UNITED STATES
NUCLEAR WASTE TECHNICAL REVIEW BOARD

SUMMER BOARD MEETING

June 25, 1997
Crowne Plaza Hotel
Las Vegas, Nevada

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P R O C E E D I N G S

8:00 a.m.

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

There must be something great going on in the back of the room. Thank you.

Welcome. The Board is very pleased to be back in Nevada. Our last meeting was in Pahrump, which was an indication of our commitment to having an opportunity, a maximum opportunity, to interact with the people who would be most affected, directly affected by a repository at Yucca Mountain. That meeting will long be remembered for the 1000 cookies that we consumed in a two day period in Pahrump, and for the wonderful hospitality that we enjoyed there, and we're pleased to be back here in Las Vegas.

The board, our board has gone through a transition through which we replaced most of the members of the board. It's important that you know who they are, and I'm going to introduce them to you.

First of all, as some of you have heard already, I'm Jared Cohon, the Chairman. I'm dean of the School of Forestry and Environmental Studies at Yale University. But as of next week, July 1st, I become president of Carnegie-

Mellon University.

John Arendt--and colleagues, if you'd raise your hand, maybe even turn around so people can see you--John Arendt is a product consultant and a specialist on the nuclear fuel cycle and the transportation of radioactive materials.

I want to interject here that the Board, in its transition, also has reorganized the panels through which we conduct a lot of our business. John chairs the panel on the waste management system.

Daniel Bullen, professor of Nuclear Engineering at Iowa State University, an expert on many aspects of nuclear waste, including waste packages and total system performance assessment. Dan chairs our panel on performance assessment.

Norman Christensen, Dean of the Nichols School of Environment at Duke University, an expert in terrestrial ecology.

Paul Craig, professor emeritus at the University of California at Davis, a physicist by training, I remember that, and someone who's been involved in science and science policy for many years.

Debra Knopman, director of the Center for Innovation and the Environment in Washington, D.C., former Deputy Assistant Secretary of the Department of Interior, and a former scientist and science manager at USGS, and an expert in ground water hydrology. Debra chairs our panel on site characterization.

Priscilla Nelson, program director in the Directorate for Engineering of the National Science Foundation in Washington, a former professor at the University of Texas and an expert in geotechnical problems, engineering. She chairs our panel on the repository.

Richard Parizek. Richard is professor of Hydrologic Sciences at Pennsylvania State University, and an expert in geology and ground water hydrology.

Alberto Sagηϑs, professor of Civil and Environmental Engineering at the University of South Florida, an expert in materials and corrosion, with a particular expertise in concrete and its behavior under extreme conditions.

Jeffrey Wong, science advisor to the director of the Department of Toxic Substance Control of Cal. EPA in Sacramento, an expert in risk assessment. Jeff chairs our panel on environment regulations and quality assurance.

Florie Caporuscio, who was a member of our Board, has found it necessary to resign from his membership on the Board. Florie works for a company, the primary business of which is with DOE. It was a conflict situation that he could not resolve, and unfortunately felt it necessary to resign.

Our Board staff has not changed. It's the same friendly faces that you've seen at most of our prior meetings. In the interest of time, I'm not going to introduce them. I'll just note that they are probably the best professional staff of anybody like ours that you could find.

We have a very full agenda over the next day and a half, well timed and well prepared we think to deal with the issues that are being faced by the program. As we all know, the program is focusing on a very important milestone, the viability assessment, which is scheduled to be completed and issued on September 30, 1998, a little more than a year from now. Our meeting has been organized, therefore, around that topic.

Total System Performance Assessment is a key element. Indeed, it's become the centerpiece for the Viability Assessment and for determining the suitability of Yucca Mountain to house a repository. It's also the basis for the proposed revisions in the DOE site suitability guidelines. It seems clear, therefore, that it's critical that we as a Board understand TSPA and that the scientific community generally and the lay community understand TSPA.

There are obviously uncertainties in Performance Assessment. It's important that we understand those as well.

TSPA is a key tool as well to understand those uncertainties, and we look forward to hearing from the program and various experts in understanding those uncertainties better, and to understand where we are in the process of quantifying those uncertainties. That will be the focus for today.

Tomorrow, we will hear about other products of the Viability Assessment. These are related to the projected cost of repository construction, plans and the cost of additional work to take the program to the license, and performance confirmation after licensing.

I have a few housekeeping and administrative things to announce. First of all, we ask that all participants sign in. We had a little computer glitch earlier, which has now been resolved. So during the break and at other times, if you could sign in, we'd appreciate that.

All speakers, Board members, presenters, questioners, commentors are asked to please speak clearly into the microphone. As you know, these meetings are on the record and it can't be effectively put on the record if you don't speak into the microphone and if you don't identify yourself.

We will have a public comment period at approximately 5 o'clock. People who wish to comment during that period are encouraged to sign up with Helen in the back, in the corner next to the door, and we will call on you at the appropriate time.

Without further ado, I would call on Lake Barrett, acting director of the Office of Civilian Radioactive Waste Management, to talk to us about the status of the program and the Viability Assessment. Welcome back, Lake.

BARRETT: Thank you, Jerry. Good morning, everybody, and good morning especially to the new members of the Board that I have the honor to be addressing for the first time today.

There will be official prepared remarks that you'll get copies of following, but I'd like to summarize some of the points here first.

Congress, as you know, endorsed our program plan in the 1997 Appropriations Act, and the President's 1998 budget request for the Program supports its continued implementation. With adequate funding, we will complete the viability assessment of Yucca Mountain next year and maintain the momentum toward geologic disposal as set forth in the Nuclear Waste Policy Act.

Congress is once again considering legislation to address the near-term management of spent fuel. The Senate has passed a bill, similar to the legislation that it passed last year, siting an interim storage facility and Nevada Test Site, with an alternate siting provision if the President, upon consideration of the Viability Assessment information, determines that the site is not suitable. The House is also considering legislation that would direct the Department to begin waste acceptance at an interim storage facility at the Nevada Test Site by January, 2000, irrespective of the information in the Viability Assessment.

As you are aware, the Administration opposes the preemptory siting of an interim storage facility near Yucca Mountain before the Viability Assessment has been completed. The Administration believes that a decision of siting of an interim storage facility should be based on objective, science-based criteria and should be informed by the Viability Assessment of Yucca Mountain. Consequently, the President has stated he would veto either bill if presented in their current form.

Despite its opposition to the current legislation, the Administration remains committed to resolving the complex and important issue of nuclear waste management. Secretary Pena has stated his willingness to work cooperatively with Congress on nuclear waste disposal issues. Whatever the outcome, the Federal Government's longstanding commitment to permanent geologic disposal should remain the centerpiece of the Nation's high-level radioactive waste management policy.

The near-term management of commercial spent fuel remains an important issue to utilities and others. On December 17, 1996, we formally notified the Standard Contract holders that we would be unable to begin accepting spent nuclear fuel at either a repository or any other federal facility by January 31, 1998. Legal action was subsequently taken by utilities, and the case is being considered by the court.

While the Department believes based on the contract that it is not obligated to provide financial remedy for the delay, the Department is willing to consider utility proposals to amend individual contracts to mitigate the impacts of the delay on accepting fuel.

Over the past several years, the Yucca Mountain project has been focusing on addressing major unresolved issues, which will be described in our Viability Assessment work that you will be hearing here for the next couple days. This will permit us, by 1998, to provide the four components of the Viability Assessment required by the 1997 Appropriations Act.

While the Viability Assessment is not one of the decision points defined in the Nuclear Waste Policy Act, its completion is expected to be significant to the development of a repository. The Viability Assessment will give policy makers key information regarding the prospects for geologic disposal at Yucca Mountain, and to justify its continued funding of the program if it is warranted.

The Viability Assessment also serves as an important management tool for the program. The development of the components will help integrate the ongoing activities and assembled information will guide the completion of the site characterization by identifying those areas where additional scientific and technical work is required to evaluate the site, to prepare a defensible, complete, cost-effective, and timely license application.

The general agreement between the program and its regulators on these remaining activities is central to the continuation of the geologic disposal program. This is especially important in an ever tightening Federal budgetary situation where so much emphasis has been placed upon balancing the Federal budget and reducing the Department's discretionary funding allocations.

The presentations later today and tomorrow by staff and our contractors from the Yucca Mountain Site Characterization Office will provide the Board with further details regarding the activities that support the viability assessment work. I look forward to hearing the Board's views on our plans and approaches so that we can appropriately address your concerns as we complete the components of the Viability Assessment.

In its most recent report, the Board notes that the regulations governing the spent fuel disposal should be updated because they are too detailed and were enacted too early in the repository development process. We agree. The public comment period on the proposed rule which was issued last December has been extended and ended on May 16th. We are presently evaluating the components, including those from the Board, for the next steps.

I'm pleased to report that we have made significant progress since the Board's last meeting. The speakers who follow me will describe our progress in performance assessment, engineering design, and site characterization.

We have completed the five mile loop of the ESF on April 15, 1997. From this point forward, the work in this facility will focus primarily on the thermal and hydrologic testing, and confirming our understanding of the rock where the repository would be constructed.

The Board has recommended an accelerated excavation of the east-west drift to obtain information on the west area of the current exploratory studies facility. We agree with the Board and are conducting detailed planning and an additional small-diameter exploratory drift to the west of the mail tunnel. This excavation will help improve our understanding of the rock characteristics and hydrologic processes that are important to design, construction, and performance of the repository at Yucca Mountain.

In our Waste Acceptance, Storage and Transportation area, we are focused on the planning for the long lead time activities that must precede the removal of spent nuclear fuel from reactor sites once a Federal facility becomes available.

During the past year, we developed a market-driven approach that will rely on the maximum use of private industrial capabilities, expertise and experience to provide the necessary services and equipment required to accept and transport commercial spent nuclear fuel to a Federal receiving facility. We are presently working to establish a competitive procurement process to award fixed-price, multi-year performance-based contracts.

Also to address the long lead time requirements related to a Federal receiving facility, we completed a non-site specific design for a centralized interim storage facility and submitted a topical safety analysis report for this design to the Nuclear Regulatory Commission staff on May 1, 1997. The staff has docketed the topical safety analysis report on June 10th, after completing their acceptance review.

Although the implementation of our revised program has refocused the program on key issues and maintained the momentum of the repository program, the products associated with the Viability Assessment will provide all parties, including the Board, a better understanding of the geologic disposal at Yucca Mountain, and the significance of the data that will then be available.

We intend to keep you apprised of our progress and look forward to a constructive dialogue as we carry out our mutual responsibilities.

Now, I'd like to elaborate a little bit about the significance of the Board responsibilities over the next two years, and the importance of the Board's activities.

Responsible government management of the Nation's spent nuclear fuel and high level waste is one of the most daunting tasks that we have. We are the leaders on this planet. We were the first ones to create this material, and we should be the first ones to have its ultimate disposition path forward. It's important to the world. It's important to this Nation. It's important to all the states, and especially the State of Nevada, where we're looking at Yucca Mountain.

I believe it becomes more significant as time goes on and the situation changes around us, and there are many changes that are happening today that impact on that, and I'd like to mention a few of those.

First of all, the Cold War is over. We have a lot of surplus fissile material that is weapons capable that must be responsibly managed in the near-term and ultimately disposed of. The ultimate disposition point for that material is this program. It doesn't matter if you're from the anti-nuclear point of view where the material should be disposed of in glass and immobilized. It doesn't matter if it's from the pro-nuclear point of view, returned into mixed oxide fuel. Either way, it must come to this program. There's no other place for it ultimately to reside.

If we are going to influence the Russians and the North Koreans about what they should do with their material, we should not be hypocritical and stand up and say you should do it the way we say, but we will do it other ways.

From the national security point of view, we do have a Nuclear Navy, we will have a Nuclear Navy for the foreseeable future. The Nuclear Navy produces spent nuclear fuel. The Russians produce spent nuclear fuel from their nuclear navy. If we do not have a clear path forward for the ultimate disposition of our national defense nuclear fuel, how can we go back and tell the Russians what they should do with their spent nuclear fuel, if we do not have a complete forward path for ours?

I think as most of you know, Al Lamm has announced an accelerated proposal for cleaning up of the DOE sites which we started back in the 1940s in World War II. That has a goal of primarily cleaning up those sites in the next ten years, being completed about 2006. 90 plus per cent of the toxicity that he will be stabilizing has to come to this program, so another reason why this program's success is important to the Department of Energy and all the states where the Department of Energy facilities reside.

Nuclear power is last, but not least, of the reasons why this program is important, and that is probably undergoing as we speak, and the next couple years, the greatest change since its inception. That is the change of competitive electricity markets and de-regulation in the electricity field.

When you met last January, there were nine closed nuclear power plants in the United States. As you meet today six months later, there are now eleven. It hasn't gotten much notoriety, but Commonwealth Edison, the largest nuclear utility, has announced that they are going to close the design plants, which were the first of the large thousand megawatt electric power plants, they're going to close those early because they're not going to put the capital investment into steam generators. Around 2002, those plants will start to close. This accelerates the need that this society and this generation move forward with responsible management of the spent fuel from those sites.

I believe it's important not only from a legal contractual point of view that the Federal Government discharge its responsibilities to receive that material, it is also a morally and intergenerationally important issue that we get on with that matter.

Now, the ultimate disposition aspect of that is the

Deep Geologic Repository Program, and I believe we're reaching a point with the Viability Assessment that there will be policy makers deciding where we go next. Those policy makers reside in our nation's courts, in our White House, in our Congress, and on Wall Street, and these are very important matters that we do the right thing for this generation as well as future generations.

One of the concerns that we must be careful about is I believe this program is a very fragile program. We are besieged from an extreme left, from an extreme right, and from well meaning groups and individuals in the center. The extreme left and extreme right are fairly easy to describe. The extreme left believes there should be no solution to this program, that we should suffer a little more for the mistakes of the past by going into the nuclear era, and that no solution is probably the best solution.

The extreme right thinks putting money into this program is money down a rat hole and we shouldn't do it. We should take that money and do other things like short-term interim storage, and we shouldn't be spending society's money. Both of these groups have their views of what the proper future societal constructs ought to be. The extreme left would like to take my discretionary cap and spend it on windmills. The extreme right would like to take my discretionary cap and spend it on better bombs. I think neither of those are the right solutions.

I believe the Board will have a very influential role to play when the Viability Assessment is completed. The White House, the Congress, the courts, the Wall Street will really make the decisions about if we continue this program, or if we search for a "better way."

I think many of these folks are well meaning within the Board and others, and I'll quote a remark that was published in Physics Today by a former Board member, Warner North, which many of you remember, and I'll just quote it. It's short. Okay? "We should not proceed reckless with high speed toward geologic repository. Rather, we should find better ways."

I believe that is a potential mortal danger that the policy makers will look for an easy way out about continuing on with an imperfect program, and this is an imperfect program. I think it's a good enough program, but it is certainly an imperfect program. But I think we need to be careful that where constructive criticism from the Board, which I think is vital and I think should continue and I think you should tell us better ways and things, that that not get misinterpreted in the policy making awls, is well, there's a better way and let's go back and let's start over from scratch, and let's go back to 1955 in the Hanford Tank decisions, and not proceed and find better ways that probably aren't realistic and do not exist.

I would be pleased to receive any comments or questions that you may have.

COHON: Thank you, Mr. Barrett. Questions?

Jared Cohon, Board. Lake, could you expand a bit on the program's view of the role of VA, and in particular, I think a key issue here is how to position the products of the VA appropriately vis-a-vis policy makers. And by that, what I have in mind in particular is representing it as the important milestone it really is, but at the same time, leaving room for what we know will be the case, which is that more work will be necessary beyond VA before you get to the point where you could apply for a license. That strikes me as a rather delicate line to walk, and I wonder if you could give us your current thinking about how that's going to be handled.

BARRETT: The Viability Assessment itself is not a decision. As we've mentioned, it's a compilation of information that we'll know at that time about how good and how bad Yucca Mountain would operate as a repository. It will talk about what its costs are. It will talk about what its performance is from a waste isolation retardation point of view, out to, you know, the million year time frame. It will talk about its uncertainties in science, what we know, what we don't know, and will be an accurate technical scientific exposition of what it can and can't do as we know it at that time.

That, coupled with what its licenseability is, in our view, that would be the licensing plan that accompanies the design and the performance assessment, and also the cost estimates on what it would cost to proceed to licensing and what it would cost for a total life cycle point of view.

Also, and contemporary with the Viability Assessment, we are also planning to do a total program-wide cost, TSLC, total life cycle cost estimate, and also a fee adequacy report that will assess what is the income and what's the state of the waste fund and the financial aspect. With that information, the Congress and the President can decide the next steps.

When we complete this, this will be the end of 1998, the President will be making his final decisions on the FY 2000 budget request to Congress. The FY 2000 budget request to Congress should go to the Congress in January or February of 1999. That will be several months after the Viability Assessment has been produced. There will be time for the president to receive input from wherever the President wishes to receive input, and I expect the Board may be one of those inputs, as well as others, and there will be a budget request to Congress.

Under the Constitution, the Congress will consider that budget request, will pass a bill, send it back to the President. The President will sign it or not, and we will then have policy guidance to go forward.

The Viability Assessment, and we just had a review for the last two days on that, and I think it's coming together quite nicely, we have found nothing that is a show stopper yet, but there's a lot of work left to be done.

This information, I believe, will be crucial to the policy makers if we should continue or should we go look for a better way. And, you know, there is construct under the Act; if the President believes there's a better way, he can make a report for the near-term and long-term management and proceed that way.

So the Viability Assessment, although itself is not a decision, it will really bring about a major policy debate I believe in this country about do we continue or do we say this was just science is not going to be good enough and that we should go back and try to find a better way.

COHON: Thank you. Dan?

BULLEN: Bullen, Board. Along those same lines, if you look at the pending legislation, there seems to be an emphasis on the Viability Assessment that makes it almost a suitability assessment, and I understand your description of how the process is a snapshot in time, but I'm interested in your thoughts on how the policy makers may actually take the

Viability Assessment and turn it into a suitability assessment, and it's probably not appropriate to do so if you don't think the data are available.

BARRETT: Well, it is not a suitability determination. A suitability determination is a lot more. A suitability determination has all the NEPA work that needs to be done, which will look at, you know, the transportation and a lot of the institutional issues and non-direct geologic repository issues that the Nation needs to consider when it makes a decision to really go forward. It has great political implications, meaning the President wishes to designate that site. The governor has a right to a veto, if the governor so desires. The Congress has veto override. So there's a big political component to that. So the Viability Assessment technical information is necessary, but insufficient for a suitability.

Now when it comes as it relates to interim storage, I think what the President has said is that we should really have the information on the viability of Yucca Mountain before any decision is made about site interim storage facility. What it boils down to, you can take the extremes on the thing. If Yucca Mountain is almost a sure thing from a technical, scientific, licenseability, cost point of view to be the repository, you'd want to know that. If it looks like it's a non-sure thing from a performance, from a cost and from a licenseability point of view, you kind of ought to know that if you're going to choose an interim storage site in its very basic form, although it's not the political statements or anything else. And I think it's a very simple statement; we should not enter and designate a site until we have that information in hand. And beyond that, it's going to be the political and the other considerations that Presidents have to do under the Constitution.

COHON: Thank you, Mr. Barrett.

BARRETT: Thank you.

COHON: Our next speaker is Steve Brocoum, who is Assistant Manager for Suitability and Licensing for the Yucca Mountain project. He's speaking on Viability Assessment, the description of the products and the schedule for completion. Dr. Brocoum?

BROCOUM: As I was getting up this morning and was getting dressed and have to make the commute downtown here, I was wondering why anybody would want to have a meeting in Las Vegas in June, and then I heard on the radio they were talking about the EPA clean air standards and that the President's making this decision, how the Administration is going to go, and it will be 97 today in Washington, D.C. with a red alert for air quality standards. So maybe it's better here than Washington today.

I'm talking about the Viability Assessment, the products and the schedule. I will give an overview on it. I'll briefly describe the products and the schedule, and I'll talk about some of the challenges in completing the Viability Assessment.

This is a statement out of the budget, the President's budget. "The completion of the constituent elements--those are the four parts--of the Viability Assessment constitute a logical convergence at which the program can make measurably improved appraisals of the prospects for geological disposal at the Yucca Mountain site. The assessment is an interim step in the process leading to a site recommendation." So it's just a step, in a sense, along the way.

It consists of four products: the preliminary design concept for the key or the critical elements of the repository and waste package; the TSPA, which will describe how this design will perform at Yucca Mountain based on all the available information; a plan and cost estimate for completing the license application, and there was a question to that earlier and, you know, this is part of the package which tells you what additional work needs to be done from the Viability Assessment to the license application; and then the cost to construct and operate the repository.

Together, these four packages as a whole give the policy makers the information that they need to decide how the program proceeds. It tells them what the repository will look like. It tells them out it will perform. It tells them how much more work for a license application, and it tells them what the whole thing is going to cost over its lifetime.

