste package surfaces. This tends to increase the relative humidity on the waste package surfaces.

In the low temperature operating mode, heat transfer is less effective and, therefore, the temperature differences between the drift walls and the waste package surfaces, for example, actually tend to be greater and, therefore, we get a greater reduction in relative humidity.

This is a slide showing invert evaporation sensitivity to location. Invert evaporation is important because it determines the chemistry in the invert. This slide shows that in the high temperature operating mode, in large regions of the repository, the inverts are actually dry for the first thousand years, and at later times, the evaporation rates in the high temperature operating mode are greater than the evaporation rates in the low temperature operating mode.

This is important to remember when you're making comparisons between the chemical conditions in the invert between the low temperature operating mode and the high temperature operating mode.

Another thing you can notice here, too, is that the variability between the center and the edge of the repository is greater for the high temperature operating mode than the low temperature operating mode.

The only uncertainty that's actually included in the TSPA-SR and in the supplemental TSPA from thermal hydrology is the uncertainty associated with the infiltration field. So that is the same in both the supplemental and the TSPA-SR.

This slide shows the sensitivity of waste package temperature to infiltration and location. So you can see in the high temperature operating mode, variability is much more significant in the temperatures, and the sensitivity to infiltration rate is somewhat more sensitive than in the low temperature operating mode.

So in the low temperature operating mode, we've got less variability in waste package temperatures, and the sensitivity to infiltration is also less.

I'm going to summarize the impacts of the different thermal operating modes. Waste package temperatures, the high temperature operating mode peak waste package temperatures range from 126 to 185 degrees C., versus 65 degrees to 91 degrees C. for the low temperature operating mode. And that's based on mean infiltration analyses.

Temperatures are sensitive to thermal K, but more so for the high temperature operating mode. The high temperature operating mode also exhibits larger variability in waste package temperatures and stronger dependence on infiltration flux.

Waste package relative humidity tends to be lower in the low temperature operating mode, with less variability and dependence on infiltration.

Invert saturation. Inverts tend to be dry up to 1,000 years, depending on location in the high temperature operating mode. Saturation trends are similar for both operating modes after 1,000 years.

Invert evaporation rates tend to be more variable and higher in the high temperature operating mode after 1,000 years.

We used data, are in the process of using data from various sources. We currently are performing a fully three-dimensional NUFT simulation for a partial, or a segment of an emplacement drift to provide a benchmark for multi-scale TH model. We used data from the various field tests. We have data from a quarter scale drip shield condensation test. We are about to conduct quarter scale ventilation and natural convection tests. And we are currently analyzing some THC laboratory column tests.

That concludes the TH portion of the talk. Now I'll move into the chemical environment portion. We get key inputs for this model from the near-field environment, from the TH environments, and from design. Key outputs include water composition at various locations within the drift.

I should point out that in the TSPA-SR and in the supplemental TSPA, we do not calculate chemical conditions on the drip shield and on the waste package, and the reason we do not do that is because the degradation models for those barriers do not explicitly include the dependence chemistry.

In the invert, however, radionuclide solubility and colloid stability are dependent on chemical conditions, and we provide those chemical conditions.

Main improvement for the chemical environment model for the supplemental TSPA is that we do include the uncertainty associated with incoming fluid compositions, and we perform analyses both for the high temperature operating mode and the low temperature operating mode.

This table summarizes some of the key chemical environment uncertainties. The red font denotes those uncertainties that are included in the supplemental TSPA, the uncertainty in the compositions of the fluids entering the drifts, which I just mentioned.

The other key uncertainty is that associated with radionuclide sorption on the corrosion products. Pat Brady in his waste form talk will discuss this issue in more detail tomorrow.

One of the improvements, model improvements, is we have developed a model for the mixing of different waters in the invert. There's water from at least three different sources. That includes water from fractures, water from the matrix, condensation water, and water from the waste package. All of these waters basically have different chemical compositions. And although we didn't get this model completed in time for implementation into the supplemental TSPA, it is a model that we plan to implement in the next TSPA.

This slide illustrates the effects of evaporative concentration for two different water types, a tuff pore water type, and a J-13 water type. These waters are representative of the two types of waters that can come into the drift from the near field. Fracture water, or J-13 water type, enters the drift at the crown of the drift, and that water tends to increase in pH as it concentrates due to evaporation.