This is just brief outlines of each of the four products. We want to talk about the design bases, the functions, the requirements, what assumptions went into the design, what the concept of operation is, both during construction and operation. A lot of that occurs at the same time for many years. A general description of the site, you know, the setting, the physiography, the geology, the hydrology, certigraphy and so on. A description of the design for the surface facilities, the sub-surface facilities, for closure and de-commissioning and for Nevada Transportation. The engineered barrier system, the waste package design, the EBS design, the waste forms. Design alternatives that were considered, different thermal loadings, approaches to criticality control, and so on. And, finally, how the design evolves through the various phases. We have several phases in the design. I think Mr. Snell will be talking about that a little later.

So, basically, we envision this being documents, hundreds of pages long, which will describe the design. The final product is due in 8/98. The VA is due at the end of September of '98. A draft will be available in July of '98.

The TSPA. The key thing here in our TSPA is those often used words, the transparency and traceability, are very important to show how the repository will perform. We're putting a lot of effort in making sure that the end elements of the program, the scientists and the engineers have input and support the assumptions to do a TSPA. That's why we have the expert elicitations and that's why we have the abstraction workshops. The draft will be completed in June of '98; the final in 8 of '98.

The plan and cost estimate to the LA. That will include our overall strategy for development of the LA. It will give some legislative and regulatory, a framework. It will talk about a licensing strategy. It will discuss issue resolution and it will discuss the contents of the eventual LA. It will describe the work that needs to be done between the VA and LA in the site area, the design area, TSPA. It will have other regulatory activities, safeguards and security and our emergency plan. And there are other things that will be discussed such as how we intend to comply with 960. It will put the EIS in its proper context, and talk about the site recommendation.

It will include the cost and schedule for that work from the VA to the LA, and it will describe the performance confirmation program, such things as how we will confirm, if you like, the waste package and the EBS performance, the drift liner, the environments of the drifts, the near field and the far field environments.

The final again is due in 8/98. The VA is due in September of '98. Draft will be available in September of '97 and we will use the information in this draft to have some interchanges with the NRC so that as we complete our Viability Assessment, the NRC understands how we see moving on to the LA, and we have some interchanges and understand their point of view. This is to avoid surprising the NRC and to avoid, of course, surprising ourselves if the NRC has comments.

The fourth product is the cost estimate to construct and operate. It will, of course, discuss the assumptions. It will discuss the different phases, the design and engineering, the construction and placement, the caretaker, that's the period between emplacement and closure, the closure and de-commissioning, and Nevada Transportation.

For each of those periods, and by project element, it will give the costs by project element and by time period. That will be completed, the draft in June of '98, completed in 8/98. So all the four products converge in the August time frame in 1998.

So, in a sense, we have to complete four major products. I think that's unprecedented for the project, when I think back, the products we've completed. The TSPA will probably be about 1000 pages. The design almost that big. The license application plan probably 200 pages or so. This will probably be about 100 pages. It's a little early to give you exact numbers, but that's what it looks like.

The challenges. The first point was already discussed. This is not a formal decision. I always like to say the Department only has one real decision in the whole process and that's to recommend a site. That's the real decision that the Department has. This is not our decision. This comes out in '98. A draft EIS comes out in '99. A final EIS comes out in 2000. A site recommendation, a formal decision is in the year 2001. So it is not equivalent to the site recommendation, and I always like to say that site recommendation encompasses site suitability.

Again, four major products are being completed at the same time frame. We have to ensure integration among those products. And there's some sequencing here, because you need to provide input, engineering input and scientific input into the TSPA process. This fall, major input is going into TSPA for them to do their TSPA from the engineering side of the house and from the scientific side of the house.

We have to move ahead in the absence of an EPA standard. We don't have a standard. We're not sure when we'll have a standard. We know 60 will be changed, so we have to make some internal assumptions so we can move ahead.

We have to incorporate the current understanding of the site conditions. So as best as we can, we want the product that comes out at the end of September to be relevant and current. At the same time, we do have some, I don't want to call them limitations, but we have to feed from one product to another so that we will try to make this as current as we can, but there are some data cutoffs as you prepare all these products, and that's just the reality of the situation.

We have to appropriately assess the design options under consideration. Again, this is very important because for TSPA to evaluate the whole system, they have to know what kind of engineering barrier system design options we're going to choose. You'll hear a little of that again from Mr. Snell. And that has to be done earlier than September. It has to be done this fall for TSPA to do their job. So there is that logic issue to handle.

The last viewgraph I want to show kind of shows you the conceptual TSPA logic. And the reason I'm showing it is to help put in context what is being discussed today versus what is needed for the whole TSPA for VA. So we have the science and engineering program in this box, the various process models that are derived from science and engineering in this box, the abstractions, the expert elicitations, the waste containment and isolation strategy, which kind oversees, or in this case underlies the program, the external reviewers and oversight, the NRC, ACNW, this Board, the TSPA peer review, the analyses, documentation, and TSPA.

The things in red are the things we're covering today. So we're covering waste containment and isolation strategy. We're covering waste package and repository design. We're having an update on this from Larry tomorrow, Larry Hayes. Of course the two models we're having extensive discussion, and the abstraction process and the expert elicitation, UZ flow. So what is in red is being covered at this meeting, so I just wanted to lay that out to help people understand how it all fits in.

Those are my comments. Any questions?

COHON: Thank you, Dr. Brocoum. Dan?

BULLEN: Bullen, Board. Dr. Brocoum, before you turn that off, do you want to put that one back up? I have a question about it. One of the issues that we discussed yesterday in our overview of our Board meeting with respect to PA was the fact that on that centerline for process model abstraction, TSPA, VA analysis and documentation, and tied into the process models, there probably ought to be two-headed, don't you think? Isn't there a communication in both directions?

BROCOUM: Yes.

BULLEN: You show a flow direction there that says the process models just give you an input, and it goes.

BROCOUM: Yes. There probably should be various feedback loops through here. This is not a full logic diagram. All these diagrams are always imperfect, and I'm almost hesitant to show it because how important design is. Design is very important. And look at the visibility it gets on this diagram. So these diagrams always have a point of view and they're always somewhat flushed. The reason I showed those is to show you what we're covering versus what is yet to be covered in such a public meeting.

BULLEN: As a quick follow up question, you touched on design, and we realize that you're going to have to freeze your design a little bit early on to do the VA analysis. But in light of the fact that you're discovering more about the characteristics of the mountain, how flexible do you see your design being prior to the completion of VA, and then the subsequent--

BROCOUM: Well, the design is evolving. I'm looking for Dick. I can't see him in the audience here. He's hidden somewhere. There he is. The design is throughout the whole process. Certain things have to be frozen in terms of PA to do their job. Other parts of design move on.

Even those things that are frozen are revisited as we get input back from TSPA, for example, for the LA. There's is another phase that starts for the LA design. At the time of the VA issue, we start the second or third phase of the design for the LA. So there are iterations. But for the purposes of the VA, certain things have to be frozen. That's kind of what I meant to say.

COHON: John?

ARENDT: Arendt, Board. I have several questions. The first is will the VA include the design of the surface facilities? We speak of repository and I have difficulty sometimes differentiating between what's included for the surface facilities.

BROCOUM: There will be inclusion of a surface facility design, certainly enough to cost it out, because you need to have a cost estimate. And so you need to have enough design to enable you to do a good cost, and in fact there is a design already for the surface facility.

ARENDT: Right. The second question is you speak of critical elements. Roughly, do you have any idea what per cent of the cost these critical elements are? Are they 50, are they 90?

BROCOUM: I personally don't have a feel for that number, so I'd hesitate to guess. Do you want to say anything, or are you going to do it during your talk?

ARENDT: Yeah, if it comes later, that's fine.

BROCOUM: He will do it during his talk.

ARENDT: I'd be interested in what the critical elements are.

I'd also be interested in what kind of contingency you're using, or will be using?

BROCOUM: I didn't mention that. For the cost estimate, we will have a contingency shown. There is a contingency analysis and comparison with past cost estimates. In that cost estimate document, there will be contingency.

ARENDT: Okay, thank you.

COHON: Debra?

KNOPMAN: Knopman, Board. Would you put that chart back on? I know you have some hesitations about reading too much into this, but I did have a question because I wanted to understand the evolution of the Total System Performance Assessment.

On that same line where you go from TS VA analysis to the documentation to VA, the next step shows an update for license application. Where is suitability in that chart? And do you mean to imply that there will not be an updating, a full updating of TSPA at the time of suitability?

BROCOUM: No, we're planning a full update on the TSPA. So we envision another process similar to the one we're going through this year for the TSPA/LA, you know, abstraction, workshops and expert elicitations and that stuff. So we're currently planning a full update to the TSPA.

The suitability is encompassed in the site recommendation which occurs in the year 2001. It's right in here. A formal departmental decision on that is in the year 2001. There's a lot of things leading up to that. For example, prior to that, we are looking at 960, and we have a compliance evaluation against 960, assuming we get 960, of course, finished. So it would be encompassed in that line.

This was just meant where we go in the future, not to show all the steps. There are a lot of steps from here to here. You know, you could take this and you can make it a flow chart that goes around the room in detail.

KNOPMAN: All right. But for all intents and purposes, the TSPA update that you're doing for license application will be done in support of a site recommendation?

BROCOUM: That's correct.

KNOPMAN: That will be the product then?

BROCOUM: The site recommendation and license application are fairly close together. So we will probably have the same TSPA for the site recommendation and the license application.

COHON: Alberto Sagηϑs?

SAGγIS: Sagηϑs, Board. In your challenges, you mention moving ahead in the absence of an EPA standard. Would you expect the final version of whatever standard applies will seriously affect the overall result of your work?

BROCOUM: No, because this particular work here will show how the repository will perform, and then you can compare that performance to any standard you want. In other words, you can compare it to any potential future. So the work shows how the site will perform, and then you can decide, well, I'll compare it to some international standard or I'll compare it to a draft standard. So the VA itself does not depend on having a standard in place.

SAGγIS: But of course the overall outcome is going to be affected by that, or would you expect the standard to be such that--

BROCOUM: The place we would not be able to meet forward is when we do our compliance against 960, at least our draft 960 that we publish we say that we need to have the final EPA standard and the final NRC rule, although we still have an NRC rule. So the place that I would see that we would get hung up is when we went to see how the site complies with 960. That would be our stopping point.

Now, the engineers will tell you they would like to know what their designing to, so they want the standard yesterday because they're in the middle of a major design effort. So they will want the standard. When Jean Younker talks, the next talk I believe after the break, she will tell you what the interim standard is that we're using now. We're using an internal standard. But, again, if the EPA standard is very different, then there may have to be some adjustments.

COHON: Jeff Wong?

WONG: Jeff Wong, Board. Steve, can you go back to I guess it's overhead Number 4? You have the four components of the Viability Assessment, and in Number 2, you talk about the Total System Performance, describing the probable behavior of the repository. Can you expand on what you think that product probable behavior is? Do you think it's going to be the system is going to operate within a range of values?

BROCOUM: They will produce a series of curves and many different formats that will show you the range, the expected values, in a sense, the mean or median. It will show you the whole distribution. I think all that information will be in the--I'm looking at Bob Andrews here and he's nodding yes--all that information will be in that TSPA/VA.

Is that the question you're asking? I'm not sure I'm answering the right question.

WONG: I'm trying to get a feel of the product's probable behavior.

BROCOUM: Well, this is all in probability space anyway, so all these things are distributions and that's basically what we're saying here. It's a distribution, and where you want to look at that, you know, different people may look at the distribution differently from their value systems. I'm looking at the two people back here to see if they want to add anything to that.

WONG: Thanks.

COHON: That's probably right. Priscilla Nelson?

NELSON: Nelson, Board. Steve, I've got a couple general scoping questions. First, what kind of detail do you think that will be set up and given about how the repository is going to be operated in the context of preparing the cost estimate?

BROCOUM: There will be a, there is already, and there will be a cost of operations that will describe, you know, in a reasonable amount of detail how the repository will be operated. That will be part of the design. The cost of operations will be part of the design, and I think--I'm looking at Dick. Is that going to be described at all today? I think it's been described to the Board. We've had various presentations on cost of operations to the Board over the years, so I think there have been several presentations to the Board over the years on cost of operations.

NELSON: Okay. I imagine it's changed a bit from earlier presentations.

BROCOUM: You're correct.

NELSON: Okay, let me ask you this. In terms of the aspect towards compliance at some point in the future, to what extent do you view the work being done on the engineered barrier system components that are being developed, at least to my experience, as sort of separate entities or design components that can more or less be applied; to what extent is that really part of the strategy for achieving whatever compliance will ultimately be the basis for the comparison?

BROCOUM: What we envision is we will have a design that consists both of natural and engineered features, and we may carry options to enhance that. We may carry various options that would, we hope, improve performance, and as the debate goes on, what the standard is and what kind of a margin you want to have beyond that standard and all that. We then will have to make a decision where we go. Each option, of course, will add cost to the overall design and total system life cycle cost.

NELSON: In the VA concept, will these options be developed in the concept of the Total System Performance and how they will improve or enhance the system? Or would they be considered more or less as separate options?

BROCOUM: No, they will be options, but they will have a performance associated with them, and they will have a cost, certainly a cost differential associated with them.

NELSON: Okay. I have one general request.

BROCOUM: I should make a point. There are many permutations you can do on this.

NELSON: And they're not all independent either.

BROCOUM: You've got to pick a few that kind of show you the range of possibilities.

NELSON: Okay. Just in general, very often you've got on the previous, or I guess the last slide, repository/waste package design. I guess sometimes when I'm going through some of these flow charts, I end up wondering about the interface between, say, the natural and the engineered system, or the near field/far field, or science and engineering very often. It seems that the things are somewhat separate, and wherever it becomes possible to explain the nature of that interface, either in either of those three spaces of engineered versus science, near field/far field, natural and engineered, I'd appreciate it those could be highlighted.

BROCOUM: Those interfaces are very important, and sometimes are a challenge and we are very aware that we have to worry about interfaces. There's other interfaces also between the repository and the transportation system, so that is very high on our scope right now.

NELSON: Good. I'd like to understand that. And I guess my final question deals with to what extent does the project view this whole process, and actually getting the document into a publicly consumable and understandable form? To what extent does the project view that, and what plans might be laid so that next September, there would be a way of communicating the work done and the conclusions to the public?

BROCOUM: First of all, you know about the intense effort on the TSPA transparency and traceability. And, of course, we have a TSPA review panel. We have a member of that panel in the audience today, and they've issued us our first letter, their first letter report, which makes a lot of recommendations that we have to seriously consider. That's first.

The second thing, each of the four documents will have an executive summary which will be written, I assume, to be understood by an informed member of the public. The documents themselves, of course, will be technical documents and we will make them as readable as we can, but they are, by their nature, technical and complex documents. I'm not sure what else I can add to that.

NELSON: So there's no plan to actually more or less take those documents and translate them into something that the average lay person might understand what this document is about?

BROCOUM: I can tell you there's been a lot of debate about that, and I'd actually defer to the Director at this point.

BARRETT: Lake Barrett, DOE. The main focus is that these be good, sound engineering, science, well integrated documents, and not to go spinning up or spin down, nor try to put glossy covers on to influence that this should be stopped or this should go forward or whatever. So that's the focus, that's what the project's been told, stay on that JOB. The rest of that, and transparency principles and that, all apply.

As far as we have not really thought much about, let's say, the packaging, marketing or any of that sort of the program, of the Viability Assessment documents at this point, we have time to do that, you know, toward the end. But we do understand the need for people to pick up a hundred pages and what's this all about. Okay? So we're looking at how we're going to do that, but we've made no decisions on anything like that.

BROCOUM: Let me add one thing to this, though. There is, I talked about the TSPA, the other major technical document is the design. We have created what we call a Repository Description, the RDD, Repository Description Document, which is a short 50 or 75 page document. We just issued it. It has a facing page, a top page, it's in landscape orientation. The top page is a photograph or drawing, and the bottom page has a description. So you can go through the whole design for, again, for an informed individual to look at this. This is a description. It doesn't control the design; it just describes where we are.

That document will be updated every few months, depending on how fast the design is changing, and issued again. So that exists right now. We just issued that about a week or so ago. It's called the Repository Design Description, I think is the exact title, and I'm not sure anybody has a copy here, but we have issued it and I think it's getting printed right now.

So, for example, I think that will be--it's not part of VA, but it is something someone can pick up and say, gee, what's this thing going to look like and how does it work. So we have that already.

COHON: Norm Christensen?

CHRISTENSEN: Christensen, Panel. Just to be sure that I think all of us understand or have the appropriate expectations of what VA is going to accomplish, I think the name for some of us coming at it fresh might imply that there's going to be an up or down, viable, not viable kind of decision. And what I hear is in fact Mr. Barrett suggests that there were two possible end points, one of them being that this is a sure fire winner and very cost effective, the other being that it is far too costly and technically unfeasible, and the answer probably, given the uncertainties and the lack of standards at this point, will be someplace in between. So the expectation probably then is not yes, it's viable/not viable, but rather just simply a range of--

BROCOUM: Well, it's a trade-off. I mean, we're going to have a design. It will have a certain performance. If that good enough? It will cost so much. Is that cheap enough? So it's a very complicated, I think, public policy debate that will happen. That's correct. And you can increase performance, but you will also increase cost, and that is not a technical decision, it's a policy decision I think in the truest sense.

COHON: Russ McFarland? Never mind. Dan Bullen?

BULLEN: Steve, this could be a problem, having followed Lake Barrett's presentation, but I--Bullen, Board. I'm sorry.

With respect to the acceptance of the surplus fissile material and the Navy nuclear fuel, one of the issues that will have to be addressed in your performance assessment analysis will be criticality and the problems that may be faced when 10 CFR, Part 60.131 has to be evaluated. And I know Part 60 may be changing, but with respect to it as it stands right now, how do you address criticality or maybe conversely, how do you accept waste acceptance criteria?

BROCOUM: Well, I would have to turn to one of the technical people to explain that. If you notice in the TSP peer review report, they point out that that part, their recommendation is that the NRC clarify that. It's not clear if it's just for the operational phase or if it's--you know, there's a lot of issues there. It's a very complex issue. I don't know if it's going to be addressed at all.

BARRETT: Barrett, DOE. There will be in the Viability Assessment family of documents a safety story, let me say, for criticality, safety, which is going to be a combination of deterministic, probabilistic, you know, as that goes forward, as I believe had been presented to the Board sometime ago in the presentation. So we can do that again for you or the Board, but there will be more of an explanation of that.

BULLEN: Bullen, Board. Just as a follow up to that, but will there be a decision made as to certain wastes that won't be accepted based on the fact that there's a criticality issue, or do you think that you'll be able to design waste packages that will meet the requirements both near-term pre-closure and long-term post-closure?

BARRETT: I won't prejudge what the results are going to be. If we feel there needs to be like a limiting condition of operation, like a reactor tech spec., we will say that. If we believe we can accommodate it, we'll say that, too. And then if we say we don't know really yet until we do more refinement, which would come in the license application work, we'll say that also. So I really don't know, but we will address the issue of criticality safety in the viability assessment package and the follow on work needed.

COHON: Cohon, Board. I have some questions about sequencing, and questions as well about specific things that are expected to happen during certain times.

For each of the four major products that you showed us, there's a draft version and a final version, with some period of time between them. Let's start with the first one that you presented, design. The draft is scheduled for July, '98 and the final in August, '98. What happens in that month? To whom does the draft go? Who are you seeking comments from?

BROCOUM: That's fundamentally DOE review. That's I think the first that the DOE sees. That's a deliverable that we get from M&O. That's, my guess, the first complete version of the document that we see.

I need to say one other thing. We are doing our detailed planning right now, and part of our detailed planning is to issue management plans for each of these four products, and the M&O is going to give us those management plans on November 1st. In those management plans--see, right now, we just have a draft and a final--in those management plans, what we're expecting to see is chapter by chapter, you know, who the lead authors are and the reviewers, a lot more detail. So we're working up that detail now, so some of the questions you're asking, I may not be able to give you a very good answer at this point in time.

COHON: But the review in this case at least will be exclusively DOE internal review of that?

BROCOUM: Yes, it's designed for a DOE review.

COHON: Is that the case with all of these other drafts, or should I go through them?

BROCOUM: Yes, that's correct.

COHON: Okay. The major sequencing question I have is that the plan and cost estimate to LA, the draft of that is scheduled for September, '97, and as you indicated, your intention there I believe was to use that for a basis for your interchange with NRC to avoid surprises. But the Total System Performance Assessment draft is not scheduled until June, '98, something like nine months later. I would have thought that TSPA VA would be the primary basis on which, or a primary basis on which the plan and cost estimate to LA would be based. So, I mean, you mentioned the difficulties--

BROCOUM: The difficulty, if PA tells us they need 14 months to do a full PA, I think it's 14 months is the number, that's what they need, and so, you know, we're trying to give them--part of all the planning for this was scheduling the hand-off from the science and engineering to the PA people. That was a very difficult thing to do when we actually planned this program about a year ago, and we're now revisiting that in detail for '98.

So if there are surprises out of PA, then of course we'll have to make a last minute adjustment to that. You know, that's how this stuff will work in the real world.

COHON: Let me make it clear. I have great respect for the challenge that you face. I mean, this is really a grand problem that you have here trying to integrate and coordinate all of this. And your point about surprises and having to adjust, that's what this program is all about, or has been throughout its lifetime, and will continue to be. But given that we have, and I'll do a quick calculus here, something like I guess you said 14 months effectively to the end of VA, the work that goes into the VA, that you would have this gap, if you will, this interval of a whole nine months between when one product, that one product which is so dependent on the result of another is to be finalized nine months before that other one is finalized--

BROCOUM: No, it's not finalized. All the products are finalized in the August of '98 time frame. Okay? There's a draft of the license application, but it's not final. That's not final until August of '98.

Also, you know, PA has a draft in June. My guess is we'll be starting to get some, if not the whole document, some results earlier. So I think, yes, if you're thinking total system, you know, totally sequential system with no overlap and no communications, your point is good, but I think there's going to be a lot of--as they start to get their results, if there's anything unusual coming out, we'll have to immediately incorporate that into the other documents. That means it will be a management challenge week after week, especially as we get close to this, to do those kinds of things.

So I think in reality, that will happen. It's very hard to show that on your milestone charts.

COHON: Thank you for that clarification. Of course you presented it in that linear fashion, so that's how I would come to understand it. And your point, of course, is well taken, that draft does not mean final. That's why I used the word draft.

It's often the case, though, that when one goes to draft, the intention is that what I'm showing you is what I intend to be final, depending of course on what you say to me when you read my draft. Well, point made.

Other questions? Debra Knopman?

KNOPMAN: Just a quick question. Knopman, Board.

You talked about the DOE review of the draft. What provisions, if any, are you making for some kind of external peer review of these products before they're final?

BROCOUM: There is no formal external review of these products, but these products, and all the things that feed these products, are available as all normal deliverables are.

So once the deliverable comes in and it's accepted, it's normally available to the public.

In all honesty, if we wanted to go through a process like that, in my estimation, it would add a year to the process. And we have experience in that. We've done it for the SCP, where we did go draft, it was '87 or late '86, so it took one year.

COHON: Thank you, Dr. Brocoum.

BROCOUM: I just wanted to show this; I did get a copy. This is relatively thin, color cover. I just wanted to show you. And then there are diagrams on the top and descriptions on the bottom, for example. So it will give, again, the reader an understanding.

ARENDT: What's the date of that, Steve?

BROCOUM: June 5th, '97.

ARENDT: Oh, that's an update then.

BROCOUM: It's new. This is the first time, and it's Rev 0.0.

BARRETT: Barrett, DOE. That is the sequential actor, the document you have there, John, so it is a newer version, an updated version. It is more understandable to an educated reader.

COHON: Thank you. We will now hear from Robert Loux, Executive Director of the Nuclear Waste Project Office for the State of Nevada, who will be providing comments on

Viability Assessment from the State. Welcome, Mr. Loux.

LOUX: Good morning. I'd like to thank you for your invitation to be here today. I'm sorry I wasn't with you at your earlier meeting this year in Pahrump. Circumstances had occurred that made it really impossible, but on behalf of the State, I'd like to welcome you all here again.

I am the Executive Director, I think as most of you know, for the Agency for Nuclear Projects, which is currently housed in the Nevada governor's office, and we've been actively involved in reviewing and then participating in the program since the early Eighties, as some others in the room have been involved as well.

What I'd like to do this morning briefly is give you our view essentially of the Viability Assessment and hopefully try to put it in context as it relates to the rest of the elements in the program for you all, if I'm successful. Let me tell you at the outset that our view is the Viability Assessment is, by and large, a political document that has been contrived to serve a very specific political purpose, one which I think is perhaps obvious to many of you, since the questions that you've asked earlier have been very close to being right on that point.

Let me, in trying to explain this, put it in some historical context to begin with. As you all know from the Nuclear Waste Policy Act and the subsequent regulation that has developed therein, there is a very step-wise procedure for recommending a site for development as a repository, and indeed licensure. And, of course, those steps include, as you've talked about already, the application of the 960 guidelines for a suitability determination once the Department of Energy believes they have sufficient data and information to make that decision, and of course that leads to a recommendation to the President for development, and as I think Lake indicated previously, some role for the State to play at that point. But assuming that all that goes forward, then of course the application of the Nuclear Regulatory Commission for license, and the application at that point of the EPA standard and compliance with the EPA standard is a prerequisite to further development of the repository.

I mention all that because in our mind, the VA is designed to serve a purpose of trying to alter the entire regulatory scheme as we currently know it in the Nuclear Waste Policy Act. In late 1994, and indeed probably early '95, the Department of Energy worked hand and glove with the Senate Energy Committee to write and, indeed, put forward much of the legislation that is currently being developed on the Hill. And, now, I'm not talking about the interim storage provisions. We can leave those aside. Those are separate and apart from this discussion.

The most important parts of that legislation, apart from interim storage, from our perspective, are three-fold. Number one, the legislation proposes to eliminate entirely the site suitability and site recommendation decision by eliminating the need for the 112 siting guidelines that have been spoken of earlier.

Secondly, the legislation preempts and totally takes out of the EPA's hand the standard setting for a standard that the repository must meet. And, lastly, it puts in place in legislation what DOE calls the Program Plan, which many of you are familiar with. But, indeed, the Program Plan allows the Department of Energy then to build and operate a repository without having to substantially apply EPA standard for compliance till some hundred years of operation at some later point in time, Ostensibly, if it can't meet regulation at that point, something else will be done with the material, at least we're led to believe.

When the Department of Energy was working with the Energy Committee and, indeed, in the early 1995 time frame at the very first hearing, many of you know that Senator Johnson was perplexed that now the Department of Energy at that hearing could no longer support the bill, and he indeed remarked that, gee, you've worked with us hand and glove to develop this whole thing, and why can't you now. And of course the Administration and the White House had stepped in at that point and indicated that they were not going to support the legislation for some of the reasons that Lake had described earlier, the spacing of the interim storage well in advance of the knowledge of Yucca Mountain, but in part also because of these other regulatory schemes.

Well, the VA was developed by the Department of Energy very shortly after that hearing, and its purpose again, in our mind, is to serve a political end, and that is to attempt to make the White House and the balance of the Administration comfortable with DOE's program, sufficient so that they would indeed support the legislation that's on the Hill, and ostensibly now that's off to the 1999 time frame, based on the timing now of the Viability Assessment. And it's not perhaps the interest in interim storage, as it is in altering the entire regulatory scheme which is really at the heart of what the VA is intended to do by shoe horning, if you would, in the Administration into accepting the legislation and that part of the legislation that does alter the scheme.

The suitability of Yucca Mountain under the 112 guidelines, of course, under this legislation would be taken away. The EPA would have no longer a substantial role in standard setting, and once again the decision, the final disposal decision under the NRC regulations would not be made until some hundred years of operation of the repository.

As many of you know, early on in the program, it was very clear what the role of engineered barriers, for example, should and ought to be in the development of repository designs and schemes. And, in fact, it's underlined, the underpinning of the 960 guidelines and, indeed, the EPA standard as well as the NRC regulation that the engineered barriers are not designed to mask or otherwise compensate for otherwise less than perfect geologic or hydrologic conditions.

So in taking in full when we look at the Viability Assessment in total, it has as its purpose trying to get the legislation passed as its primary goal. I think the former Director had indicated that it's independent of regulation, that it's designed to make an investment decision, and I heard Lake say earlier today that the view of the regulators are key to the program. All of this taken in context suggests to us that the Department itself has some very serious doubts about the viability, if you would, of the site under the current regulatory scheme, and believes that it probably only can go forward under some altered regulatory scheme as placed in the legislation.

This is not to say that the State of Nevada is not taking the Viability Assessment seriously. We have, in the process of putting together a team to review all of the aspects of the Viability Assessment, we continue to plan to be very active in its review and in commenting on it, and we will, of course, again be very active with the Administration and others in trying to demonstrate what we think the true purpose is.

I think by and large, our view is that the Viability Assessment, aside from the political aspects that I think it serves, is going to be a document that is going to be unlikely to be relied upon for any substantial decision making in the program simply because of lack of data and a number of other problems associated with where we are with integration and a number of other factors currently going on in the program.

But, again, we plan to take it seriously and we'll be there at the table reviewing it, and we'll be, I think anxious to share with you on an ongoing basis our view of the contents of the assessment, and we're learning more every day, as you are, about when it will be available, what will be available for public review or not, as the case may be, and hopefully we'll be able to continue to communicate.

But I want to thank you for your invitation. Thank you for allowing me to at least give you our view, perhaps a little blunt, of what the purpose of the VA is, and hopefully have put what its role in the overall program is in context.

With that, I'd like to thank you again, and would be happy to answer any questions that you have.

COHON: Thank you, Mr. Loux. Questions? Paul Craig?

CRAIG: Paul Craig, Board. It's widely reported that the State of Nevada is unalterably opposed to Yucca Mountain. I wonder if you'd expand on that a little bit and explain to us the circumstances under which that statement might not be true.

LOUX: I think you probably have aptly described it. I don't think it needs any additional description beyond that. I don't believe there are any circumstances in which that view can be changed. The State has had a long history of working with the Department of Energy and a long history with the development of the facility itself, and more importantly, a long history with seeing how the political system has, in a sense, preempted the ability to make a scientific and technical decision about the site, or better sites, as the case may be. But I think you're correct. It is unalterably opposed, and I don't see any circumstances that would change that.

COHON: Cohon, Board. I'd like to ask you a question about suitability and suitability guidelines.

You stated the State's objections to the change in those guidelines, and a change from what they were, or are, I should say, to a proposal based on performance, overall performance measure. Other than the fact that it's a change, what's wrong with that?

LOUX: Well, I think there's two things wrong with it fundamentally. The first and very obvious one is it does not in our mind comply essentially with the requirements of the law. It does not meet the criteria specified in the law about what the guidelines ought to contain, the technical factors that would qualify or disqualify a site. That's fundamental and first and foremost.

I guess more importantly, or as importantly as that, is that when the law was developed and when the program was constructed early on, the idea of a suitability determination applying these technical factors with qualifying and disqualifying conditions was to set up a preliminary step by which we would know whether or not it made sense to continue an investment in this program on an ongoing basis, at which point in time, performance and these other kinds of measurements would be more appropriately applied. But, in fact, we found that the site had these technical factors, problems with certain aspects of geology or hydrology. Then it made sense to make a decision at that point about whether we ought to continue or not, without having the ability to mask those characteristics through performance assessment, by the application of various engineered fixes to these problems.

So I think that the Congress thought, and I think many of us who worked on the Act early on thought that the 112 guidelines applied early in the site recommendation process served a very fundamental purpose, and a fundamental purpose in applying specific factors related to specific geologic and hydrologic conditions that would not be masked or otherwise obfuscated by performance.

So that's the fundamental purpose why we think continuation of the guidelines as they are, and in order to know whether moving forward really made sense, in many ways it serves the purpose that we've heard today about Viability Assessment in some sense, although Viability Assessment in our mind doesn't give us a very clear and distinct picture of the site because of the application of performance assessment.

COHON: Well, sticking on the first point, if the overall measure, or measures that are used to assess the performance of the site include all of those specific technical factors that are in the current setting guidelines, and if the process by which that assessment is done is transparent so you can see the factors and how they contribute to the overall measure, why wouldn't that be acceptable? Isn't that an improvement over what we have?

LOUX: Well, I think if in fact it met, first of all, the clear direction in the law, in other words, the proposal was not in violation of the law, and secondly, if I understand your question correctly, we were able to see very clearly these very individual characteristics isolated by themselves, then I think that probably would serve the purpose of the guidelines as we see them.

COHON: I think just to add I think what probably is a fine point to this, if one steps back and asks the question and is interested in the answer, how will this site perform overall, then I think one can't help taking the step of combining somehow these individual technical factors into some overall measure. One should be able to do that. There's no reason in principle why you could not do it in a way where the connection between individual factors and the overall measure is understood and clear. However, there is a combining going on, and that can't be avoided.

LOUX: Well, in some sense, I think it could be in some way in the current, or the scheme as we have understood it for the last 12 or 15 years, the application of these various stages of the program sequentially with one following the other, dependent upon the previous result, I think leads you down a path that allows everyone to have a lot more confidence in what the final answer may be, and the process about how you arrived at it.d

The combination of or combining of these functions and steps, not only I think for those of us who are involved in the program, is going to be somewhat difficult to sort out the various pieces in the black box and understand it better. But clearly the public is going to have a very skeptical, if not more skeptical, view of all of this as it goes down the road. The view right now of course, not only in Nevada but elsewhere, by the public is that daily, the entire regulatory scheme is being changed to fit what the site conditions are, and this is, you know, violating the rules in everyone's mind.

So, clearly, if there was any way the public confidence could be more eroded than it is already, the combining of these processes and, in essence, creating at least the perception of a black box is going to cement that forever, I think.

COHON: Dan Bullen?

BULLEN: Bullen, Board. Your comment on the new legislation, the third point was that the program plan calls for building and operating with no EPA compliance for about a hundred years or so. And in my understanding, the EPA compliance is actually a long-term compliance for safety. Do you think that that has a significant impact on the health and safety of the public if they don't have that EPA compliance for that time frame?

LOUX: Well, let me try to put it in context. I realize that there are some licensing on the front end of this process that involves some aspect, and I was generalizing to some extent the previous remarks, but in essence, what happens is that you don't have a final decision about disposal, meaning the final compliance with the standard, until we've had this operating experience for a number of years.

The question that's out there in everyone's mind is what if at that point in time, you can't reach a conclusion about that you can successfully comply, what happens to the material at that point? What happens to the program at that point? The view is, by and large, that we're going to have an unlicensed repository operating during this period of time, which in and of itself is a concern. But at the final point, what happens? Are we led to believe the stuff will be dug up and moved somewhere else after this period of time?

Once again, it violates the confidence that in fact some of us had for a number of years in the current regulatory scheme where things were set up step-wise and deliberate where it was clear at every step of the process what the decision had to be made and what the follow on decisions would be made in the future about the site, dependent on the previous decision.

When all these things are sort of combined and meshed together, as was describer earlier, on the front end, and then you don't have the final decision occurring until after this operating period, again, the view of, by and large, the public is that we're going to have an unlicensed repository operating. No one really cares that the site meets certain criteria or standards, or they'd apply them early on and try to make a definitive decision before we had committed ourselves too far in the process.

COHON: Bill Barnard?

BARNARD: Bill Barnard, Board Staff. Bob, you mentioned that engineered barriers shouldn't provide a mask for an imperfect site. If engineering and engineered barriers are used to improve the performance of the Yucca Mountain site, does that, by definition, mean to you that the site is imperfect and unsuitable?

LOUX: Not necessarily. It seems to me that a decision about the site and its suitability, at least envisioned under the regulation as we understand it now, has to be made on the basis of essentially the site itself and the criteria that are spelled out in the 112 guidelines. You make a determination of suitability based on those decisions itself.

If in fact you pass that test, then it seems to me that there's an appropriate role down the role for enhancing performance with an engineered system, but as long as it's not used to mask these various defects, assuming that they occur, but in fact to provide another layer of protection and performance for the site, and that's the role that I've always understood and believed that the basis goes as far back as the NAS report on isolation of waste in '83.

COHON: Leon Reiter?

REITER: Leon Reiter, Staff. Bob, I'm thinking about your response to Paul Craig's question when you said the State is unalterably opposed to the repository. How should we look then upon research funded by the State, for example, the large amount of work done by Track Corporation and Jerry Szymanski? Should one look at that as ways to support your opposition to the site?

LOUX: Well, I would guess that it depends on your own bias when you take a look at those reports. If in fact they need to be looked at in the work the State has performed, in and of itself as a separate independent scientific endeavor, if in fact you're going into reviewing those things with the point of view that you've expressed relative to opposition, then you're going to have a certain bias yourself in looking at them.

I believe that the scientific work the State has performed for the most part by and large will stand up to work that has been performed by anybody else in this program, and in some cases, I think it's far more credible.

COHON: Other questions?

(No response.)

COHON: Thank you, Mr. Loux.

LOUX: Thank you.

COHON: We will now take a break and reconvene at 9:55 sharp. Thank you to all the speakers.

(Whereupon, a short break was taken.)

COHON: We turn now to the topic of Total System Performance Assessment. Dan Bullen, a member of the Board and the Chair of the Board's panel on Performance Assessment, will chair this session. Dr. Bullen?

BULLEN: Thank you, Jerry. And I'd appreciate it if everybody just grabs their coffee and has a seat.

In order to keep us on schedule, since we're five minutes late, I won't have any significant opening remarks, although I'm sure that puts fear into the eyes of the current Board, because I'm known to speak a great deal.

We do have a very interesting morning session on repository design and engineered barrier performance with respect to Total System Performance Assessment. The speakers that we have today include Dr. Jean Younker, who's going to talk about the waste isolation strategy, who will be followed by Bob Andrews, with PA aspects of waste package performance, and Richard Snell talking about repository design and operation.

The fourth talk with be Dave Stahl, who I'm going to offer apologies to because I know he's with Framatome/COGEMA and not with B&W Fuels. That's an oversight on the agenda and on his name plate, and I promise that that will never happen again, Dr. Stahl.

And, finally, we will close the morning session with Dr. Della Roy, who will speak to us about the behavior of cementatious materials, an area that's actually of significant interest to the Board with respect to ground support and performance during the operational phase, and then long-term performance with respect to repository performance assessment.

So without further ado, we'll open the second session this morning with Dr. Jean Younker talking about the waste isolation strategy.

YOUNKER: Good morning. Thanks for the introduction, Dr. Bullen, and it's a pleasure to have the new Board members here today. I found myself, as I was putting the talk together, wanting to refer back to previous briefings we've given the Board, and I noticed as Steve Brocoum was talking, that we have to get used to the fact that many of you haven't been in those previous briefings. So if I do that, I apologize ahead of time. It's just kind of become habit on our part, but we will try very much to rely on the fact that you have not been with us over the years, like the previous Board was. We kind of got used to them, so we have to establish a new way of interaction, and we're looking forward to that. i'm not saying that that's anything that we're not anticipating with great interest.

So my first bullet then says that in fact we did brief the previous Board, and some of you I know had begun to attend the meetings last summer, on the update to the top-level strategy which had been previously expressed in the 1988 site characterization plan.

We said in that briefing, and in the material, this "Highlights" document that was distributed in draft form at that time, that the reason why we were updating the strategy from the site characterization plan was because certainly over the years, we'd had improved site understanding with all of the site characterization activities that were ongoing. We had had a change in the fundamental design concept for the repository waste package system, to the more robust waste package design, and which then also directed our attention to what thermal loading effects would exist and potentially the value of thermal loading as a design option.

Similarly, we had done a couple of iterative performance assessments since the site characterization plan was issued. Therefore, we had some improvements in our ability to predict performance of the total system. And similarly to what you've already been talking about, we have an evolving regulatory framework where our initial thoughts on the site characterization plan and defining the way we would analyze the performance of the site was certainly more based on thinking about a release based standard, such as was in 191, now we're looking at most likely a dose based standard, which then causes us to think through and present the information in a different manner. So all of that led us to a point where updating kind of the overall framework and the way we look at the parts of the system made sense.

To go back and review for the new members, the utility of having one of these strategies like the one that we're talking about today is that it's provided a framework for combining the natural and engineered components of the repository in a system that will meet the performance requirements. It gets you in a position where you can improve your designs, looking at cost trade-offs, as Steve Brocoum mentioned, and allows you to also look back to the site testing, laboratory testing and support of those design options that helps you enhance confidence in the performance. So it kind of gives you that overall framework. I think it's given us a way to better explain and put into context the work that we're doing in this program.

The way I talk through this today I think is a little different than probably a year ago. I think the way we express it has been fine tuned over the past year. So if you'll follow through with me kind of the approach or the way we think about it now, we identify the site and design features that, when you take them in combination, are sufficient to meet the performance requirements. It's iterations between design and performance assessment, looking at the various design options, and Dick Snell will talk through this with you in a lot more detail than what I'll just mention them here, but looking at various design options that will improve performance, looking at them quantitatively and then of course apply a cost factor to that. As we talked earlier, the cost of those design and improvements will be a very important input to the policy makers who will have to determine what kind of margin on your performance you need in order to take this kind of a facility into a licensing environment.

Our performance assessment models get updated on the basis of improved site information and our engineering understanding that comes from looking at these design options. So we're moving then in this kind of flexible strategy that allows us to incorporate new information.