The other water type, which is the pore water, which can enter the EBS by imbibition through the invert, is represented by tuff pore water type, which shows a trend of decreasing pH with evaporative concentration.

The reason I point this out is because the TSPA-SR used J-13 water type. The supplemental TSPA in the EBS uses the tuff pore water type, primarily because the majority of the water that enters the invert is through imbibition. And as I mentioned earlier, we have developed a model to account for the uncertainties associated with the mixing of these waters in the invert.

As I mentioned, we do account for the uncertainties in the incoming fluid compositions into the EBS, and this slide just illustrates the mechanism by which we do that in TSPA. We receive--the incoming compositions are transient, and what we do is we divide the post-closure period up for the high temperature operating mode into six representative periods, as indicated here, a pre-closure period, boiling, cool down, extended cool down period, transition to ambient, and ambient. And in each one of these periods, we extract a representative composition from the near field seepage compositions and use those as starting waters in our chemistry model for our invert chemical condition calculations.

Just to give you an idea of the impact of thermal operating modes on the chemical conditions in the invert for pore water type seepage, I'll briefly walk through this slide.

As indicated over here, high temperature operating mode is represented by the red dashed lines. The low temperature operating mode by the blue lines. And you can see that the low temperature operating mode has a tendency to remain up in this area with pHs that range above seven. On the axis down here, we have evaporative concentration. So this parameter 1 minus the evaporation rate divided by the seepage rate, when that is equal to 1, we have no evaporation. So this axis location indicates no evaporation. To the right, it's condensation. To the left, it's evaporation.

This shows you that 50 to 1,500 years after closure in the high temperature operating mode, that our pHs in the invert, once evaporation occurs and increases, the pHs tend to decrease. At later times, the pHs tend to increase until we're up in this region. So the main difference I want to point out here is that in the low temperature operating mode, our pHs remain typically above seven. In the high temperature operating mode, pHs can go down low in the range of near five for the first several thousand years.

I'll summarize the impact of the thermal operating mode on the chemical conditions in the EBS. There are two general types of water, as I mentioned. Matrix water, pH tends to go down with evaporative concentration. Fracture water, pH goes up with evaporative concentration. Matrix water is used in the supplemental TSPA. In the TSPA-SR, we implemented fracture water. That's a key difference.

Bottom line is that the high temperature operating mode will tend to have lower pHs and higher ionic strengths because of higher evaporation rates.

Now, Jim Blink in his talk will summarize the effects of the different operating modes on performance processes. I will not do that in my presentation beyond what I've just mentioned here.

And then Mike Wilson will actually describe the differences in doses due to the different operating modes. But this should provide you some basic information on why there are differences in the two operating modes and between the TSPA-SR and the supplemental TSPA.

We used data from various sources, mainly published literature on the formation of natural brines and evaporites. We have laboratory evaporation studies that have been conducted for the project. We've made comparisons between our models and the evaporation studies. Of course we can check using simple handbook solubility values. There's published literature on different waters mixing in oceans, estuaries and lakes. And we have some ongoing analyses of our THC laboratory column experiments.

Now I'm going to briefly just outline the work that we've done in the transport, flow and transport area. I will not present any results in this part of the section, mainly because of time. But I want to outline the main improvements for the supplemental TSPA, includes seepage evaporation at the drip shield. We did not include that in the TSPA-SR. We've improved our drip shield and waste package flux models. They're less conservative and we have incorporated uncertainties.

We now have an in-package diffusion model. Before, we neglected diffusion, and once radionuclides were released from the waste form, they were immediately released from the waste package. We now account for radionuclide sorption. There are large quantities of corrosion products that form inside the waste packages. These can significantly delay dose, and those have been included in the supplemental TSPA. And as I mentioned earlier, Mike Wilson and Pat Brady will show results related to sorption.

This slide summarizes key uncertainties that we evaluated for the SSPA. As you can see, some of them were not implemented in the supplemental model, and typically that was based on one-off UU studies that showed that their effects were not important with respect to total system performance.