With that, we select a subset of site and design features to look at a cost-effective system design that meets or exceeds, depending on your preference for the margin, the performance requirements. On that basis, we then have what we are now describing as our safety case, and the difference between the strategy and the safety case, I think we're still working on the way to explain that.

But basically, we iterate as necessary to incorporate the new site and design information, and the little schematic that I have next in the package is one that we've, in the last few weeks, evolved to kind of help us think about the way this is working in our current thinking. And that is our site information, our design information, the options that we might consider just shown schematically here, like diversion barriers of some sort, or we've referred to them as drip shields. The Performance Assessment modeling that takes those design features and the site information, puts them into kind of a simple model or abstracted model of the repository design and the site we described as our waste containment and isolation strategy.

I'm going to walk through the elements of the current version of the strategy as we're thinking it right now. We don't have it completely written down yet, and in fact it's evolving I'm sure as we speak here today. But together, the way the results of that, or the way in which we are able to describe that is the thing I think we're referring to as our Safety Case, but it would be unfair for me to tell you that all this is locked in concrete in terms of the vocabulary, because it really truly is evolving as I speak.

Okay, in terms of the kinds of presentation features of our Safety Case that you'll hear Dick Snell talk about, we talk about preventing and delaying the radionuclide releases. We talk about mitigating the transport after the releases. So I think you'll see that our vocabulary that we're using has a little bit more of the flavor of the kinds of words that are used in a Nuclear Regulatory Commission atmosphere of Defense-in-Depth. Much of our thinking in the last year has evolved around that concept of Defense-in-Depth. Part 60 gives us a concept of multiple barriers that has conservatism, redundancy and margin, so we're trying to put our thinking into the words that our regulator will best understand when we present our Safety Case.

And, clearly, from Part 60, we are given the direction that engineered barriers should be used to compensate for uncertainties in natural barrier performance. Similarly, natural barriers are there to compensate for uncertainties in engineered barrier performance. And, of course, that drives you to the question how much do I have to characterize those natural barriers in order to take credit for using them to compensate for uncertainties in my engineered barrier performance. So these statements may sound simple at first blush. They obviously aren't simple. They spin off into a lot of questions about priorities and balancing between the engineered barriers and natural barriers.

Steve Brocoum said that for the time being, it was very difficult for us to not have some kind of defined interim standard for the engineering and performance assessment folks to aim at as we go through this evaluation of options. And so on this slide, we've written for you what our interim performance standard is at this point in time, provided to the project by DOE as just kind of interim guidance.

We'll use the interim standard that is a requirement that the expected annual dose to an average individual in a critical group living 20 kilometers from the repository shall not exceed 25 millirem, all pathways, all radionuclides for 10,000 years.

There's a goal that goes along with that requirement, which is to provide, as I mentioned, sufficient defense in depth to ensure the repository will satisfy the requirement, conduct the analysis beyond 10,000 years to gain insight into longer-term performance, and for this period, the expected annual dose to an individual of that critical group 20 kilometers away should be below the 10,000 year requirement.

So we have a goal to keep the peak dose whenever it occurs at 20 kilometers below the 25 millirems. We have a requirement for 10,000 years to keep it below 25 millirems. So this gives our design team and our PA analysts a good clear understanding of where they're heading, at least for the interim period until we have an EPA standard.

I might mention that, I think probably you're familiar with this, but part of the reason for the way this is cast is because of the NRC's proposed de-commissioning standard. I think DOE believes that this is consistent with or at least not inconsistent with what NRC has put in that proposed standard for de-commissioning.

I'm going to move now to the elements of the current version of the strategy as its evolving. This is kind of a break point into the specifics, so if there are any questions about the interim standard or anything I've said, now is probably a good point to ask if you want anything to this point. Are you okay on that?

BULLEN: Do you want to take questions now?

YOUNKER: Well, I just thought, because I move into now walking through the four elements of the strategy, and I wasn't sure whether this might generate any questions. If it does, it's kind of--we can come back to it of course.

BULLEN: This is Bullen from the Board. It did generate questions, but I'd prefer to wait till the end.

YOUNKER: That will be great.

Okay, what we're going to do now is walk through each of the four elements. I'm going to put a picture up over here. You've seen the picture before. It's changed a little bit, and in the words that I'm going to use, word slides that I'm going to use that follow to walk through what are now four elements, for those of you who are familiar with the blue book and the previous versions of the strategy, we had it broken into five elements. Now we have four, and you'll see there are some minor changes to several of the elements, and I've Italicized the words so that you can kind of see where there are some changes.

Before I do that, I need to talk a little bit about some of the questions that you've asked, which is how is the strategy being refined on the basis of new information, and the recent information on potentially higher percolation fluxes and better definition of the heterogeneities that we have in the natural site properties. Average percolation flux through the repository from one to ten, and you may see numbers higher than that when you see the results of the expert elicitation that's trying to pin down the extreme values, or the range of values.

We know seepage in the repository drifts will be less than that, but it's certainly going to be highly variable in space and time and difficult to predict.

Thermal effects will redistribute that moisture with slow return to ambient conditions over several thousand years, but we also know that how much redistribution we get and the time period that that occurs over depends very much on the amount of flux, the actual value that this flux is at any given point in the repository spatially and temporally.

And we also, I think, have a better understanding perhaps than a couple of years ago about the kinds of heterogeneities that are going to drive our calculations for taking credit for the various properties of reduction in concentrations during transport like dispersion and sorption and diffusion. So we know we're going to have some uncertainties there to deal with, some of which will probably remain irreducible.

So given that kind of new thinking, what do you do with it? Well, this goes right back to the logic I was giving you before, which is the selection of site and design features depends on their expected contribution to performance and those uncertainties that I just laid out.

The improved understanding of the moisture conditions, better definition of the spatial and temporal variability are used as input to our sensitivity analysis. Using those sensitivity analyses, we can then refine the site and design features included in the strategy. So this is part of that evolution and flexibility that I'm trying to communicate to you.

The way the strategy looks today, the natural barriers under Number 1, which is limited water contacting the waste packages, still the ones that are not Italicized are the same as what I would have told you a year ago, or did tell you a year ago. Natural barriers are the semi-arid, unsaturated zone setting for the Yucca Mountain site limits the net infiltration. I think that's a pretty obvious statement.

We do know there will be some diversion of the downward percolating flux above the repository. Exactly how much and what part of the net infiltration will become percolation flux and reach the repository and potentially become seepage is of course the part that has high uncertainty.

From the engineered barrier perspective, we know the drift wall provides a capillary barrier against seepage under certain flow conditions. We know that there are flow conditions under which, if you get enough fracture flow, enough water focused in a fracture, that you will potentially get drifting, and so that's one of the uncertainties, and you'll hear more about that I think when you hear some of the reports from the unsaturated zone expert elicitation.

We know the heat from the waste reduces the available moisture for some time period. But as I just said, how long that is is critically dependent on the flux, moisture content in the surrounding rock.

The one that's different here that becomes a part of the first element of the strategy, given the potential for the higher flux, is that we can consider some engineered diversion of the seepage that will enter into the drifts. And so we can look at the potential for, as I've shown on this chart over here, some kind of either drip shield or potentially even a coating on the package to further prolong the life of the waste package if it is encountering higher flux conditions than what we were assuming perhaps in the past couple of years as we put the strategy together.

Okay, moving over to the robust waste package part of the strategy, we use a corrosion-resistant inner barrier, corrosion-allowance outer barrier, same basic dual walled package that you've heard briefed before.

Galvanic processes may offer protection to the inner barrier. I think over with our waste package elicitation, Expert Elicitation Panel, as well as the report that Steve mentioned that's a draft report just received from our TSPA peer review panel, it's very clear to us that this is going to be a big question. Exactly how much galvanic protection you get, how much you'll be able to take credit for, what you can do to engineer better confidence in that galvanic protection is going to be an important uncertainty that we're going to have to address, and this statement, I suspect, is likely to evolve as you see the strategy in another meeting.

Potential for use of ceramic coating on the waste packages may prolong the life, as I just mentioned. And then use of backfill. Right now, the way we're considering the backfill, the major function that it plays is to protect these ceramic coatings if we did end up using either a ceramic coating or a ceramic drip shield, something that needs mechanical protection because it may be brittle and could be subjected to breakage when the drifts cave in.

We could, of course, also take credit for some limiting of advective flow to the waste packages and potentially below the waste packages by the presence of backfill. And that we did talk about in the previous versions of the strategy.

Okay, you can see on this one the mobilization concepts for the radionuclides are basically all the same. Some radionuclides we know the solubilities limit the mobilization. We know there are some key ones where that is not the case, so those remain important parameters, the solubilities of those radionuclides remain important parameters for input to TSPA.

We know that cladding reduces waste form surface area exposed. How much we'll be able to rely on the cladding being there at the time that we need it, 10,000 or 5,000 years into the future, what it will take in order to get regulatory credit for the cladding being there is certainly one of the issues that we're talking about right now, an important element being looked at in the strategy.

Long containment time limits alteration of the waste form. If the cladding does its job, and we know some of it at least will do its job, if galvanic protection works well, we're in a situation where when we finally do get breach of the waste packages, reaction rates are lower because temperatures are lower, so we're in an environment that's a little less hostile, both to corrosion of the waste package, and to alteration rates of the spent fuel or the glass.

Limiting the impact of engineered materials on water chemistry may also be useful to reduce mobilization. We're going to talk about that a little bit later, I think, the question of whether any of the engineered materials that we put in as part of the engineered system will in some manner either change the chemistry that would be either adverse or in fact potentially helpful to us to help keep the solubilities low.

Our fourth barrier now is a combined one. If you're familiar with the earlier versions of the strategy, we put the transport through the engineered system here, and the natural barriers out to the point where the water is withdrawn into just one element.

The engineered barrier portion of this, potential for additives to material beneath the waste package to delay transport. If there is a way that we can put in materials that will help us to keep the flow in a diffusive rather than an advective type of process, this would clearly be helpful, and so that's one of the things that we're talking about, not only that issue, but also just are there any kinds of chemical changes that you could induce by the kinds of materials you put there, are there any sorptive materials that you could actually put in your invert.

The use of backfill reduces the potential for advective flow. I did mention that before, and that's not new thinking.

On the natural barrier side of this, I think there's nothing really fundamentally different. Matrix diffusion in both the unsaturated and saturated zones will reduce concentrations. How much credit we can take for that is driven by the question of the uncertainties in the heterogeneities in the system.

Sorption will be effective for some radionuclides, and there are a couple of key radionuclides like neptunium where how much credit we can take for sorption along the flow paths is a really critical parameter.

Concentrations will be reduced when the UZ flow reaches the water table, mixing with the larger volume in the volcanic aquifer. We know mixing and dispersion during transport will lead to dilution. How much we can take credit for, how much testing it will take to be able to take credit for it is of course the key trade-off. And we know that additional mixing occurs at the point of water withdrawal. How much we can take credit for that is another one that's going to be an open issue for discussion.

Now, the overlay on all of this, we're kind of talking about the undisturbed performance of the system when I talk through the strategy. Of course the disruptive processes and events have to be looked at as kind of an overlay on this.

We know the early site screening considered the probability of significant disruptive processes and events, and we believe that we would have screened out the Yucca Mountain site if the probability was high enough that they were really going to be credible events. So, therefore, our current approach is to analyze the features and events and processes on the basis of their likelihood and potential effects.

We use our TSPA to evaluate the consequences for those limited number that are sufficiently probable that we do have to consider them, and we'll have to include the analysis in our Total System Performance Assessment presentations to the regulators.

So the question here is you have some cutoffs that are kind of precedent for us to use in the EPA regulations, but in this case, there may be some very low probability but high consequence effects and of course volcanism is a good example where we will do some consequence analysis, some limited amount, even though I think we believe technically that we're right on the margin of it being an incredible event, or an event that could be screened out.

So, in summary, the development of this strategy I think has provided an iterative basis for establishing this thing that we're coming to call our Safety Case. It has accommodated the evolving understanding of the site processes and conditions very well, I think. It's given us a basis for a systematic evaluation of design features and a way to describe what we're doing in terms of determining their performance benefits. And we're trying to build into it, the way we put it together and the way it will evolve, the flexibility to deal with the uncertain regulatory framework that we face in this program.

Thank you.

BULLEN: Thank you, Dr. Younker.

Questions from the Board? Craig?

CRAIG: Paul Craig, Board. I'd like to ask you to help me understand how to think about uncertainty and changes in expert opinions on how the mountain works. And specifically, I'm thinking of the kinds of non-linearities that show up when you're dealing with capillary flow. And you now have the situation where you've gone up substantially in the flux, with some of your experts saying that the actual flux may be even higher than that, and all of this is happening when you're in a regime where the equations are extremely non-linear.

If the expert opinion begins to shift, it may mean that there's a possibility at least that the engineering approaches that you're taking may have to get modified dramatically, and all of this is happening on this very fast time frame under which you're operating. Help me to think about that problem.

YOUNKER: Well, I think as a geologist, I go back to the fundamental observation that we have pretty good evidence that the site has not been saturated. I mean, we don't think the water table has been at least--it hasn't left us a signal if it's been at repository level. So you can almost start with an inverse calculation and say, okay, what kind of flux could I have on any kind of long-term basis, and not have the water table rise. Now, there's a bunch of assumptions there, too, of course.

But I guess if the flux goes up to some level, say, of tens of millimeters, we kind of have some confidence at least that we can still look at some of these diversion systems that we're talking about as being very real options. So, I mean, we kind of think of it as a combination of that uncertainty being the potential for looking at real feasible design solutions where you do divert increased flux.

And, you know, I still think our understanding of the system right now is that those episodically higher fluxes are pretty localized, and also both in space and in time. So

I think we're not expecting, unless our expert panel presentation surprises me, we're not expecting them to tell us that we're going to get flux conditions, average spatial flux conditions on the high ends of some of the distributions you're talking about. I think those are episodic and localized, in our best understanding.

Did that help?

CRAIG: A bit, yes. Thank you.

BULLEN: Dr. Nelson?

NELSON: Nelson, Board. A couple questions. First, there's no zeolites on your list any more. Are zeolites back burner stuff?

YOUNKER: Well, they were there in my--they're here. They're there. I think our concern on how much we'll be able to take credit for the zeolites goes back to Dr. Craig's question, and that is what are the flow paths. If the potential flow paths below the repository tend to be, particularly in the unsaturated zone, tend to be pretty much along fracture zones, then we may not get enough exposure to the zeolites, and proving that we'll get enough exposure to them could be a very difficult endeavor.

So the question, once again, is that trade-off. They're there. If we can show that some of the radionuclides will diffuse into the pores and get in contact with the zeolites, then of course we'll take credit for it. But when you look at the major contributors to dose in our current calculations, they tend to be technetium, iodine, neptunium, which are also ones, although neptunium may be absorbed more highly given some of the uncertainty in that right now than what we thought in the past, but technetium and iodine just are not going to be helped much by contact with the zeolites.

So, yes, they're still there. Yes, we'd love to take credit for them as much as we can. The question is what will we have to do in order to take credit to prove that the flow paths really are through the zeolites.

NELSON: Okay. I want to ask just two more questions. The first one is a general question about this is a mountain that's generally under a normal stress regime situation, and you can expect some stress redistribution around the openings that actually may represent some de-stressing of the rock that may actually have some opening of discontinuities at various locations, and that would tend to be a function of layout to a certain extent, and some things about shape and the other aspects of the repository design, which really overall is going to interact pretty strongly with the mountain itself and how the mountain is able to cooperate, because this is a fairly large facility relative to the mountain.

In terms of interacting between the natural system and the engineered decisions and the PA, it seems like that's one area where that aspect of how the mountain is responding really brings all three parts together. How is that being managed or handled in this?

YOUNKER: I almost want to say yes. Yes, we know it's important and, yes--I mean, the question of whether the mechanical response to the actual facility, you know, from the presence of the openings, whether the thermal mechanical effects will cause changes that are dramatic enough that we have to incorporate them into TSPA, you know, whether TSPA needs to reflect those changes in the system that we're modeling after the repository has been in place, is something that we are definitely aware of. You know, I don't know what exactly you're looking for in terms of a more specific answer.

NELSON: Well, would you see, for example, if this were identified, would you explore this prospect and perhaps identify this as being an important issue that feeds right back into performance confirmation decisions?

YOUNKER: Yes.

NELSON: If it's identified to be important enough on some basis?

YOUNKER: Yes, I think there's no doubt, I mean, if we do have a 50 or a 100 year monitoring time frame, we get a lot better chance to get a handle on at least some of that, because we go through a fairly major thermal pulse. But if it's just the mechanical response, I guess some of the tests that we already have done and will be doing will give us at least some local evidence for what kind of mechanical response. But the kind of thing you're thinking about I believe is fundamental changes in permeability such that you really will change the flow system.

You know, the answer I've heard given to this kind of general question is that this is a tectonically active area, and that over the ten million years plus that these rocks have sat here, they've been subjected to a lot of readjustments. I know this is your specialty, so I want to be careful here, but people have led me to believe that the kind thing we're doing to that mountain is not really very significant compared to what it's already been subjected to by nature.

Now, that may be, you know, a way over generalization, but we are aware of that issue. We know that we have to be able to answer that question.

NELSON: I guess the sense of if this situation does develop and you find it in PA, can you find it in PA and respond back to the engineers in design and to the science in terms of the in-field experiments that might plan down if this became important that there was an area of increased permeability in the vicinity, say, of the crown of these openings?

YOUNKER: Yes, we would expect the site testing that we're doing, both in the current facility as well as the ones that will be done in the new excavation, would feed that information to us, make us aware that it's something that's substantial enough that we need to incorporate it into our models, because we ask the questions about the kinds of things that could conceivably affect performance, but we expect from the site and the engineering side, the information back to you. So I think the process is there to do that.

NELSON: Okay. That would be interesting.

Just one last thing. We heard a little bit about PISA. How does PISA relate to what you're doing? Can you tell us what PISA is?

YOUNKER: Yes. It's basically an integrated document that tries to pull together the information that we need in order to support the Viability Assessment with the TSPA and the design, the LA plan, the cost estimates. It's really the complete set of integrating information from the science and the engineering design that supports those VA products.

BULLEN: Arendt?

ARENDT: Arendt, Board. Is depleted uranium being considered as a filler or barrier at all at this time, or do you plan on considering it?

YOUNKER: I've heard it discussed. I'm probably not the right person to ask. Why don't you hold that and ask Dick Snell while he's up here, and he can give you a more definitive answer.

ARENDT: Okay.

BULLEN: Russ McFarland, Board Staff?

MC FARLAND: Jean, a question. Earlier in your presentation, you make the statement that backfill could limit advective flow to the waste package. Later, you have eliminated the could and made it definitive.

Is there information, sufficient information presently to be definitive on that statement?

YOUNKER: Probably not, and you just caught me in an inconsistency. I didn't mean to be more definitive in one place than another.

MC FARLAND: Thank you.

BULLEN: Victor?

PALCIAUSKAS: Jean, in the early versions of the waste isolation strategy, a low percolation flux was envisioned. And in that time, a high thermal loading strategy was considered very favorable. You know, it would provide a high temperature, low relative humidity, and very long lifetime packages. Now with the percolation flux about an order of magnitude larger, clearly that's called into question whether you'll have that kind of environment. So where does thermal loading fit into this thermal strategy? Does it play any role? Or what is your thinking about it at the present time?

YOUNKER: We're certainly having lots of discussions. I tried to allude to that on the strategy statements. There's no doubt that we would still get local boiling temperatures, even if the flux is quite high. The question is how much water will we drive away. Will we mobilize it in such a way that we actually divert it from the system and it goes down fracture zones, say, between the drifts?

That's an area of high uncertainty. I think probably when Bob Andrews talks about TSPA, you'll see him going back through that in quite a bit of detail talking about the kinds of input that we need in TSPA to represent it. But, you know, it's still a consideration, the extent of which you will get dryout, the volume of rock that will be dried out. It's certainly a very important potential contributor to the strategy. The question is how will we reduce the uncertainties enough to know how to characterize it.

BULLEN: Knopman?

KNOPMAN: Knopman, Board. Jean, I want to commend you for a very clear presentation.

YOUNKER: Thank you.

KNOPMAN: Not giving anyone very much time to speak, and I know it's hard to condense all that you've done in such a short talk.

My question follows a little bit from Russ McFarland's, and it's just--it has to do with the engineered barriers, and while the intent is to impede transport, it also can, either backfill or some of the inverts, can impede drainage, which is a good thing before there's any kind of release from the package.

YOUNKER: Right.

KNOPMAN: Can you describe a little bit about how that trade-off is getting analyzed and handled? It's a temporal problem, and I'm just wondering how that's getting ironed out.

YOUNKER: In terms of the value of it for--

KNOPMAN: What sort of criteria you're using for making a trade-off between impeding drainage in the near-term, versus a long-term transport--

YOUNKER: I think that the main function in the current sensitivities for the backfill has really been just protection of any kind of engineered diversion systems. And so to go back, and we have done sensitivities in the past of the value of the backfill in terms of keeping the flow diffusive, you know, avoiding advective transport after anything is picked up, so I think we've done the sensitivities, to answer the question. When Bob Andrews talks, if you would ask that question again, I think you can get a better answer from him than what I can give you, except to say yes, we've thought about it. I'll defer to him.

BULLEN: Perizek and then Sagηϑs, and then as Chairman's prerogative, I'll hold my question until this afternoon and hope you're going to be around for a little bit of close out.

YOUNKER: I'll be here.

PARIZEK: I want to ask a question, this is Parizek, about the 20 kilometer values. I assume that's probably where spring deposits occur in Crater Flats. I know you have to pick some kind of a number and 20 kilometers sounds about where the--

YOUNKER: The 20 kilometer is pretty close to where we do have potential evidence of previous outflows from our aquifer. So that's one piece of data. The other one is the 20 kilometers is quite close to the Nevada Test Site boundary. It's the first place where you really do have public people living, although the actual kind of subsistence farmer that the regulator envisions is 30 kilometers down gradient. So there are a number of reasons why the 20 kilometers was chosen, but in part, because that is a point--I think the actual outflow evidence is around 23 or 24 kilometers.

PARIZEK: That raises a question about accessible ground water levels to wells in the future. If you have spring deposits that are that close, the water table could be quite shallow somewhere in Crater Flats and still be accessible closer to the mountain.

YOUNKER: Correct.

PARIZEK: So this is a biosphere issue.

YOUNKER: That's right.

BULLEN: Sagηϑs?

SAGγΙS: This is Sagηϑs, Board. Have you narrowed down the nature and size gradation of the backfill as possible options? Do you have a list of those things already set up?

YOUNKER: I sound like I'm really ducking your questions, but Dick Snell is going to talk about some of those options that are being looked at. I don't think we've narrowed it down. They are looking at a suite of options that would give us different potential performance. So sand, you know, crushed tuff, the kinds of things we've talked about in the past. Dick will go through a menu with you.

SAGγΙS: Okay. Because that could affect significantly the corrosion response of the container material. And I don't know if that is something that has been addressed in the expert elicitation for the corrosion predictions.

BULLEN: We'll save that one for Dave Stahl. And, in fact, it is my Chairman's prerogative I'm going to thank Dr. Younker and ask her to sit down, and I will ask my question later today. And I would also note that our Board is very talkative and we've allotted about half the time for presentation and about half the time for questions, and obviously we're carrying over, so I would just ask the future speakers to keep it succinct and we'll ask you lots of questions, I promise.

Our next speaker is Dr. Bob Andrews, who talks about performance assessment from the viewpoint of waste package performance.

ANDREWS: Good morning. Let me, with Dr. Bullen's recommendation, quickly go through some things, and then we'll cut to the chase and you can ask whatever questions you have about what is or is not going to be in TSPA for the Viability Assessment due in draft form, as you heard, in June of next year, one year from a week ago, some date on my calendar anyway, and the final due in August of next year.

I want to walk through our approach, and very quickly schematics and components that Jean's talked about, and then I think the main focus is on some key issues associated with EBS performance. And although I list a number of EBS performance issues, I think the directive was to focus in on near-field environment and on the waste package itself, so I will zero in and spend more time on near-field environment, in particular thermal hydrology and on the waste package itself, although there are other EBS component issues that impact total system performance, and then end up with a few conclusions.

First let's remind ourselves that we have a system here, and let me put up the next slide as well so we can really take full advantage of the facilities we have here. We have a system, and we've broken the system up into its individuals components, and in fact we've broken this discussion up into engineered component this morning, and natural component this afternoon, and a little bit more natural component tomorrow morning.

So we do have a system of natural features, and I have blown up the engineered components into various aspects within that component here. What we're going to be talking about this morning are those components that impact the EBS, which includes, of course, the site, because it's sitting in the site, and that site will respond to the presence of the facility. It will respond mechanically and thermally and chemically. And then the package and any other components degrade, the waste form degrades, and then there is ultimately EBS transport to the edge of the drift, and then of course the far field takes over.

This boundary between the engineering and the ologists occurs here, and occurs down here again. So there are a number of interface issues between engineering design and science, the natural system.

But what are we about to do? We're about to integrate all the relevant site and engineering information, design components to make a prediction of how this site may behave, potential long-term consequences and consequences now being defined with respect to the interim standard that Jean talked about, which is a dose, individual dose based performance measure. We will evaluate that expected performance for the reference design. There is a reference design that will be produced using representative models and parameters.

We will, as we have in all other PAs, evaluate the significance, i.e. the importance or sensitivity of a number of key technical issues, which are uncertain right now, and their impact on that total system performance, i.e. dose in this case. We'll use reasonable ranges in parameter values in the uncertainty analysis. I'm going to come back to this issue of variability versus uncertainty a little bit later, and it will be treated explicitly as stochastic process from point to point within the system.

And I think as Jean talked to and Dick will follow me, the benefits of alternative defense-in-depth designs will be evaluated in the viability assessment.

This slide more or less shows a key component, which is how water may move, and I emphasize may move through the system. A large fraction of the water, and we'll talk about this this afternoon, will be diverted just by capillary forces through the rock itself, but some fraction greater than zero is expected to seep. It might seep with a water film around the drift boundary, or it might seep as pendular drops. That's an uncertainty. The package will ultimately degrade. This particular schematic does not show the drip shield that was on the picture over there. The concrete invert will degrade. The cement or whatever other lining is used will also degrade with time, and we're looking at the time evolution of these degradations, the time evolution of water, time evolution of degradation of concrete, time evolution of water, time evolution of degradation again, and time evolution of degradation of the waste form and ultimately transport.

Just a point of reference, we've broken it up into these individual bubbles, but those individual bubbles have a direct correlation back to the waste containment and isolation strategy, and also have a direct correlation back to NRC's key technical issues. They're one to one mapped more or less.

Now, the sub-issues might be slightly different, phased slightly differently, but I want to point out that the key aspects are similar between the key technical issues from NRC and the waste containment and isolation strategy, and in fact their significance and incorporation in performance assessment.

Not to belabor the point, but each one of these components that feeds into the assessment of total system performance has a series of outputs, if you will. So you can expect, if you've looked at previous TSPAs, a family of plots that relate to each individual segment that impact performance. Starting with the near-field thermohydrologic environment, we are concerned with the humidity, temperature, liquid saturation and liquid flux inside the drift as they change with time, and of course as they vary with space. So I'm trying to bring out the stochastic aspects, the variability aspects here with the X,Y, and then of course the temporal aspects with T.

And these responses, as we'll come to in the following slides, are uncertain, so you can expect a range of responses of relative humidity, temperature, saturation and flux in space and time due to some uncertain issues that we'll come to. The same thing with near-field thermochemistry. The same thing with the drip shield itself, if there is one.

Focusing again on those that are of primary interest. waste package and thermohydrologic environment, the key information that comes out of the waste package degradation is the time of the initial opening or "pit" through the multi-barrier system, whether it's two or three barriers, whether there's a ceramic coating on it or not, the time at which the outside environment sees the inside of that package, and the variability in that in space, given that we have roughly, or would have roughly 12,000 waste packages, there will be a variability in that time.

And then it's also a function of the rate of pitting, not just the initial pin hole through the package that's driving performance, it's the distribution of how that package fails after that initial failure, which is time dependent and environment dependent. Again, all those other issues are on there for completeness.

So let's focus in on the near-field environment ones and the waste package ones, and I'll draw a tiny little effort to some of the others.

What are the issues or uncertainties, if you will, associated with the thermohydrologic environment? Well, first and foremost, in fact, the range of possible responses of the in-drift thermohydrology relative to temperature, et cetera, is a function of what the pre-emplacement hydrogeology is, and its variability in space, and in fact its variability in time with the onset of climate changes, et cetera.

Key amongst those is in fact the percolation flux, the average volumetric flow rate through the geologic media initially under ambient conditions. That will drive how it responds later on once heat is imposed on that system. And we're going to spend some effort this afternoon talking about this exact issue, so it's probably not worthwhile belaboring it here.

Another key aspect, as Jean pointed out, is the actual thermal design itself, you know, how close the packages are, how close the drifts are to each other, the size of the drifts, the size of the packages and the thermal output per package, the total thermal load area. That clearly impacts the thermohydrologic response of the mountain.

Another key issue is the actual variability in the thermal load, that there are going to be some hot packages, there are going to be some cold packages. This is kilowatts per waste package. And there are going to be some moderate packages, and there's obviously ranges between.

We in performance assessment, as we do with everything, will dispertize this distribution, and we fully expect to dispertize that, at least our current thinking, into three separate groups of packages, appropriately weighted by their inventory, appropriately weighted by the number of packages, appropriately weighted by the area in which they are emplaced.

Those of you that have looked at previous TSPAs knew we just took a representative package, a single point average package, average package thermal output used to derive the thermohydrologic response. Now we're going to use the variability in that and accommodate that.

The key point down here is, as well, as we'll talk about this afternoon, there's a range of reasonable alternative conceptual models of the natural system. You know, this is uncertain. The impact of that uncertainty on the thermohydrologic response hasn't been directly evaluated. It will be evaluated in sensitivity analyses conducted in TSPA/VA. And, of course, there's ongoing testing, as you're well aware, the small scale tests and the larger scale tests being conducted to gain additional confidence, whether that confidence is on line on June 15th, 1998 is uncertain, but there might be some discussion of those test data and how they compare to the models that have been used.

It's worthwhile to put up this one on the thermochemical environment, because Della Roy I think is going to be talking about this particular issue, the interaction of the ambient system and the ambient waters with emplaced concrete or alternative concrete designs for the liner.

In previous TSPAs, we've had no effect of changes in thermochemistry due to emplaced materials as they impact waste package degradation or waste form degradation or EBS transport. In TSPA/VA, that issue will be addressed. We're going to do some modeling, it's in fact been initiated, to derive the key geochemical parameters of water interacting with the liner, and then use that as a basis for revising the waste package degradation.

Skipping the next slide and going on to the waste package degradation, the first issue is clearly the waste package degradation is a function of the waste package design. And what we intend to do in TSPA/VA is carry the reference design. The bulk of the analyses will be conducted using the reference design. But we fully expect that alternative designs will be carried at some lower level of detail. That might be alternative materials. It might be alternative coatings. It might be alternative thicknesses of designs, et cetera, but the focus would be on the reference.

The key aspect here is the degradation rate--well, let's go through these one at a time because you wanted to focus on waste package. One key element in the degradation rate will be the percent of package surface or the percent of packages that actually see advective water, i.e. drips, because of the potential for salt buildup during evaporation, the potential for increased degradation as a result of that salt buildup.

So there will be a detailed assessment of the probability of seeps, and depending on the design and whether that seep is diverted or not, an impact of that seep and whether or not there's a ceramic coating on top, the impact of that seep on the degradation rate or the percentage of surface that sees seeps would be calculated.

The degradation rate of the corrosion allowance material, we do have corrosion allowance which is now mild steel modelled that's been based on literature and some lab data, analog sort of information. That model is being updated as we speak based not only on additional literature searches and additional lab data that have come in from Livermore over the last months and will continue to come in from Livermore in the next months, and we've increased, it's been a focus of the expert elicitation to evaluate the model used for the long-term degradation of the corrosion allowance material. It's a function of a lot of different environmental parameters. Those environmental parameters would be calculated and then fed into this model.

A key aspect here is, and one of the aspects in fact being elicited is the treatment of variability from package to package or from local mini-environment to local mini-environment within a package.

The potential for enhancing the degradation rate at either welds or by microbiologically induced corrosion, MIC as it's commonly referred to as, is being elicited from the experts. So that's part of the elicitation.

The galvanic protection that Jean talked about a little bit is also being derived from the expert elicitation, but there's some additional laboratory data to hopefully constrain that elicitation that Livermore has been correcting. The idea would be to use varying throwing powers, as it's called, or varying degrees of protection of the outer barrier on the inner barrier, and derive that from some values that are being elicited. Again, the variability in that would be treated stochastically.

The corrosion resistant material, there are some data here from laboratory data and literature values as well on the various corrosion risk materials, and it's also being enhanced by the expert elicitation as well to build additional confidence into the corrosion degradation model used for the corrosion resistant material.

Let me skip over waste form degradation and mobilization and transport. Those are other aspects of the EBS that are crucial to development of a source term for far-field transport. But in the interest of time and to allow you more opportunity to ask questions, let me just jump to the conclusions.

We've devoted a large effort to identify those significant aspects that impact total system performance as they relate to the engineered barrier system components. We use a number of avenues, you know, we've used previous TSPA studies, a lot of sensitivity analyses. We've of course hear from the NRC and see what they write, what they think about key issues associated with EBS performance. The Board has made suggestions in this area. We are eliciting some ideas on this, and in addition, we have our own TSPA peer review that had some comments associated with uncertainties and issues associated with prediction of EBS performance.

So all of these things have been used to identify those key issues that we've just walked through. We are addressing those issues within the context of TSPA, we I should say hope, because we haven't written it yet obviously, we hope transparently visibly, but time will tell how transparent and how visible that will be. But all these issues are being addressed within the current context.

We are taking advantage of expert elicitation in particular on one key aspect, which is the waste package degradation itself in quantifying some of the uncertainty, and then help us evaluate the variability and how to treat variability in degradation models.

And I just wanted to end with the fact that this long-term testing program that's ongoing at Livermore and Argonne and PNL in varying aspects of the EBS performance will continue throughout VA and post-VA, through licensing, and I think even in performance confirmation time frame. So we fully expect, you know, additional models will be developed, additional testing will be used to substantiate the models, or modify those models, et cetera.

So with that, let me stop and open it up to any questions you might have.

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

PARIZEK: Parizek, Board. How well understood is the availability of, say, air circulation in the mountain as it relates to really the performance of the individual barriers? I mean, you have access tunnels, you'll have placement drifts, and there's also the natural air circulation in the mountain that may somehow affect the behavior of these components.

ANDREWS: That's very true. The air circulation issue, the ambient air situation and including with the presence of the ESF, has been treated and accommodated in the current unsaturated zone flow model to the extent where they can predict transient pulses, pneumatic pulses, if you will, both small scale and large scale, and in fact, you know, mega scale, mountain scale pulses of air through the mountain.

So then that UZ flow model, which has been, if you will, calibrated based on those observations, is then used as the basis for predictions of the thermohydrologic response. And ultimately, once we have larger scale thermohydrologic tests, which you know now is the scale of the drift, turned on I think in November of this year, once those data are available, that would help confirm at a slightly smaller scale, you know, the air and heat and moisture redistribution. So we start with what we've observed, and then try to make predictions of performance from that point.

BULLEN: Cohon and then Craig.

COHON: Just a couple points of clarification. You showed a chart that indicated the significance associated with various key components affecting long-term waste containment. I didn't catch what the origin of those significance ratings were. Where do those dots come from?

ANDREWS: What we have done, you know, over the years, that's a little bit qualitative, in fact it's very qualitative. What we've done over the years in performance assessment is evaluate, you know, various components and various uncertainties and various designs, and we looked at then the significant components that were derived from that, you know, what really drove our performance. If we changed this or that, what really made a big impact?

COHON: So it was your assessment of the significance?

ANDREWS: Yes.

COHON: Okay. In characterizing using notation spatial variability, you used X and Y. Right?

ANDREWS: Yes.

COHON: Why not Z as well?

ANDREWS: Well, that's a good question. There's a little space issue in there, but also the thing is the repository environment is a spatial X,Y environment, you know, it's more or less a planar. We're not talking about a multi-level system.

COHON: I imagine we're going to hear from some hydrologists later who are very interested in the Z direction as well.

ANDREWS: Yeah, but in the EBS, you know, most things are at this plane, so I took the Z out. You're right; it's a 3-D system.

COHON: Okay. One other issue which might be purely semantics, but it may be more significant. At one point, it sounded like you were equating spatial variability with stochasticity. Did you mean to do that, or was it the case that in some cases, you were treating spatial variability stochastically in your model?

ANDREWS: Probably the latter is the better way of saying that.

COHON: Okay.

BULLEN: Craig?

CRAIG: Paul Craig, Board. A comment and a question. As you know, we've divided ourselves up into panels, and I was one of those who got the assignment of trying to figure out what TSPA was all about from a standing start, and I'm happy to report that while it's not like reading the Readers Digest, it is nevertheless true that after investing a couple of months with TSPA '95, I have the feeling I actually do understand what you are up to, and it was pretty clear. It's a good job, and keep it up.

ANDREWS: So one out of ten is not bad.

CRAIG: I'm really happy to report that.

Now, one of the issues which emerges there has to do with the sub-models or the process models, as you call them, and what you tended to do in TSPA '95 was to choose single models for each process. And, of course, that can get you into trouble if your models are wrong.

On the last viewgraph that you showed, you now indicated that you're going to use--not the last, but one of them--that you're now going to use a range of reasonable models. That looks like a really major change in the whole approach. It means you're not only looking at uncertainty in parameters, but uncertainty in models, at least for the waste package that you're talking about here. So the question is narrowly, what kinds of models are you thinking about for inclusion in the waste package portion, and then more broadly, what kind of an approach are you taking to think about different conceptual process models throughout the entire project?

ANDREWS: Okay, that's a big question. Let me focus on the first aspect, the waste package part of it.

As you're aware, we are using some experts right now to assist us in elicitation of a range of what they might feel are reasonable alternatives. And part of that might in fact be not a model per se, but a parameter within a model. Some of that we will try to treat as, if they feel it's appropriate, as variability. And if they think this is really an uncertainty, you know, that they can't distinguish between this or this, but they don't think it's a variability issue from point to point, then we would treat those alternatives explicitly and discretely.

Now, we hope that they weight those alternatives, but it may be that in fact they have insufficient knowledge right now to appropriately weight those alternative conceptual models of how this might behave or appropriate model. And in those situations, and there will be a large number of those, we would treat all, let's say it's three, all three models discretely and look at the impacts from a performance perspective of all three.

Sometimes we don't necessarily go all the way to total system performance. We might stop at, well, did this change my waste--even if I made alternative assumptions of the current models of waste package degradation, did it substantively change those two key aspects that I'm interested in, i.e. the initial pit time and the distribution of pits. And if it didn't change it substantively, then we would just document it, show the impact of that alternative model, and say it didn't make a difference.

If it does make a difference, you know, in the waste package part, then we would propagate those results through the system and essentially get two different PDFs, if you will, of net outcome, in this case dose. And that's the case not only in waste package, but in the flow system, and I think the example I gave there was on the thermohydrologic model.

As you're aware, there's been a lot of discussion of equivalent continuum and how you accommodate for fracture flow when you're doing a flow model. You know, do you try to treat them discretely? Do you somehow treat the fractures and the matrix distinctly? And so if it makes a difference in terms of the thermohydrologic response, which right now we don't know, I mean, I think we have some inferences that maybe it doesn't, but there's some other more important drivers like the thermal load itself, that swamp the uncertainty, if you will, in the conceptual model of flow. But if they do, we would treat both and look at the results of both, first from the thermohydrologic response point of view, and then say, okay, if there's a difference here in relative humidity and temperature and saturations and fluxes, then let's propagate those all the way through down to, you know, the biosphere.

BULLEN: Leon Reiter? Oh, go ahead.

CRAIG: One brief response. Thank you. Since you did use concepts of variability and uncertainty in parameters here, let me encourage you to be very careful about keeping those separate. They were confused in TSPA '95, and I had to ask a lot of questions before I was able to understand. Variability to me refers to differences in nature, the thickness of a layer changes from one place to another. That's real important in spreading out the dose over time. Uncertainty can be uncertainty in parameters, and you've got probability distributions, but uncertainty can also be uncertainty in models, like is the mountain uniform and permeable, or does it have big cracks so the water runs through something like a bath tub drain.

My question was focusing on models, which is significantly different from parameters and variability. And since you do have very different models which people are proposing for certain processes, it seems to me really important that you explore the important models and how they give you different ways of thinking about the mountain?

ANDREWS: We agree.

BULLEN: Leon Reiter?

REITER: Leon Reiter, Staff. First, I just want to amplify what Paul said. When Rick Anderson was here telling the Board about their TSPA, he said that the most difficult issue they dealt with was model uncertainty. I just wanted to amplify what Paul said.

But I wanted to discuss the solubility of radionuclides. In TSPA '95, one of the most important conclusions that sort of impressed a lot of people was that for many scenarios, the peak dose associated with any radionuclide was due to neptunium out in the order of hundreds of thousands of years. And, in fact, they may have been one of the reasons why the National Academy was looking at a million years. I know Bob Buttons is here. He can correct me on that.

A few weeks ago, I heard a presentation I think by Abe Van Luik at the ACNW. He said now that you are now assuming a solubility of neptunium a hundred times less than you were in the past, and as a result, this being the peak dose, disappeared, and now the peak dose is technetium and iodine concentrated between 10,000 and 20,000 years. This something the Board has been really interested in and we talked about it in our last report. Is this right? This is such an important issue. How are you going to ventilate this, sort of expose it to discussion? Because it really changes--

ANDREWS: Dick can talk about ventilation.