Impact of thermal operating mode on EBS transport. EBS flow, evaporation rates are a function of thermal response. In May at the last TRB meeting, I presented some results on the effects of seepage evaporation on dose for early failures. It was shown that seepage evaporation does not produce a significant effect.

EBS transport. Diffusion coefficients are a function of temperature and saturation. So they're a function of the operating modes indirectly. Our in-package diffusion model relies on water entering the package through the gas phase, and adsorption of water onto surfaces inside the waste package. We calculate diffusion through those water films.

The amount of water that sorbs onto the materials is a function of RH, which is indirectly a function of the thermal operating mode. So this sort of summarizes the indirect effects of the thermal operating mode on EBS transport.

We have various sources of data that we can use to validate our understanding of the processes and our models. Our EBS quarter scale tests, which include condensation beneath the drip shield, flux through the drip shield, laboratory data for diffusivity of unsaturated crushed tuff, laboratory column transport and sorption tests, published investigations of colloid characteristics, and field data on colloid facilitated transport.

In summary, the efforts that we have put forward the past three or four months have substantially improve our understanding of uncertainties associated with EBS environment, and this work will help us in planning our future work.

Ultimately, in the TSPA for the license application, all key EBS uncertainties should be included in that TSPA. We've made a significant step towards identifying those key uncertainties, and we have implemented some of those in the supplemental TSPA.

We are currently planning our future work, and much of that work will be directed towards completing our evaluation of uncertainties and completing our development of models to implement those into the TSPA.

And as I mentioned a couple of times during my talk, conclusions regarding the impact of thermal operating modes on performance will be discussed in Mike Wilson and Jim Blink's talks.

BULLEN: Thank you, Dr. MacKinnon.

I would like to leave a little bit of time for questions, and so I'm going to delay the public comment period for, to start with, ten minutes, and we'll see how the Board goes. I will at most go 15 minutes, and then we'll allow a half hour for public comment.

So, questions from the Board? Dr. Parizek?

PARIZEK: Parizek, Board.

Relating to the pH data on Page 23, I guess all of that has a lot to do with, I suppose, the behavior of steel sets in the drifts, and so on? Because, really, the waste package, if it works the way it's supposed to work, won't care about this kind of pH environment, will it? It will be there for so long that this won't impact it, but it may start tearing apart steel sets and that sort of thing? I'm talking about the more acidic environment under the high temperatures.

MACKINNON: Actually, our analyses have shown that the interactions of the seepage with engineered barrier system materials does not significantly impact the chemistry of the seepage inside the drift. The lowering of the pH is due to evaporative concentration and precipitates, formation of precipitates. So this just represents the evolution of brine. And this represents the pore water type that has a tendency to decrease in pH as you concentrate the brine, and that's primarily because the calcium carbonate ratio is greater than one. And as I mentioned, the matrix pore water has the opposite effect.

The effects of this sort of behavior really will only be important to early releases, and primarily for releases before 4,000 years.

PARIZEK: I had another question relating to the drip shield. In terms of condensation underneath a drip shield, it seems like the fact that there may be condensation there on a smooth surface versus a rougher surface of the emplacement drift, should allow some moisture to move down the interior side without it actually dripping, but actually kind of move as a river along the side. Is there any discussion about that, or any experience with that from the laboratory work as to what percentage of condensed moisture that could accumulate would drip versus just flow down the under side of the drip shield, and as a result, really not encounter the waste package?

MACKINNON: We do have a discussion addressing this specific issue of condensate forming on the under side of the drift and shedding along the smooth surface and not contacting the waste package. Our quarter scale condensation tests actually verify that behavior.

In the model, we implemented a drip shield condensation model in the supplemental--in the one-off study for the supplemental TSPA analyses. And what we did there was we introduced an uncertain parameter. That uncertain parameter represented the fraction of seepage that shed on the under side of the drip shield. And it turns out that that phenomenon is really not that important. Well, and it's primarily masked because waste packages do not really--they do not fail when condensation is really on the under side of the drip shield is important, which is at earlier times when you've got larger temperature differences. But at later times when you actually have releases, the condensation is not that significant. So, in our results, the importance of drip shield condensation was relatively low.

PARIZEK: Thank you.

BULLEN: Other questions from the Board? Dr. Sagüés?