REITER: Because is changes many people's perception of the repository.

ANDREWS: That's true. I mean, there's been a lot of laboratory data at LANL and Livermore and alternative models of those laboratory data have been developed to try to explain ultimately the neptunium solubility in the aqueous environment that we expect to have there.

There was, and the Board was amongst them, a lot of critique, I guess, not criticism, but critique of the solubility values used in TSPA '95. In fact, a previous Board member felt we were way too overly conservative using some very short-term laboratory data, which he believed hadn't reached saturated values. So they were not at equilibrium essentially, the liquid phase and the nuclide were not in equilibrium.

We tended to agree with him. In fact, we did work with LANL and Livermore last summer to substantiate that yes, indeed, it probably should be a factor of at least, you know, 100 lower than what was used as the expected value in TSPA '95. It had a very wide range of neptunium solubility values. So now the expected value is moving down a little bit and that will be substantiated in TSPA/VA. Do we have something written now? Probably a white paper sort of thing, but not anything that would be an actual document.

BULLEN: Chairman's prerogative here, Leon. I've got to keep this moving along, and so I'll thank Dr. Andrews and we will move on t our next presentation. Corner him in the hallway if you get a chance, Leon.

Our next presentation is by Richard Snell, who's going to update us on repository design and operations.

SNELL: The subject here repository design and operations, and because of the time and because I know that you like to ask questions as opposed to watching a bunch of view charts, what I'm going to try and do is move fairly quickly through the presentation chart material, and then let you get at some of the questions that you've asked some earlier, and you'll have some probably as we go through this.

I will do a very brief review of where we are with the repository. This is a top view of, if you will, of the mountain. The area that's currently under consideration for the repository is highlighted here in the cross-hatch red area. The exploratory studies facility is here, north ramp, main north-south and the south ramp. North is at the top of the picture. And there's some interesting notational data on this chart, which I'll not review here, but you can do that in the handout materials that you have.

A quick look at repository operations areas, and this is showing both surface and sub-surface, but briefly, north portal operations up here, waste receipts coming in from this direction. The north portal operations area has both a radiation control area where the waste handling building, for example, would be, and there's a non-radiation control area where the administrative and support functions would be handled. Those are situated in order to take advantage of the natural wind conditions at the site and minimize problems with regard to releases. North in this picture now is to the left.

South portal development operations would be the location where the mining operations, the underground development would be headquartered and operated. What you're looking at here, again, is the exploratory studies facility, north ramp, main drift, and south ramp.

In the underground as it's currently envisioned, there is an emplacement area, exhaust shaft here for ventilation, a development exhaust shaft here for ventilation for the mining operations. And at the present time, development would begin at the north end and would move from north to south, and the present plan is that the emplacement operations would begin after a portion of the underground had been completed, that is, perhaps something on the order of 10 per cent, or thereabouts, of the emplacement area, maybe a bit more. We're still looking at that.

There then would be an isolation, and I'll show you this a little bit more, between the emplacement area and ongoing development to the south. There's an exhaust main that runs underneath the whole repository area, and spatially, we're talking about 1200 or 1300 meters across this site area here that we're looking at.

Briefly, the kinds of materials that are coming to us in the repository, we're getting rail and truck casks expected. The waste forms that we're getting are spent nuclear fuel canisters, assemblies from different kinds of power reactors, DOE spent nuclear fuel, defense high level waste. And from an emplacement standpoint, we're looking especially at a spent nuclear fuel disposal container and another contain for defense high level waste. This reference here to DC-5 is to a so-called five pack, that is five glass logs inside a container. And there's information here on the loaded weights on these individual elements, peak units per year, and there's some descriptions or definitions, rather, down at the bottom.

A little bit closer look at the underground and ventilation aspects, a bit of a blow-up of this picture that you saw here, but as I was describing, emplacement would begin on the northern end, development would continue to the south. There will be an isolation, a physical isolation between these areas so you don't have any possibilities for cross-contamination of air.

This is an active radiation controlled operations area, and will be treated accordingly. All of the conventional radiation control measures that one would use in any kind of a nuclear plant, nuclear operation, would apply. And that, for example, is in the ventilation sense, in terms of moving air into that zone as opposed to allowing air to be pushed from the radiation zone into outlying areas.

The concept right now is that we would probably have perhaps four emplacement drifts open at any one time during waste emplacement operations. The idea is that in order to implement the thermal loading strategy, which we'll talk about a little bit more, for the repository, we'll probably need to do some mixing of packages as we bring them into the underground, and alternate thermal output from these packages in the emplacement drifts. If we have four drifts open, it gives us some operational flexibility on how we place the packages, what sequences we use. So these drifts would be open, the isolation is here, underground mining and construction would be going on in this region here.

I'd like to put this one up on the other viewgraph and keep it up as a reference. I'm going to talk a little bit about the engineered barriers in some detail, and one of the things I want to do is show you this picture here. I like one that's got a colored background a little bit better, but this is a cross-section or partial cross-section. If you imagine that you're down at repository depth standing on the west side and looking to the east in the repository horizon, and you cut a partial section, what you're looking at here are three emplacement drifts in the reference design. They're 5.5 meters in diameter, and it's a relatively simple concept with a waste package in the center of the drift resting on a pedestal which in turn rests on an invert in the bottom of the drift. With the current thermal loading approach that we're using for this reference, the center dimension on the emplacement drift is 28 meters.

One of the interesting things about this, as simple as the picture is, is that there's a lot of space between those drifts. There's a lot of rock in there. So when we talk about moisture or water coming down through the repository horizon, it doesn't mean that everything that gets to repository depth goes to a drift. Lots of it can go right on by. There's plenty of room there, and that's an important thing to keep in mind.

Let's take a look at some design options, and I'll have to say at this point that this chart is incorrect, I'm sorry to say. It's the reference case. The reference case does not include backfill, which is pictured here. I happen to have an extra chart with me that I brought, and your handout, I'm afraid, has the one that shows backfill, but if you would, simply strike out the backfill. That is not in the so-called reference case at this point. The one you see here is the one I just talked about. It's the emplacement drift wall. This is a concrete lining, as we presently envision it, the waste package, the pedestal, the invert, and I'll talk a little bit about some of the features in the reference case, and then try and move through some of the other material, then we can get to questions.

Thermal design is one of the major drivers in the repository design, one reason being that it has a very heavy influence on how much real estate we need in order to emplace the waste. We've got a repository design requirement for 70,000 metric tons of material in the first repository, and we're currently looking at ranges of thermal loading in the emplacement zones of anywhere from 25 to as high as about 100 metric tons per acre. Currently for the reference design, 85 metric tons per acre is the selected emplacement density, if you will, in terms of how much material per acre.

That areal mass loading is this point here. That's controlled by spacing the waste packages. It's controlled in part by an imposed 18 kilowatt limit for package, and that has to do with local temperature considerations, impact on the rock and so forth, as well as others. So the 18 kilowatt high limit, and I think in one of the earlier presentations, there was talk of perhaps three different thermal loadings, an 18, a ten, and a two, I think, for defense high level, which is low heat released. That's one of the reasons we talk about multi-emplacement drifts and the ability to put wastes in in sequences in order to get our balanced thermal loading per design.

Looking at the rest of the picture, and I should mention here that down in the lower left-hand corner, you'll see a coding, stars, triangles, diamonds and so forth. The idea on those codings is that each of these features tends to bear on some particular aspect of the design. Controlling the environment in which the waste package lives, a star is one thing. A robust waste package, the circle. Limiting the mobilization of waste, the diamond, and radionuclide concentration reductions, the diamond. And each of these features on this picture are noted accordingly to give you a notion anyway of some idea of why they are there.

This is a very simple case, and think of the reference design not so much as the current total design, but regard it, if you will, as a starting point for coming up with a design which we believe will meet the requirements. As it happens, it does, based on the interim standard that Jean described for you. If you look at the TSPA analyses for this reference design, it does meet, or will meet, that interim standard, albeit with a very small margin. And one of the things I'm going to talk about is how do we deal with those margins, what does that mean to us.

Briefly then, and I wasn't so brief on that, I apologize, concrete lining, tentative design right now is we're carrying a three liner design, but a precast concrete liner for most of the emplacement drifts is expected. The location of the joints on that precast section is important because it may bear on seepage into the drifts. It is normal concrete. We're looking at several kinds of concrete and cements because of issues on cementatious materials.

There's an air gap in here which is just that, it's space, but it does give us some capillary barrier benefits when we look at moisture which moves through the rock above the emplacement drift. When moisture gets to the drift in the early stages, if the drift is still intact, tendency would be for moisture to move around the outside of this concrete liner. We know that it's going to degrade, and the longer periods of time as it does, gets on the inside. Again, you get a capillary tendency for flows to occur around the inside of the concrete as opposed to dripping straight down on the package.

The package itself, a corrosion allowance outer barrier, corrosion resistant inner barrier. Galvanic protection is included in this. I'll talk a little more about that. A large package in the drift. We have a sloped layout, a very modest slope, so that we do not pond water if we do get seepage and liquid water inside the drift during operations.

The pedestal I mentioned, the invert which has potential for additives, we can talk about. Zeolites exist in a layer down below the repository horizon, and as Jean mentioned, the value of the zeolites to us in terms of overall performance is still being evaluated. We know the zeolites are there. They do have the sorptive properties that are potentially beneficial. The questions really have to do with the fact that the zeolites have a relative high hydraulic resistance, first of all, compared to some of the other adjacent rock, and also the presence of fractures and to what degree the zeolites are fractured. If they are highly fractured, much of the water may move through the fractures as opposed to coming through the zeolites, and you don't get the advantage of the sorptive characteristics in large measure. But we don't want to disturb any natural features that potentially are beneficial, so we're controlling temperatures so as not to damage the zeolites.

Okay, let me talk briefly about the EBS in terms of philosophy and approach. What's it supposed to do? Well, it's supposed to work in concert with the natural barriers so that the repository meets the performance requirements. That's pretty straightforward.

Thinking of it in licensing terms and in terms of the reliability of the performance, it needs to be configured in such a way as to provide defense-in-depth, in other words, not just one feature, one thing, which if it doesn't work, all is lost. It's got to be something with multiple elements to it.

And, moreover, looking ahead towards licensing, it's a system that has to be defendable by analysis and by test in the licensing arena.

Briefly, the strategy for developing the EBS. We want to develop a set of operating for expected, if you will, conditions, and bounding conditions over the life of the repository. For example, water quantities, you've asked us about percolation flux and what does it mean to us.

Having identified that set, we want to characterize a family, if you will, of EBS features that we could use, a menu or a shopping list, if you want to put it that way. And then we want to use performance assessment sensitivity studies to perform evaluations of those features, and evaluations of the overall performance of the repository. And we're using combinations of those EBS features. When we compare performance with those combinations against the performance requirements that we have on the interim standard, then we have some measure of how well we're doing. Does this design work or does it not?

Okay, let's take a look at some of the kinds of things that we think we're faced with right now. You've heard in some of the presentations and you'll hear a good deal more I think throughout this program that some of these numbers or number ranges are still under development, so I'm giving you some numbers which have some variability to them. Percolation flux, for example, Jean I think, or Bob, I've forgotten which one, showed one to ten. Some of the information suggests one to 15. I put up one to 15 millimeters per year as a for instance on percolation flux. This is something that we're looking at, now six millimeters per year as an average. The one to 15 is a range, and it's based on what the scientists can explain better than I can, but they've looked at several methods of evaluating percolation flux, and those several methods give them answers in that range. Six seems to be close to a consensus number right now for an average.

We know we're going to get climate changes, both short-term and long-term, we think maybe 30 millimeters per year, for example, as a result of climate changes that we may see.

We're going to get variability in the percolation flux distribution. Rock is fractured. Fracture patterns are difficult to predict and they're varied. As a result of that, we expect we're going to get some focused flow conditions in the underground. We won't get a nice uniform percolation flux through the whole repository area because of fracture patterns. We're going to see water focusing locally, and then moving through the system accordingly.

We expect to get episodic behavior, and there's a lot of questions here about to what degree does percolation flux at the repository depth follow episodes that occur at the surface, or in the regions up in the infiltration zone, higher up in the mountain. If they tend to follow those episodes fairly closely, we're likely to see them down low. They're probably going to be moderated considerably by the rock, so we're still working on what's the fidelity as you go down through the mountain. But from a design standpoint, we feel it's prudent for us to deal with those possibilities.

We're going to look at seepage into the emplacement drifts. I talked a little bit about that on that cross-section. And what we're going to do is we're going to look at these EBS features, develop them in some cases, and evaluate them both from a performance standpoint and from a cost standpoint. We're doing some tentative evaluations now because we'd like to put our efforts into those performance features that promise to have the most value to us. We don't like to spend a lot of time and resource on things which PA tells us have marginal or insignificant benefit. We like to identify those with the major benefit, and concentrate on those, and a good deal of work is being done here.

I make this point because it's mentioned a lot before. In order to do this, there's a lot of interaction between the site people and the scientific data, performance assessment and design, and the process is an iterative process. You just don't go do it once and walk away. We're talking to PA every day, and the same thing with the scientific people. You have to do that in order to make this kind of a process work, and it's normal for design.

Okay, let's take a quick look at some of the options. Now the backfill is back on the chart. You can compare this to the chart that's earlier in the handout. The only differences between this chart, once you take the backfill off that other one, the only differences between this one and the earlier one are those things which are highlighted in yellow on the chart, and specifically cladding credit, which is one of the things that we can take benefit from. The cladding credit is an element which gives us a key performance parameter for the design. The information we have now suggests that if we have temperatures in the fuel of over 350 degrees C., we run the risk of damage to the cladding. So the design premise is keep the centerline temperatures in the waste below 350.

What the cladding credit, if it's intact, has great potential benefit for us, so if we find other elements that also bear on cladding performance, we will be careful to deal with those appropriately also. We know that some of the cladding that comes to us from the utilities, for example, has defects. The defect rates were suggested in another section we had, and might be on the order of 1 per cent. We're using right now for PA evaluations about a 10 per cent assumed inadequacy or failure on cladding that comes to us and is put in place, and that covers other things in addition to any deficiencies as we receive the material.

Ceramic coating is another element. Looking at the outside of that package, the two layer package, the ceramic coating potentially has long life performance, good water resistance and it's another feature under consideration.

Backfill does a couple of things for us. It provides rock fall protection, for one thing, because over long periods of time, we know the liner is going to degrade. We know that the ground conditions are going to be such that we're probably going to see rock falls in the emplacement drifts. If you don't have anything there and you are relying on a ceramic coating in a waste package and rock can fall and cause chipping or cracking or any other degradation on the package, you begin to lose those kinds of performance. So backfill has significant potential benefit in terms of mechanical protection.

It has another benefit in that it has some thermal characteristics which may be advantageous to us as well. More particularly, it's quite a blanket. It does provide for a large temperature differential between the package and the surface of the backfill. Because the backfill surface temperature is a good deal lower than the temperature of the package, when you use it, one of the things that seems to be beneficial is that evaporation and condensation activities which we expect below, or in the emplacement drift, those condensation and evaporation cycles tend to occur on the surface of the backfill, not on the surface of the waste package, because it's at a higher temperature. The fact that they occur here means that salt deposits get left here, not on the surface of the package. The corrosion envelopes that we're looking at suggest that if those processes in the deposition of these salt deposits is occurring here rather than here, the corrosion, the basic corrosion performance on the waste package materials is substantially better.

Drip shield is another option we're looking at. There are several ways to do it. This one is shown being supported on the waste package. There are versions of it that take you down so that you support it off the invert. There are other shapes, you know, roof type shapes, and so forth, which can be used. We're working now with PA doing evaluations on models of this performance, and what we're doing is we're taking the reference design as a starting point, as I said, and then PA is doing sensitivity studies and saying okay, if I have cladding credit, how much better does it look? If I have ceramic coating, how much better does it look? How much better does it look if I add backfill and a drip shield?

I've got a more complicated picture which would boggle your mind. I didn't put it in this presentation. But it's got, oh, I don't know, probably 10 or 15 more things that are highlighted in yellow, and that's not the whole sweep. The message is there are lots of things that we're looking at here.

Okay, briefly, performance over long time frames for the EBS, first of all, we're looking at non-project test and empirical data where we can find it, relevant data. We're looking for natural analogs for the materials and processes that we think we may use. We want to get the most effective use out of test programs that we have in the system already. We're doing a lot of laboratory materials tests, which you will hear about from Dave Stahl. Drift scale test was referred to earlier. That is a full size test, that is, a size that matches the emplacement drift as we expect it right now, about five and a half meters in diameter, has the concrete lining, has over 6000 channels of instrumentation to tell us what's going on in the package, the rock, the liner and so forth. That data is going to be very helpful to us.

And then looking ahead a bit, the performance confirmation program, there's some planning going on now in a performance confirmation program that would be implemented later on, but that program would include information on the emplacement drift liner, the concrete liner, on the waste packages and the EBS features, and the environments inside the drift in the near field and in the far field over long periods of time, 50 to 100 years perhaps. And those kinds of time frames provide a good deal better platform for forecasting extremely long duration performances. So a six month test on material is one thing; a five year long duration test is better. If you can get 50 or 100 years of data, that's a lot better yet.

I'll stop there and ask if have any questions.

BULLEN: Thank you. I'll take Chairman's prerogative and ask the first question this time, since I've deferred previously.

Could you put the design options for waste isolation design features, that last one with the yellow highlight, back up?

SNELL: Sure.

BULLEN: I have a question about the analyses that have been completed to support these kinds of selections, because if you're going to take credit for cladding and add a ceramic barrier and add a drip shield and add backfill, have you done the thermal analysis to make sure that you don't have some mutually exclusive conditions here that you've heated up the waste package, if you've got an 18 kilowatt package, to such a high degree that you won't have cladding credit or you'll lose out on some of your performance parameters? Have you checked out the interplay, I guess is the question?

SNELL: We have to a limited extent. We have not done as comprehensive evaluations as we will have done by VA.

But, yes, there are some key temperature parameters that prevail. I mentioned one of them; 350 max on the cladding credit. There's a 200 degree max temperature on the waste package surface--excuse me--200 degree limit on the liner, the emplacement drift. And we've also got a temperature limit that protects the zeolites. So we're working within those confines.

The intention on any design feature option that we pick is that we pick a feature or identify a feature in such a way that it's not subject to a common load failure, first of all, with any other feature. We're looking for independent behavior, because we're talking about defense-in-depth, and in order to have defense-in-depth, we've got to have features that behave separately and are not subject to failure for the same cause.

So I think the answer to your question is yes in a limited way. I don't expect, for example, that--well, I do expect that these features could, for example, be used in combination, yes.

BULLEN: Okay. Other questions from the Board? Sagηϑs first, and then Nelson.

SAGγIS: Sagηϑs, Board. I presume that the concrete will be non-reinforced concrete?

SNELL: I believe it would be reinforced. But, Dave, you're nodding your head.

STAHL: Yes, it's reinforced with stainless steel needles.

SAGγIS: Okay. That, of course, introduces another material's durability, question as to the use of reinforced concrete with stainless for such a long period of time may be a little bit doubtful. And that would be in an arch type of construction then, or would it be all reinforced together?

SNELL: We're talking about a precast segment, and the joint designs for the precast mating features have not been fully detailed. But it's an interesting question because the top head center of the emplacement drift and where you position the joints as you assemble precast sections, there might be circumferentially a total of four or five segments. Where you put the joints becomes important because we're probably going to, in effect, control the failure mode for the emplacement drift. Clearly, it's going to last I think, the emplacement drift, for a long time, but it's not going to last forever. It's going to degrade and fail eventually, so we want it to fail in a controlled way.

SAGγIS: Because if the reinforcement corrodes, it would--if it corrodes an extremely small amount, like a thousandth of an inch, that's enough to crack the concrete cover, which would cause falls of the lower part of the arch segments, which would fall down in the system, and so on.

SNELL: It's an interesting comment. We might consider other forms of reinforcement, for example, you know, fiber types or carbon filaments or things of that nature. There's a number of other options that could be used for reinforcing materials in concrete.

BULLEN: Nelson?

NELSON: Nelson, Board. Dick, I'm curious as to what extent you can carry the option of not having a drift liner? Thus far, it's in the reference case, and really evaluating the impact of not having a liner, the consequence of not having a liner on the ultimate performance of your repository?

SNELL: It's certainly something to consider, I agree. And, frankly, I don't think that we have up to this point in time. The reason that we haven't is that in discussions with the repository consulting board and others as the design has progressed so far, they've been concerned that unless you had a pretty good ground support system, it was going to be difficult to maintain the underground emplacement areas for the sufficiently long periods to get the waste in place and keep it open until you can conclude the period during which you have to consider retrievability.