SAGÜÉS: Yeah, I was intrigued by Figure 12, transparency Number 12, and, of course, the relative humidity being lower in the cooler concept. The first question I had was this takes into account the presence of the drip shield as well, or the drip shield doesn't change the results very much?

MACKINNON: It takes into account the presence of the drip shield, in that radiation between the waste packages is accounted for. Radiation between the waste packages and the drip shield is accounted for. And radiation from the drip shield to the drift wall is accounted for.

SAGÜÉS: Okay. So this a fairly linked kind of--

MACKINNON: Yes.

SAGÜÉS: Now, if you go to the previous, to 11, and you look, say, for example, on the left there, I was trying to figure out, say, when both the low temperature concept and the high temperature concept reached, say, for example, 80 degrees centigrade somewhere in the middle of the distribution. The low temperature would reach that at around 800 years, or so, something like that, if you go to the middle of the distribution, and then you trace the line from there, and you find out equivalent point in the hot concept, and that would be at around, say, 1,500 years, or so. Do you see what I mean? Look at the center distribution. And then if I go now to Figure 12 again and I look at, say, 800 years for the low temperature, the relative humidity is about 45 percent, and if I go to 1,500 years when the high temperature concept has the same temperature, then in that case, the humidity in that case is 80 percent. So, I mean, there's more to the story than just the difference. If I compare the two packages at the same temperature, the difference appears to be even greater, dramatically so.

MACKINNON: Yes. Well, let me see if I can answer your question.

SAGÜÉS: And I'm trying to make sense out of it. Maybe you can tell us a little bit more about how this works.

MACKINNON: Okay. In the high temperature operating mode where we get dry-out, the magnitude and the duration of dry-out actually control the relative humidity up to about 900 to 1,000 years. As I pointed out on this figure here, this represents the interior of the repository, and right around 900 years, it crosses over, and this is the interior of the repository. So the edge of the repository early on has a higher RH, as compared to the center of the repository, and that's switched at the later times.

Now, at the earlier times where you have substantial dry-out, especially in the center of the repository, that drives down the RH. As the drift wall and the dry-out region decreases, the RH tends to come back up. At that point, the RH is determined by the temperature difference between the drift wall and the waste package surface. That temperature difference in the high temperature operating mode is less than the low temperature operating mode.

Although the temperatures are higher, the difference is less, and as a result, you get lower relative humidities in the lower temperature operating mode, and it's mainly because in the higher temperature operating mode, heat transfer by radiation and convection is much more effective at the higher temperatures. And so it tends to want to equalize the temperatures more than in the low temperature operating mode. And that's why you see a lower relative humidity in the low temperature operating mode as compared to the high temperature operating mode.

SAGÜÉS: Right. What I'm saying is that also the amount of water at the drift wall is making a difference; right?

MACKINNON: Pardon?

SAGÜÉS: The amount of water at the drift wall is making a big difference there.

MACKINNON: Right.

SAGÜÉS: Whatever is the amount of water, whichever measurement you want to use for that.

MACKINNON: Right. In both cases, the RH after--in both operating modes, after 1,000 years, the RH at the drift wall is close to 100 percent, although the temperatures are higher in the higher temperature operating mode.

BULLEN: Dr. Wong?

WONG: Jeff Wong, Board.

I think I have an easy question. On your Slide 17, you have a list and you list these as the multiple lines of evidence. And can you help me understand, the Board asked for lines that should be independently derived, or derived independently of performance assessment, and it seems like a lot of the tests that you have listed up here actually went into supplying data or developing models for PA. So can you show me what part is truly an independent or alternate line of evidence, and what part went to building the PA models and supporting the PA?

MACKINNON: That's a good point. Actually, all of this information is project derived information, and I hope that Ardyth Simmons addresses your question in her presentation. I came prepared just to discuss project related information that we are currently using right now to help validate our understanding of the important processes and our models.

BULLEN: Thank you, Dr. MacKinnon.

With that, I'm going to call the technical portion of this session to a close, and invite people up for public comment. We have--actually, prior to public comment, I'd like to ask Brett Leslie from the Nuclear Regulatory Commission, who has requested to make a statement, to please come forward.

Would you like to do it here?

LESLIE: Thank you, Dr. Bullen.

I'm Brett Leslie from the U.S. Nuclear Regulatory Commission. I'm a staff member there. That's actually kind of a comment and question. I have two questions, and I'm hoping that perhaps during the public comment period or if DOE has time, to provide responses to these questions.

Let's start with the shorter one first. The first question concerns a DOE statement earlier today that errors associated with their total system performance assessment for site recommendation were discovered back last fall. The question I have and we have is how in the future will DOE inform interested parties of the errors they find?

The second area of our comments and our question concerns the scope and timing of the U.S. Nuclear Regulatory Commission's sufficiency comments. Dr. Brocoum indicated that in November, 1999, the Department of Energy informed the Nuclear Regulatory Commission what DOE documents would be the basis for their site recommendation. At that time, the NRC indicated that we would base our preliminary sufficiency comments on our review of those documents.

However, today, Dr. Brocoum's presentation indicated what documents will form the basis of the Department of Energy's site recommendation. As an independent regulatory agency, the Nuclear Regulatory Commission is guided by and follows what is required by law.

So what is required by law? The Nuclear Waste Policy Act requires that the Nuclear Regulatory Commission provide preliminary sufficiency comments to the Department of Energy for inclusion in its site recommendation. The law requires us to comment on the sufficiency of two items, the sufficiency of the Department of Energy's at-depth site characterization, and the sufficiency of the Department of Energy's waste form proposal.

Further, the law requires that the basis of our comments be the sufficiency of the information with respect to inclusion into a potential license application.

Dr. Bullen earlier addressed the question of sufficiency of data supporting the Department of Energy's Supplement Science and Performance Analysis Report. Dr. Brocoum indicated that last week, the Department of Energy and the Nuclear Regulatory Commission met. We did meet, and some information was passed in public forum. That information stated that the data and analyses supporting the Supplemental Science and Performance Analysis Report, including the low temperature repository design, would need to be qualified if they were used in a potential license application.

Today, Dr. Brocoum stated two things. One, selection of the design, either high or low temperature, would occur after site recommendation, but prior to license application. Two, in his slides, he indicated that the Supplemental Science and Performance Analysis Report is used as input into the Department of Energy's site recommendation.

If there are two design options addressed in the Department of Energy's documents supporting its site recommendation, and if each design could be potentially included in a license application, the Nuclear Waste Policy Act seems to require that the Nuclear Regulatory Commission provide preliminary sufficiency comments on each design.

Given that the Supplemental Science and Performance Analysis Report is 1,300 pages, and the Department of Energy has requested the NRC provide our preliminary sufficiency comments by October 1st, the release of this document is critical.

Will you please indicate when the report will be available, and how is that date impacted by today's statement that the date for cutoff of data has not yet passed, and that the document has not yet entered into DOE review process?

Thank you, Dr. Bullen.

BULLEN: Does DOE want to respond on the record to that one? Dr. Brocoum?

BROCOUM: As to the first comment--Bill has been taking notes here--I think, you know, Robert Clark indicated that the deficiency report was written on the errors for the TSPA. I think what we're going to do in the future is, and I think what we're supposed to do on our quality program, is when an error is discovered, a deficiency report should be written at that point in time, and then you could, you know, track and resolve following a structured process. And that's what we will do in the future.

If the document is in process, like the SSPA, you would obviously resolve that error before you issue the document. If the document has already been issued, then you would issue an errata sheet, or some other form of communication to show that you do have an issue or an error in the report. So that's how you'd inform the public.

I think Bill, in terms of when we will issue the document, Bill has indicated that it would come out in the next several weeks. Volume 1 is a little more ahead of Volume 2. Volume 1 has gone through a review. Volume 2 is following, and that's just going into the review right now. So I can't give you an exact date when it will be issued, but it will be issued over, we hope, the next several weeks. So I can't give a precise date. I wish I could. I just can't. Just like you can't give us a precise date when your rule will be finalized.

BULLEN: Any other comments from DOE and NRC?

(No response.)

BULLEN: Okay, we've got that on the public record.

We have about 21 minutes or more maybe left, and I have three members of the public who wish to comment. So, dividing by three, turns out to be seven. So, we'll start with Mr. McGowan. Mr. McGowan has also given me written questions. If he so chooses, I would also read those into the record and ask them, but I'll defer to him first.