Are you thinking of just rock bolts and mesh, or nothing at all?

NELSON: Well, I can think of both of those things. But it occurs to me that, I mean, if we're talking about a cost trade-off, this is certainly something to consider in terms of understanding what the impact of not having a liner is, and what does that mean in terms of retrievability or of long-term performance, and it may actually make some things easier to document. Different kinds of data may actually be required in order to address that issue in terms of the rock mass stability which might not be acquired if the commitment is there for a drift liner system. It's a rather expensive component of the facility, and it just seems to me that it might make sense to carry that liner-less option.

SNELL: I think we might do two things, and when I talked about doing cost benefit evaluations on the features, we should do one on the liner itself as opposed to no liner. Let's do that and let's take a look at it.

BULLEN: Cohon and then Parizek, and then we're going to cut it off.

COHON: Cohon, Board. I'm especially interested in coordination among the various groups working on this project.

SNELL: Yes.

COHON: And what you had to say about thermal loading assumptions is a specific opportunity to explore that issue with what we heard before. You said that the maximum assumed loading is 18 kilowatts per package.

SNELL: Yes.

COHON: And I heard you say, Andrews, Dr. Andrews, that in your analysis, you were using three different values, two, ten and 18, and it sounded like these were numbers taken as representative of some probability distribution, and that you were treating it stochastically. I'm hearing people say no behind me.

ANDREWS: No, that's not quite correct. I mean, it's variability in the actual receipt of the waste. We expect them to be a range of actual thermal outputs. You know, the defense wastes would be at the low end, the 21 PWR case would be at the high--

COHON: Okay. And so you're not dealing with the fact that the actual heat output will vary by package within each category of package?

ANDREWS: No.

COHON: This represents different kinds of packages?

ANDREWS: right. So the difference between 16 and 19, for example, would not be considered; it would be 18, 10, 2.

COHON: Okay, thank you.

BULLEN: Parizek?

PARIZEK: Parizek, Board. I have a question about the liner, whether you have something packed around the outside of it which would also serve like a capillary barrier role, or is that just going to be open space?

SNELL: Frankly, we haven't gotten into that yet. It's a good idea. There are things like coatings, packing, other things around the outside that potentially have merit, and we need to take a look at that before we get--

PARIZEK: Well, there could be other values too in terms of if it would last long, that is, the concrete liner would last long, it could be like a drip shield, serve as a drip shield. Is that also being planned for its value, to see whether it will last very long? Would it serve as sort of a backup to the drip shield?

SNELL: The liner itself?

PARIZEK: Yes.

SNELL: The concrete liner?

PARIZEK: I mean, surely as a shunt to water flow, except for where you have joints, but you could plan where to put the joints to minimize the chance of leakage through the joints.

SNELL: Well, we've not done that yet. That is our intention, is that where you put the joints is very important, and what the joint details are also can be important. And the emplacement liner itself we know has a limited life, relatively short compared to some of the other components, hundreds perhaps of years compared to thousands for corrosion resistant materials. But that's a very important benefit because the thermal cycle in this thing gives you relatively high temperatures for relatively short periods of time. You peak in 10 or 15 years and temperatures start going into a temperature decline, and find in 50 years a significant decrease, 100 years more significant. At 100 years, you're down to 100 degree C. temperatures as opposed to 200 degree C. temperatures. So that is something we look at. It's a good comment.

BULLEN: Thank you, Dick.

In the interest of moving on, we'll move to our next speaker, who is Dr. David Stahl, who's going to talk about waste package design and materials. And as I look at the clock here, Dave's already into his question period, so maybe we should just ask him questions as opposed to letting him give his presentation.

That was a joke, David, you get your full half hour. Go right ahead.

STAHL: I'm David Stahl. I'm manager of the Waste Package Materials Department. I'm going to talk this morning mainly about waste package materials, materials concerns, uncertainties, how our work interfaces with waste package design and performance assessment. I will cover a little bit about the waste package designs to set the stage for that discussion.

The mission that we have in the Waste Package Materials Department is to do testing and modeling that provides a technical basis for waste package design and for

PA. Most of the effort is conducted at the National Laboratories. As Bob Andrews had mentioned, most of the waste package EBS work is done at Lawrence Livermore Lab, and the waste form testing work is done at PNL and Argonne National Laboratory.

This is a slightly more detailed picture of the engineered barrier system. What we've shown schematically here is a PWR package, a BWR package which shows the basket, and in this case, defense high level waste, in this case it's a five pack, with the potential for an insert which might contain DOE spent fuel.

The packages, as has been indicated, are about five and a half meters in length, about 1.8 meters in diameter, and they rest on piers or pedestals about one and a half meters in spacing. What we show here schematically of course is a much shorter waste package to waste package spacing that was indicated by Richard Snell in the previous presentation.

I just want to give you a little overview of the materials that we currently have in our reference design. We have an outer barrier of ten centimeters of carbon steel. We have an inner barrier of roughly two centimeters of Allow 625, which is nickel base alloy. Then we have basket material, which is made up of carbon steel interlocking grids with the stainless steel boron inserts which provide control.

I'm going to skip through these. Basically, it just indicates here some of the waste package considerations that we have in our material studies. These are the kinds of things that we need to know and control in corrosion allowance materials, and these for the corrosion resistant materials.

We also have some considerations in the basket material. We need to provide the long-term performance for criticality control, and in EBS materials, a focus on compatibility with the other materials and the ability to retard radionuclide migration.

This is kind of an overview of the environment assumptions that we use for the testing program. We assume early hot, dry conditions, followed by cooler, more humid conditions, with the potential for dripping of concentrated groundwater onto a limited number of waste packages.

We have a very conservative testing approach, in that we're looking at water chemistries ranging from 10X to 1000X J-13, pH ranges from 2 to 12, and temperatures of 60 and 90 degrees. In fact, in some of the newer testing that we plan, we're going to be looking at some concrete modified water as well.

We do have consideration of the higher water flux could reduce actually the concentration of ionic species of water contacting the package, depends on how rapid that rate is and when that occurs.

So the point made is that the corrosion degradation is more closely coupled to the local conditions at the surface of the waste package, and not necessarily the condition of the water coming in from the repository.

Test environments include controlled and equilibrated relative humidity, water line and complete submersion. And I'll talk about that a little bit later. We do plan some drip testing work that will start next year.

We have a whole host of different materials in addition to the reference materials that we're testing, different corrosion allowance materials, intermediate corrosion resistant materials, and corrosion resistant materials. We're looking at other materials as well, the 304 and 316 with and without boron for the criticality control effort. We're looking at Zircaloy added to support Navy testing, and we're looking at ceramic coatings, and I'll talk a little bit about that later.

This is our waste package materials test strategy. It hasn't changed very much over the years. In the absence of time, I won't go through the process. But it is iterative. We have developed detailed plans. We are developing models and performing tests. We do have input that we have provided to PA and will continue to provide to PA. We will get expert input as part of the waste package degradation elicitation and other sources, and this indicates the kinds of tests that we are performing.

This is a summary slide to just give you a flavor of the kinds of tests that we're doing. In engineered barrier materials, we have container materials testing, long-term tests for the most part. Crack growth tends to be a little shorter test. Electrochemical potential testing, both short and long-term, and microbiologically influenced corrosion.

We do have basket materials corrosion testing going on. We have ceramic materials testing. There should have been a bullet over there. Other engineered barrier materials and degradation and abstraction modeling should have been bullets. Sorry about that.

And we do have waste form testing which was indicated by Steve Brocoum. It's not a major focus of this particular panel meeting--Board meeting, I should say, but these are the kinds of tests that we're doing.

Now, the next few slides talk about some of the near-term results. Let me just put up another picture here. This shows the long-term corrosion test facility. We have twelve tanks in operation, with a variety of materials, four tanks that have corrosion allowance materials, two with the intermediate, and six tanks with the corrosion resistant materials, as I mentioned, operating between 60 and 90 degrees.

Some of the new Board members have visiting Lawrence Livermore Lab where these tests are going on, and it will be my pleasure and Bill Clark's to host a meeting at Livermore so more of the Board members can see the facility.

Roughly, you can see at the top there are panels which are racks, rather, which contain the materials. I'll show a picture of that. There's about 200 gallons of water in each of those tanks. This is one of the racks. You can see there are different kinds of specimens. We have some crevice specimens and standard specimens as well.

These are some of the samples that were removed from the six month test. These are kind of before and after shots. You can see that there has been corrosion here, but when you look at the bottom line, as I indicated, over here, the range of expected values--it is in the range of expected values, that is, about 80 to 110, that should have been micrometers per year. I apologize for that one. The three to four mills year is also, that's correct.

These are just an example of some of the other tests that we're doing, and I'll pass on those for the moment.

Oh, one thing I do want to say. I'm sorry, I did want to show some of the results on the electrochemical potential testing, because this is interesting, at least to me. This shows the difference in response and surface appearance of materials conducted in acid brines, as you can see 90 degree C., and you can see that the 825, which was one of our early reference materials, is severely degraded under those conditions. Alloy G-3 less so, G-30 less so, Alloy C-4 just one or two indications. C-22 and titanium grade 12 were not attacked under these conditions.

We're also doing some MIC work, basket material testing that I mentioned, and ceramic material testing. We've done some drop testing in our drop tower at Livermore Lab, coated steel up to two meters using 100 kilogram simulated tuff rock. It did not produce visible coating damage. We did increase the load and we did produce some flaking of the coating. So our goal now is to look at just what those coatings can withstand and compare that with the rock falls that we would predict in the repository.

Just a few words in regard to the EBS testing. We are looking at the impact of thermal treatments on different concrete formulations, also going to be looking at the modification of the water that would drip through ceramic. And I believe Professor Roy is going to deal with some of those issues. This again highlights some of the work that we're doing in waste form testing, and we do have a significant materials modeling effort that's ongoing. We've made considerable progress in the last year, and we're working closely with performance assessment to give them the models that they need for TSPA/VA.

I'll try to wrap up quickly. Many uncertainties have been developed, identified in the process, certainly in regard to corrosion allowance material. One of the critical things is pitting under high pH conditions, which one would expect early in life for water dripping through the concrete liner.

We're also looking at microbial corrosion and what conditions would be required for that to occur and what its impact might be.

Preferential attack of welds, and certainly the effectiveness of galvanic protection is a very important item that we'll be studying in this year and next. Durability of corrosion resistant materials, localized corrosion is the critical thing, particularly crevice and pitting corrosion. that's high on our list. In this particular instance, we will have samples coming out from the long-term corrosion test facility in the July/August time frame, so we'll have an opportunity to provide some data to TSPA/VA on this issue.

And certainly the extrapolation of degradation rates to long term, that's a tricky one. One of the things that we rely on of course is mechanistic modeling for that, and some analogs if you can find them for these materials. Unfortunately, corrosion resistant materials, most of these materials have not been around very long, usually in the range of 20 to 40 years, with these nickel base materials, so there's not a lot of analogs that can be used for that.

Let me close with a slide on the interaction of program activities. We do have significant interaction, as I mentioned at the onset, with design. We have frequent meetings with performance assessment on model inputs and the test results that we've achieved. We are receiving input from the experts from the Board, from the repository consulting board, from the TSPA peer review panel, and the Waste Package Degradation Expert Elicitation Panel.

And very important, the overall objecting of these interactions to ensure that the testing and modeling that we're doing are consistent with design and performance assessment needs.

So I'll give my voice a rest and we'll entertain questions.

BULLEN: I'll exercise the Chair's prerogative again and ask Dave the first question, which I'm sure he knows what it is.

I notice that in your corrosion allowance barrier materials, you're taking credit for radiolysis protection, and in the high power loading case of 18 kilowatts per package, and in your testing program for humid air environment, I don't see a radiolysis test there. I was wondering if there's plans for that, and if you could sort of illuminate the program?

STAHL: Yes, we do have a test to confirm what we have already predicted in regard to radiolysis. If one looks at the standard origin codes, one gets a flavor for the output of the materials, the function of time. We've done shielding calculations which indicate that for the current reference design, we're in the range of about 50 r per hour on the surface. The literature that at least we're aware of doesn't indicate any problem at that level. Certainly if it were higher, it would give us a significant problem. And that dose is going to decay rapidly with time, and we feel that by the time water can come back and deposit on the surface of the package, that the dose would be significantly down by three or four orders of magnitude. But, again, we do have a program in '98 that will be looking at that.

BULLEN: I'd like to see that data. I've got one more quick question. You showed us the waste package with the spent DOE fuel, and I didn't get a chance to ask Bob Andrews this, but in TSPA/VA, how do you address the criticality issues associated with that type of waste package, both from the degradation in package and outside the package? And my point of perspective is as reactor manager at Iowa State University with aluminum clad fuel that I don't think is very good to put in the ground. So that's the perspective that you're getting here by this question.

ANDREWS: Dave can probably answer this as well as I can. This is Bob Andrews, M&O.

All criticality issues, I think it was raised also earlier this morning, whether those are, you know, spent fuel related or other waste form related, will be addressed and the consequences addressed at least to as reasonable a fashion as we can.

BULLEN: I guess the question I have is that in TSPA '95, the criticality was sort of done off line.

ANDREWS: TSPA '95, there was no criticality.

BULLEN: No criticality, okay. Do you plan on doing it off line in the VA, or do you plan on having it as an integral part of the analysis for the repository?

ANDREWS: I suspect it will be an off line sensitivity study evaluating the potential consequences of that potential event.

BULLEN: Okay.

ANDREWS: With the appropriate weights on that potential event defined by, you know, kind of a fault tree sort of methodology.

BULLEN: I understand the consequence analysis, but I guess the other concern that I have is, as I mentioned this morning with Lake Barrett, that unless the K effective of .95 has changed, then you've got to really worry about what happens with respect to those kinds of waste forms going into the mountain.

STAHL: Let me add that there's a significant effort in Hugh Benton's department to look at criticality control in DOE, SNF cases. A variety of cases have been already looked at, and others will be examined in the next year or so.

BULLEN: Other questions from the Board? Alberto?

SAGγIS: Yes, Sagηϑs, Board. The corrosion allowance material, corrosion resistant material combination, is that pretty much fixed for the latest TSPA that you're going to be preparing, or is there possibility that that would be changed to a more normal material?

STAHL: Well, we certainly have accumulated new data since that particular decision has been made, and we will, through Hugh Benton's operation, be performing another revisit to material selection for VA. So that will be frozen at the end of this fiscal year. But we will put out another report that deals with the materials for each of those barriers, and we'll take another look at the use of the carbon steel and the 625 specifically.

SAGγIS: But for the TSPA, for the final TSPA, you are pretty much frozen at the external carbon steel, internal corrosion resistant material; is that right?

STAHL: Not necessarily. If the data moves us to make a different selection, then we'll move in that direction.

BULLEN: Nelson?

NELSON: This may show some ignorance on my part, because I know the materials are different in the past than they are at the present, and certainly the new ones that you're considering. But I saw the analog column on your chart and Dan informs me that in many cases, the analog has come from a study of the petroleum industry, some applications. I'm wondering from the standpoint of working underground in an environment similar to what Yucca Mountain is likely to be, to what extent is the Nevada Test Site and the tunnels in Ranier Mesa and some of the other underground constructions been a potential analog source for you dealing with corrosion?

STAHL: There hasn't been a good linkage there, but I know that we've had some examination of some materials that have been exposed at various locations around the site. Some of it is useful, some of it is not because the conditions were not well documented.

NELSON: Do you suspect that that is a source of information that you could make use of? Does it fit into your idea of an analog that could be useful?

STAHL: It might be, and it's worthy of evaluation.

NELSON: Okay, thanks.

BULLEN: Any other Board questions? Any other staff questions?

(No response.)

BULLEN: Thank you very much, Dr. Stahl.

I want to express my appreciation to the audience for persevering. We're going to be about 15 minutes late. Our next presentation is going to be by Dr. Della Roy, to whom I also have to apologize for the spelling of cementitious, I think, as opposed to cementatious. I'm not exactly sure what cementatious materials might be, but I'm sure she'll tell us what cementitious materials are, and I'm sure we'll be very illuminated by this talk.

ROY: I think there is a natural cementation that is probably where that term came from, and it prevails sometimes. It's an alternate term. But I will talk about cementitious materials at this moment, and I'd also say that the Yucca Mountain project is not necessarily responsible for my thoughts on this, but I'd give that disclaimer. Nevertheless, I think it's pretty much in tune with what's been going on to date.

The needed knowledge for cementitious materials in tuff repository environment we saw as a performance in a thermal environment, which is a little bit beyond what concrete has ordinarily been called upon to perform. There have been high temperature performance, but not for as long a period of time. So that is the challenge, one of the major challenges to be able to predict that for the extended period. So in both the shorter term and then through the post-closure period, that involves interactions with the host rock, interactions with the waste package, as you've heard something about, some of the factors concern pH control, what effect concrete carbonation would have on its longer term performance, other durability issues, and then the ability to tailor cementitious materials so that they might give better assurance of their long-term performance, or at least be able to understand what their long-term performance is.

And in here, one of the most important factors is the matrix. As you know, concrete is 70, 80 per cent coarse aggregate or so, and a rather small amount of the glue that holds it together, and yet is very important in its performance and is the component that one speaks about throughout most of the period of its performance.

And then saying this matrix, what we're concerned with, the conventional Portland cement is calcium, aluminum, silica compounds, which hydrate and form mostly an amorphous product which is the glue holding it together. And so we look at Portland cement having a composition very high in calcium. Other components that are often used are plotted here, or silica can be added to modify the composition, so you're not necessarily limited to a strict Portland cement, but one that is modified in order to get its desired performance.

Now, this is from some work of Professor Glasser concerned with radioactive waste management in Europe, but I thought it was useful to quote some of his. The bottom of the list of the phases that are formed in concrete from the matrix, it's listed at the bottom, very commonly the calcium, silicate, hydrate glue, abbreviated CSH. But there are a lot of crystalline phases that you get in the x-ray defraction pattern, and sometimes people tend to neglect this, but that's very important. And getting back to our calcium, alumina diagram, then there are a lot of these formed, but the most important then is calcium, silicate, hydrates formed along there. And then equilibrium with materials such as clays and zeolites if you modify the cements, they can almost become part of your matrix material.

Now, it's possible to control chemically the concrete matrix, and this illustration shows an ordinary Portland cement here, mixed with a fly ash there, and you form the products, the calcium, silicate, hydrate glue and beyond that, calcium hydroxide, which is the second major component, decreases in proportion till it can get to essentially zero, and if you're worried about high pH, different ways of controlling that amount will affect the pH and reduce it significantly.

The expression of pH in the cement matrix is a function of calcium to silica ratio, which is plotted here. Up to a certain proportion, you have this high pH of 12.3 or so, or even a little higher, with alkalis present. As you reduce the calcium silica ratio, lowered and get down to 11 or lower, and at higher temperatures, this becomes even lower.

So one is not necessarily limited then by this initial high pH, which is thought of to be one of the major concerns of Portland cement, and this just shows that different reactions at 25 normal hydration conditions, the higher temperature will bring the pH down even farther, so if you control the chemistry and the temperature, then you are affecting the pH. And one of the ways in which one can do this is by adding other components. You can add either just plain silica. I think this is also someone else's data. It's not my own. It doesn't get down to zero. You'd have no calcium. But adding materials such as blast furnace slag or fly ash or indeed silica, pure silica, will decrease the effect of this high calcium ratio.

A second major consideration is the microstructure and essentially the permeability of the cementitious matrix, and illustrated here very schematically, just cement, hydrates, normal flocculation would be a random aggregate. You can affect this by packing with a disbursant and you can affect it by in-filling with a lot of fine matrix particles, which then influence the final hydration, generate a finer microstructure and a less permeable material and lower porosity, lower permeability material.

Not only the total porosity, but the pore structure is important, and a mean pore radius expressed here in terms of nanometers is influenced by the proportion of water that you put in the original concrete. Again, add mixtures such as a disbursant, water reducers, super-plasticizer reduce the amount of water. There's no substitute really for reducing this initial water content.

In addition to the chemistry, the microstructure and showing the permeability varies by orders of magnitude as you change this initial water to cementitious ratio, and the lower you can get there, the less porous, less total porosity, and then depending upon the components, the permeability itself.

I showed this as an example of permeability related to diffusion, in this case, of chloride. Chlorides are common. The reason for picking this was because this particular cement contained blast furnace slag, which also is a means of substitution partly for Portland cement, maintains a reducing atmosphere in the cement for a long period of time, and may be important for issues of corrosion.

The permeability, I won't dwell on that. There's another mathematical relationship between porosity and permeability, but then to go on to some specific effects of temperature. It is fairly well known that in experiments that have been performed in periods up to months duration and so on, that compressive strength will be increased by heating up to a certain stage in unsealed systems, that is, where some water can essentially evaporate, so up to 200 or 250 or so, and even up to nearly 300, you may be above your original compressive strength, the tensile strength has not been considered to be as good, but there are also some data that show, not necessarily need to degrade.