Would you like to make your comment first, Mr. McGowan?

MCGOWAN: Yes.

BULLEN: Okay. Would you like to do it from here, or do you want to go--

MCGOWAN: I would like to do it from there.

BULLEN: That's fine.

MCGOWAN: It's more impressive.

Tom McGowan, Las Vegas resident. Can you hear me all right?

I am duly impressed with today's excellent presentations, particularly the last five, which were hastily prepared in less than four months, somehow miraculously. One can only envision the superlative nature of the finally completed SSPA anticipated to ensue by July 6th, in which optimistic and inexplicable case, DOE will undoubtedly receive a hug and a kiss, and God knows what else.

Unfortunately, those exhaustive presentations are only applicable for up to 10,000 years, which is my understanding, is akin to the blink of an eye, and which gives one pause. But don't let that deter you. Just continue with the excellent work.

Again, my name is Tom McGowan. That was a preface. This will require approximately five minutes. Will that be appropriate?

BULLEN: You've got five left. That's fine.

MCGOWAN: Thank you, sir. Two are gone already?

BULLEN: But who's counting?

MCGOWAN: All right. These meetings or proceedings aren't about nuclear waste transportation and storage. Ultimately, they're about the life or death of the people of the future who inevitably will be impacted by your official acts, omissions and advisory recommendations, persuasive on the formulation and direction of national public policy by the Congress and President of the United States. I believe that was the initial assumption here.

In the real accurate perception, nuclear waste is not now and never was and never will be the problem. Rather and irrefutably, the fundamental problem is comprised of the frailties and foibles of human nature itself, personified as rooted and embodied in the official generic "you," who are defying naturally order, axiomatic--and instead, persist in a state of denial and service to limited special interest--in furtherance of a wholly subjective agenda--can you invest public interest inclusively, inter-generationally and in perpetuity.

You're also the sole possible--and effective solution, contingent upon your timely attainment to a higher idealized standard of human and spiritual quality in terms of ethics, morality, reason, integrity, responsibility, and above all, conscience.

Long before there was a Congress or President and a nation called the United States, there were Ten Commandments, much briefer than what you've got here on the table, ordained by an infinitely higher authority than the Congress or President of the United States and leaders of all the nations all combined.

The First Commandment unequivocally asserts, with special emphasis added, I am the Lord they God, not you. You shall not have false God before me, not even the Congress and President of the United States, nor any agency or government whatsoever, and especially not the limited special interested--politically persuasive, profit motivated, private, commercial, nuclear energy industry, their affiliated utilities and rate paying consumers, who do have a valid and equitable interest, but so does everybody else. There's a lot more everybody elses, at last count.

The Sixth Commandment candidly asserts thou shalt not kill. It's not ambiguous at all. Not only your own progeny, but also and especially not the as yet unborn people of the future who cannot be here to plead their God given right under the same gifts of life, liberty, and pursuit of happiness which you yourselves enjoy.

There is indeed a higher moral imperative than priority concern of the humidity induced corrosion rate of Alloy 22, and the relative merits of a high or low temperature repository design alternative, one in fact inevitably impacted consequences will be the same in either case, as you astutely pointed out. And geologic time won't end in 10,000 years, as reported, but will continue for another 5 billion years beyond that limited finite term, as also will the longest lived deadly radionuclides, and once imposed, regardless of the time delay means, death is wholly subjective, as well as final and irreversible.

I will probably attest to that fact relatively soon. But you knew all that to begin with, didn't you. You really did. And because of your deservedly deemed eminent and prestigious advancements--in terms of intellect, wisdom, experience and expertise, and commensurate sense of responsibility and conscience beyond the limits of that exhibited by the hired hands of DOE, OCRWM, YMP, I hold you, the members of the TRB, to a higher standard of compliance with that moral imperative ordained by an infinitely higher authority than the Congress and President of the United States, not withstanding the fact that the Congress and the

President has enacted and amended, have ordered you to refrain from--exercise of the fullest range of your reasoning and capability, and have prohibited your exercise of judgmental address of matters of public pertinent policy respecting the underground storage of high level nuclear waste, and not withstanding your expedient disclaimer of direct accountability and responsibility of the ensuing consequences of your acts, omissions and advisory recommendations, contributed to the potential persuasion of the formulation and direction of nuclear waste pertinent national public policy by the Congress and President of the United States, which in the worse case scenario is realistically--upon the genuine best public interest on a historically unprecedented human and universal scale inter-generationally for the rest of geologic time.

You are well aware of the fact also that on axiomatic grounds, it's scientifically and technologically impossible to guarantee the safe, secure and human--in underground storage of high level nuclear waste, be it any combination of natural or artificial barriers, over any substantially term, either at Yucca Mountain, Nevada, or elsewhere nationally, or anywhere on the planet.

That's why the DOE's incomplete work in progress, SSPA, and it's unspecific, indirect response to the Board's four questions of priority concern are dead on arrival.

Because a Congressionally mandated mission task of the DOE and the TRB respectively and inclusive are impossible to begin with, and are an embarrassing exercise in abject futility, an obscene waste of time, energy and other resources, and an effrontery to public sensibilities.

Now, what is it about the word impossible that the world's leading scientific, technology and academic minds of our time are unwilling or unable to understand and accept as axiomatic fact. Don't respond. I can't conceive of a response from you on that question, none.

At tomorrow's public comment, I'll remind you of the profound implications that will inevitably apply and ensue in the instance of your failure, however inexplicably, to comply with the higher moral imperative in the genuine best public interest, not for 10,000 years, but for the rest of human time. Understand me clearly, you are and will be held responsible and accountable, and anything you say or do can and will be used against you in an international tribunal.

It is coming, guaranteed. Understand history, it happened before. The man's name was Eichman, only he was more limited in scope than you. You are above that level. I demand it of you, and you will perform.

BULLEN: Thank you, Mr. McGowan. And Mr. McGowan has also provided me with five other questions that were of a technical nature associated with the presentation. And so rather than ask them now, is there a way we could provide them to the DOE and ask for a response? Is that a reasonable thing to do? How do we do that? I'm asking my Executive Director as I look across the room.

Okay, thank you. I'll let the public comment continue with Mrs. Sally Devlin. Sally, are you here?

DEVLIN: As always, I have to welcome you to Nevada. It's so good to see so many familiar faces. And thank you so much for coming. This is my eighth anniversary, and I met Tom McGowan and I really feel what he said is very true and very scary. We met eight years ago at an NWTRB meeting under the Cashman Field when John Cantlon was there, and I said you cannot put DOD stuff in my mountain that is classified, and I still say that. And we've come a long way, and one of my favorite doctors said to me, "You stupid old lady, go to school." So I did, and we finally graduated, and that was even more fun.

But what was very interesting about this is learning the complexity of this project. And it is very complex, and I think it's wonderful that the science continues on, and I sincerely hope it continues on modelling for the next 20 years. Then I'd really be 92, and I won't care, so I just say to you keep on modelling.

I see no definitive answers in this. And what I saw in the engineering report, which I read cover to cover, as well as the report to Congress by Dr. Cohon, was the continuous use of the words transparent and uncertainty. And, unfortunately, when we had our meeting of the DOE on the EIS, and so on, and of course we didn't know anything about the SSPA, we still don't, so I hope some day we will see that and be able to eat it up and devour it and see what it says. So we were denied that, too. And, remember, we have nothing in Pahrump, and that's Nye County where the repository will be.

So, again, I say what do these words mean, transparent uncertainty, which is repeated a thousand times? And to be transparent means we can see through you, and you're uncertain of what we're seeing. And this is the impression I think that the public has, and it was very well stated at our meeting. We don't know what you're doing, because every time you have a different group come out and listen to us and we read a report, it has changed, or it has this, or it has that, particularly on the water. I talk about in the report, it mentions 70 metric tons of waste, and then in another paragraph down the page, it talks about 97,000 metric tons. We change the acronyms. We change--now you want to put in four swimming pools up at the test site when we have compression faults. And I can attest to that. In the '92 earthquake, I lost a horse in a compression fault.

Went out in the morning, and there's this gelding, who's almost 17 hands, and I couldn't find him. Fell in a hole. The whole ground collapsed. This happens all the time at the test site because of the earthquakes. The water levels go down, and there's no question that we have floods, and what have you, and this is measurable, and I don't hear you talk about that, because the whole basin and range is a compression fault.

The other thing is notice on meetings. We didn't know about this meeting. We didn't know when it started. We didn't know anything. We certainly don't know about the NRC meeting, and we don't know about the INEA peer review meeting. And I hope that others ask these questions and you will tell us when they meet, where they meet, and so forth, because some of us will split it up and go to these things because they're terribly important.

The other thing--I'm going to keep this very short--but you keep referring to different laws. When I started, it was 10 CFR 73, and I'm sure if David and Trudy were here, they'd be reminding you of these changes, and then I see other numbers, 10 CFR 963, and this sort of thing. And it is disturbing, the numbers that are inconsistent. So somewhere along the line, your ghost writers are not doing their homework with their editing, because if it's confusing to me, imagine what it would be to someone who's never read one of these confusing things.

And the only other thing I can say is on everything that you do, and Tom used the word, bless his heart, was disclaimer. I see disclaimers in every single one of your reports. I was sent the information from the May 8th. There was not one of the presentations that didn't have a disclaimer in it. And today again, everything is disclaimer.

Now, if you've been doing this for 20 years, I've only been around eight years, that's pretty bad for the billions of dollars, and again, my field was transportation and you won't even talk about that, and that's what really scares me.

But I'm going to cut this short and I'm going to talk to my bearded friend back there at the back table who is our political entity. Don't turn away. And that's you. And what has happened my feeling is about this Yucca Mountain project is you're not listening. Listen, this has not become science, but political science, and it's really terrifying to me that the Congress might be bought by the nuclear reactors, or the nuclear power industry, and they might convince Bush to say go ahead. And this is absolutely terrifying because, as I say, it's become political science.

And I'm going to end with the statement the last time we listened to a Bush, we spent 40 years walking in the desert. Thank you.

BULLEN: Thank you, Mrs. Devlin.

Our final public comment person that--or person requesting comment that registered is Grant Hudlow. Grant?

HUDLOW: Thank you. I just have two brief questions that can be answered later, and two brief comments.

Who is responsible for getting the test date to me that you talk about on the colloids transport at Los Alamos National Laboratory? That's about Page 29 of the Engineered Barrier System. So I'd like copies of that.

And the Nevada State test ruled out Alloy 22, although I hear you still mentioning it here. The stuff just came unglued. And so who's responsible to get an industrial chemical expert so that we have a chance of building a successful canister? I've asked that question for several years. There isn't anybody in the DOE that can even understand it. Maybe somebody at NWTRB can understand it. There isn't anybody in the NRC that can understand it. The NWTRB has a little broader expertise. There should be somebody available to even figure out that that's needed.

We have some new data that calls into question the millirem as a unit of predicting the damage that various radionuclides are going to do. As you know, it's very specific to each radionuclide.

The out-gassing in Hanford, SRS, as well as the nuclear plants, are creating clusters of leukemia from apparently the Strontium 90. The millirem standard would say that there's not enough there to make any difference, and the facts are that there is enough. So that needs to be straightened out.

Along those same lines--that data is in, but it's not in a final form yet. Along those same lines, we have data coming in that's indicating that Vitrium has a similar problem. I guess the question on that is if these are not radiological effects, are they biochemical effects? In other words, have we mixed the two? Because Vitrium disables the immune system, and so perhaps it's not the radiological effect that's doing that. It may be biochemical.

My last comment is we have a chance to save the nuclear industry, and Germany of course just kicked them out of the country. The same kind of movement is going on here for these reasons that I've mentioned. We have a chance to save it. One of them is we just have an agreement now with the NRC to implement transmutation, build factories, get rid of the waste, take it away from the DOE, who have indicated obviously they can't even begin to handle the problem. And so as that comes on down the pike, I just wanted you to be aware that that's happening right now.

Thank you.

BULLEN: Thank you, Mr. Hudlow.

Are there any other members of the public who would like to make a comment at this time?

(No response.)

BULLEN: Seeing none, we will be in recess until tomorrow morning at 8 o'clock, when we will reconvene for the second day of this session.

Thank everyone, all the presenters and all the public for coming. Thank you.

(Whereupon, the meeting was adjourned, to reconvene at 8:00 a.m. on June 21, 2001.)

1