These were studies for another purpose of studying the effect of carbonation, and I've jumped into this rather abruptly, combining the effect of heating and carbonation, but just to show that in exposure conditions, it's not necessarily bad to carbonate. It can increase the strength, in a sense, showing experiments using natural level of CO_2 in the carbonation studies, or accelerated studies with 5 to 100 per cent CO_2. And modified concrete compositions can increase the flexural strength, one of the bugaboos that heating is always harmful.

The additional effects to be considered are, in considering the effect of heating, are what really happens to the matrix. This doesn't show it very well. This is getting up to 700 or 800 degrees here. But you lose successively low temperature water, hydroxyl from calcium hydroxide, and then carbonate CO_2 from calcium carbonate. So these are the sort of reactions that need to be considered as one is investigating the performance under higher temperature conditions. And, indeed, here in this region, carbon dioxide that is fixed chemically in the concrete above about 500 begins to be decomposed. So it does hold the carbonate in its structure for a long period of time.

I'm not obviously, since--I'm not going to be able to cover some of these things in the package that I've handed out, but I just wanted to give a couple of examples of materials that were designed for high temperature performance with high temperature performance in mind, or with reducing the pH of the concrete. And without looking at everything, these are mortars with sand and various cementitious components, and water, adding components such as silica fume to increase the silica content, fly ash, and in the case down here, it mentioned blast furnace slag, silica, silica fume. So tailored components to make the performance better at higher temperatures, and that extended onto concrete could be more in tune with repository performance, if indeed you would use this as the coarse aggregate, tuff from the repository environment, a well consolidated, strong tuff material, but indeed build in a good bit of chemical barrier.

Compositions of some of the matrix materials we're talking about in the bottom here, rather than the 65 per cent or so calcium oxide you have in Portland cement, you can reduce it to as much as 40 or 50 per cent, and also decrease the calcium, increase the silica to be more compatible with the repository environment.

Lastly, some of these compositions increase in strength of these, or compressive strength, as you go to higher and higher temperatures, up to here at least, 175 degrees, somewhat less at 250 degrees, but it is possible to generate some of these materials that would perform well at low temperatures, and then as you heat to successively higher temperatures.

I had intended to say more about carbonation. I think maybe I will restrict that and let you look at the notes. I'd say that thermodynamic calculations have shown that unquestionably, if you have thorough access to CO_2 for a long period of time, the stable phases are indeed calcite and some form of silica. So it's a matter of factors such as, well, the total composition and also the permeability, the rate at which CO_2 can permeate through the matrix of the material.

Just to show some effect of changing the composition and the composition of the concrete, these give examples of 400 kilograms per cubic meter cement, and 100 here. You have two little cements, the depths of carbonation in a particular time is much greater. You can reduce it by 25 times by having a better impermeable cement matrix rather than relying entirely on aggregate, and so forth.

So I'll try to bring this to a quick conclusion, saying that there is, whereas perhaps one could call it only a modest data base for concrete mechanical properties at elevated temperatures, to be sufficient that that be encouraging for this sort of long-term performance, and the potential durability would depend upon the combination of both the physical and mechanical properties under the sustained elevated temperature.

The chemical compatibility of the cementitious matrix with the host rock, stability, interaction with the water chemistry, and hopefully to consider the need to be benign at least effect of cementitious material on the waste package, and adequate bonding of the matrix and the aggregate to perform and to maintain this composite material performance for long periods of time. It seems feasible to design specially tailored cementitious matrix materials, concrete, that resemble normal concrete and yet maybe tailored sufficiently to generate long-term performance beyond what would normally expect from what you put in your sidewalks, for example.

BULLEN: Thank you. Questions from the Board? Sag$_{\eta\vartheta}$s?

SAG$_{\gamma}$IS: Sag$_{\eta\vartheta}$s from the Board. The use of the tuff rocks in aggregate for that--use in a material that's normally not used as a concrete aggregate and sort of like introducing an unnecessary unknown, while not using just regular aggregates normally used in the construction for which there's plenty of experience?

ROY: Okay, that is certainly a variable. It has been used. It is not unknown. It has not been very common because there's not necessarily a large source of tuff aggregate. Certainly this site would be, if anywhere, would be in this case. One of the options that was considered was a granite, which is much more abundant, and the total chemistry is pretty much the same, but the tuff would be of course very much closer total characteristic.

BULLEN: Cohon?

COHON: Cohon, Board. Could you elaborate on why the physical properties of concrete cementitious materials seem so dependent on temperature? What are the mechanisms, in particular, compressive strength and permeability?

ROY: Well, for one thing, the major binding phase, calcium, silicate, hydrate, contain water in its formula, and that is gradually lost with heating at the elevated temperatures. If it is done slowly enough, as you can see from the one graph, up to at least 250 degrees, if it's done rather slowly, it can be advantageous. There's enough bonding that takes place despite the loss of some of the surface water that is fairly loosely bonded. So that is the main consideration.

The second consideration is the thermal

differential, thermal expansion of components that one may expand in a different rate from others. It's possible to tailor that by the aggregate versus the cement matrix to make them expand at more or less the equivalent rate so you don't have great discontinuities there.

BULLEN: Craig?

CRAIG: Craig, Board. I'd like to understand how long you must test a material, a new material, before you have some confidence that you can understand how it's going to behave over its entire life? You showed a number of curves here where there was testing over periods of months. But for our purposes, we're concerned about understanding how long a material will last before it totally falls apart. Can you give us a feeling for the time frame which is required before you have some confidence about the long-term deterioration characteristics?

ROY: Well, I think obviously the longer the better, and probably the longest some of these experiments have been have been two years or more, something like that. Of course, and I say that the Roman concretes did in fact use tuff as an aggregate, and they used cement that is somewhat like the Portland cement, and have indeed performed for very long time. But obviously you need to develop models for extrapolation of the data beyond what is feasible in terms of--

BULLEN: Any other questions from the Board? Questions from the Staff? Sagηϑs?

SAGγIS: One additional question. How many analogs of experience of concrete are used for long times at high temperature like, for example, liners for furnaces and things like that; do we have any analog like that?

ROY: Most of the concretes that have been used in such applications have been calcium luminate, refractory concrete, where they go through a slight degradation with heating, and then develop more--essentially a refractory bond at a somewhat higher temperature. I think these are not particularly suggested for this application because they usually undergo a strength loss before a strength gain. But some of these, pre-stressed concrete reactor vessels, to some extent there's experience there. Beyond that, probably no really long-term applications.

BULLEN: Thank you, Dr. Roy. And I'd like to thank all the speakers in this morning's session, and I'll turn the imaginary gavel back over to our Chair, Dr. Cohon.

COHON: Thank you, Dan Bullen, for your excellent job of chairing that session.

The restaurant has set up a buffet to accommodate the large number of people who may want to eat there. That's available for you now if you care to take advantage of it.

We will reconvene at 1:30. Thank you.

(Whereupon, the lunch break was taken.)

AFTERNOON SESSION

COHON: In this afternoon's session, we will be continuing on the overall theme of TSPA, but now looking at the broader far-field environment.

For this afternoon's session, Debra Knopman will serve as Chair. Debra is also Chair of the Board's panel on site characterization. Debra?

KNOPMAN: Good afternoon. We have a full schedule planned here, so I, too, will dispense with remarks. But just very briefly, this afternoon's session has been designed to open a window on what I think is a particularly critical element of performance assessment, and that's the modeling of the unsaturated zone and the use of experts to identify the appropriate bounds on some of the physical parameters that go into the modeling process.

I'd like personally to commend the Department of Energy for proceeding with these expert elicitation panels, taking them seriously and really giving many people outside of the program a chance to look closely at the assumptions and the thinking behind what the Department is doing.

Our program will begin with an overview of performance assessment with regard to the natural barriers. Abe Van Luik from the Department of Energy will be giving this overview for us. Abe is currently the team leader for the Assistant Manager for Suitability and Licensing's Technical Synthesis Team.

VAN LUIK: Thank you for that introduction.

I believe that I'm not unlike a steam locomotive. It's what comes behind the locomotive that interests you, and the reason that I thought of that imagery is because as a small child, we went to the train station in my home town in Holland to pick up a relative, and my brother and I had the brilliant idea of standing on the little overpass over the tracks, you know, where you can go from one track to the other, and when I got home, I got chewed out royally because the steam locomotive coming underneath me, which was very exciting, we could feel the heat, completely blackened my white shirt. So I know now that there are undesirable things about the locomotive. So it's what comes after this talk that really interests you, but my job is to put it in perspective.

So what I quickly want to go over, and you heard Bob's talk this morning, mine is a parallel talk, so it skips a lot of the detail in Bob's talk and goes right into the role of the Geosphere and TSPA, the components of it that we're interested in, the role of the Geosphere and the waste containment and isolation strategy or the Safety Case, as we probably will relabel it, and I have a bunch of viewgraphs on key information required from the Geosphere models, key issues associated with the Geosphere models, and the approach to address these key Geosphere model issues in TSPA/VA. I may skip over that so that we can get to the meat of what you're actually interested in, which is the talk about these actual models and see what's in them.

The schematic of the natural system, you can see that the natural system, the way that we perceive it has a percolation flux, influenced periodically by climate change. We have, for convenience, divided the repository into six columns, each one of which has characteristics in terms of the units of the mountain that are involved. The EBS, which you heard about this morning, which is why this is kind of grayed out, Bob has covered this. When he delivers the nuclides, slowly we hope, into the unsaturated zone, then we worry about the flow and the transport processes in the unsaturated zone, and then through the saturated zone to eventually a water well and a human consumer.

Bob showed you the rich man's version of this chart this morning, and this is the poor person's version. Bob has basically covered the EBS part of this this morning, so what my job is is to put things into perspective, that unsaturated flow and the interaction of that flow with the engineered system, and then unsaturated zone transport, saturated zone transport, and biosphere. These are the processes that I am supposed to put into perspective, and I think this chart does a very nice job. It really emphasize the EBS, and that's because Bob made it for his talk this morning. But I think it's an important point, that if it weren't for the EBS, we wouldn't even be discussing this mountain or the physics of unsaturated flow or anything else. The thing is that we are building a system by putting an engineered system into a natural setting.

The interface between the natural setting and the engineered system is important, and so in my viewgraphs, which I will probably not go into, the thermochemical environment and the thermohydrologic model that are here, part of the natural system, actually reflect the interface and the interaction between the engineered system and the natural system. And, of course, seepage would not be an issue unless you build a drift.

A similar chart to what Bob showed, but this is for the natural system. And the components that are of importance are infiltration, percolation flux, seepage into drifts, changes in aqueous flow from the thermal effects or from climate are somewhat less important in our sensitivity analyses today, unsaturated zone radionuclide transport, saturated zone radionuclide transport, biosphere, and then the disruptive processes, volcanism, seismicity, and probably for VA, we will add another one, which is potential criticalities.

If you notice, all of these things track with the key technical issues identified by the NRC, which they are using to review our work, and that all of them are covered in the waste containment and isolation strategy, except the biosphere. Why is the biosphere not checked here and here? Because we do not see the biosphere as a barrier. The biosphere is not a barrier. It is a delivery system basically for what comes out of the geosphere.

Jean already covered this, the natural barrier system provides a controlled environment within which the behavior of the engineered components can be evaluated.

Here's a word I like; it provides remoteness from variability in surficial processes, it provides remoteness from the biosphere, and it provides reduction and delay in arrival of any released radionuclides from the engineered components. And the key words, dispersion, dilution, retardation, are part of the transport modeling.

What I propose to do is, you know, we had these workshops that some of your staff attended, in which we talked about the uncertainties and the key information needs from the unsaturated zone, thermohydrology, saturated zone and both the flow and the fracture aspects of it, and we have had considerable work done in terms of looking at the seismic probability and the volcanic probability.

What we have done in these workshops is to identify key information for each of these models, identify key issues that need to be nailed down a little bit further for TSPA/VA, and then created a work scope to address each one of those issues. For example, the meeting today is basically on unsaturated zone flow. Key information needs, percolation flux, and its spatial and temporal variability. Fracture matrix flux distribution, seepage flux spatial and temporal variability, so let's just focus on unsaturated zone flow and you can just believe me that we've covered all these others, because soon we will be talking about the actual models.

So then if we go to the issues identified as important for the unsaturated zone flow model, infiltration rates, how are we going to address that for TSPA/VA? Well, we will use alternate maps, alternative maps, including uncertainty, and we will use the results of our UZ flow expert elicitation, of which you're going to hear quite a bit today.

The variability in the infiltration rate, we will do a sensitivity study to propagate surface variability to variability of depth. Climate change, we will derive climate change effects from multiple calibrated UZ flow models with alternate climate and infiltration scenarios. These are the things that we not only described in words, but described in terms of what we're actually going to do between now and VA to give us a handle on these issues. And in the write-up for TSPA/VA, you can look at each of these issues and see what the outcome was of the work that we did.

Seepage flux, a very important parameter, because if there's no seepage, if everything drains around the drifts, then we probably have very little chance of moving radionuclides into the lower part of the mountain and then into the biosphere. We will derive seepage flux from drift-scale models. We will look at a reasonable range of conceptual model uncertainty and parameter uncertainty, and of course the expert elicitation will help in this also.

The variability of the seepage flux, we'll derive it from the drift-scale model results, combined with the expert elicitation.

That's all for the unsaturated zone flow model. There's also transport model issues, saturated zone flow and transport model issues. As I said, we have looked at disruptive features, events and processes, and basically the volcanic event frequencies are going to be looked at from the results of an expert elicitation. We will scale the frequency for indirect effects. The work that we did in '91 and '93 show that the direct effects are much more important in terms of consequences than indirect effects.

Looking at the consequences of a direct volcanic eruption, we will review the work that's been done by the Center for Nuclear Waste Regulatory Analysis in San Antonio for the NRC. We will borrow their model, incorporate reasonable ranges of effects based on expert judgment into those models, and use them to evaluate the consequences.

And then the indirect volcanic events, like I said, we will develop bounding effects for those based on expert judgment again, and we will do some sensitivity analysis on the range of consequences.

The reason I bring this up is because sometimes in our enthusiasm for the modeling of the natural system or the engineered system, we do sometimes forget to bring in the fact that we are also looking at perturbations of what we know by these types of events.

Human intrusion, we will look at stylized human intrusion analyses as recommended by the National Academy of Sciences in their report on the Yucca Mountain standards, which the EPA is using.

Seismic/tectonic events, again, we're looking at an expert elicitation to give us the inputs on that.

I think the important thing to take away from my talk and now launch into the more substantive talks that come after, is that we know that significant issues exist regarding confidence in the models and, therefore, there are significant issues of confidence in the predictions based on those models. We know that.

We have been implementing approaches that we defined in these workshops to address these issues within the viability assessment. But another important thing is that additional testing and model development and substantiation will occur between VA and LA. In fact, one of the reasons that we have a peer review group working with us through the VA is so that their final recommendations can be folded into the TSPA/LA, because this is a snapshot in time, but this is where the rubber really meets the road.

Thank you.

KNOPMAN: Thank you, Abe. We'll take some questions now from the Board and Staff. Norm Christensen? Oh, you didn't have a question? Dick Parizek?

PARIZEK: On Page 3, the stick man is not smiling in this version. Is there some purpose for that? I just wanted to indicate that he was in the previous version.

VAN LUIK: As we approach the viability assessment and the license application, none of us are smiling.

KNOPMAN: Dan Bullen?

BULLEN: Abe, you mentioned the additional testing, model development and substantiation. In terms of the nuclear community, that's validation and verification that your code actually works and that you can do what you say you're going to do. I guess the question that I have with respect to the geosphere model unsaturated zone transport is that can you use it as a predictive tool to say predict where the chlorine-36 might show up, go in there with an exploratory enhanced characterization of repository block effort, and then find it and then use that as a verification tool or a V and V tool? I mean, it's a tough call, but is it something that you're considering?

VAN LUIK: My answer to that particular example is no. My answer to the general statement is that we will be doing, and Bo has already done this to some extent, forward projections of what we should see when we excavate, and in general terms, then we can verify or change the model as necessary. But as far as predicting where one-tenth of one per cent of the flow would go precisely so we can go look for it, you know, I mean if we could do that, we would all be doing hand stands with one hand. So we'd all be smiling again.

KNOPMAN: Abe, I have just a quick question, and it's a followup to Dan's, that is, the planning is going on now for the East/West crossing. The Board, so far, is not tuned in too well on what scientific studies precisely are going to be going on as soon as excavation begins. Can you say a few words about how the PA side of the house is informing the structuring of the experiments planned for the East/West crossing?

VAN LUIK: Mike Voegele will actually address this in some detail in his talk. I believe that's tomorrow. Is that correct? However, I can say that from my own experience, I participated in the planning meetings, and through me, the PA team had input into the planning into what we thought would be desirable, since the opportunity is going to come up to do certain things. And some of the things that we suggested were, you know, were put on the high priority list. Some of the things we suggested were not. But nevertheless, we feel that from these activities, we will have a much more definitive story to tell by the time of license application especially.

So I would like to defer the actual meat and potatoes of that question to Mike Voegele.

KNOPMAN: Okay. Any further questions for Abe?

(No response.)

KNOPMAN: Okay, thank you very much.

We're now going to move into a description of the process and the objectives of the unsaturated zone expert elicitation project, and I think this is kind of an interesting program structure we have here. I hope to encourage as much interaction among our panel, not just between the Board and panel, so I encourage you at a later point in the program to please feel free to jump in. This is meant to stimulate an interesting discussion, and we should take advantage of you all being here and being engaged in this to get as much out of it.

Kevin Coppersmith is with Geomatrix and Kevin is going to describe just the overall process that was used to hone in on some of these uncertainties.

COPPERSMITH: Thank you. Thank you very much. I will be talking about the objectives and the structure of the expert elicitation project itself.

I have to mention as part of these expert elicitations, part of what I have to do a lot is facilitate meetings, and a big part of that is keeping things on schedule. And it was wonderful to have Dan experience the anxiety of trying to keep people in line, on time, and to sit there and to just enjoy it and let someone else do it.

BULLEN: Thank you, Kevin.

COPPERSMITH: I'm sure I'll get my chance soon enough.

Following me, Bo Bodvarsson will talk about some of the results to come out of the elicitations, and we'll have two members, two of seven members of the expert panel who will be talking about the process, and then Bob and Bo will talk about where these results will be used. So mine is going to be a talk that's devoted to process.

The objective of the study shown here, to identify and assess uncertainties associated with certain key components of the unsaturated zone flow model. As you'll see, the overall theme in my discussion is one of assessing uncertainties.

We have ongoing programs, models, unsaturated zone hydrologic models of the program as carrying out. We have data collection programs that provide information that go into those models. The focus of this study so that it integrates with the rest of the activities is in helping to quantify uncertainty, to the extent possible. So we're focusing in, we bring in experts from within the program and outside of the program to help with that quantification of uncertainty.

So during the course of this, the assessment looked at the data inputs, looked at the modeling approaches that had been done, looked at the results that are coming out of the modeling efforts as part of the general background in helping to deal with the whole uncertainty issue.

Of course the TSPA in general is a probabilistic analysis. It can readily accommodate uncertainties, in fact should have uncertainties quantified as part of the basic assessment.

We focused quite a bit on percolation flux, which is the volumetric flow for a cross-sectional area at the level of the repository, and the various approaches that could allow you to make an assessment of that.

The two users of this study are those who are involved in the site scale modeling, Bo and those at LBNL, because they can get insights into the types of modeling approaches that may be appropriate, some of the uncertainties, and the TSPA, who like I mentioned before can handle and readily accommodate quantitative expressions of uncertainty, like probability and density functions, or alternative weights applied to different models.

In terms of the treatment of uncertainty, we are, as I mentioned, it's a major goal of these types of expert elicitations, including both modeling and parameter uncertainties. This issue came up earlier of whether or not you're in fact treating alternative models, and the example given was that it may be an equivalent continuum type model versus a dual permeability model or other conceptual models of fracture matrix interaction. We are looking for an expression of the uncertainties in alternative models, as well as just parameter values.

To ensure range of perspectives, we're using multiple expert judgments, and that was elicited from a panel of seven experts, and I will go through their names in a minute. But the goal here is to not only quantify the uncertainties across a panel of experts, but to get a clear description of uncertainty within each expert as well. So for those who do this game, we're looking for both expert to expert and the across expert uncertainties and their components.

We had panel members that came from both within and outside the Yucca Mountain project to represent a range of experience, different areas of expertise in modeling rock properties and so on across the expert panel.

I won't get into all of the details, but I will talk some about the process that we followed. It's a deliberate process that follows much of the recent guidance related to expert elicitation studies. Went through the process of facilitating interactions through a series of workshops. There was training of the experts in terms of how uncertainties could be quantified and how their assessments or interpretations would be elicited.

It is consistent within recent NRC guidance, the branch technical position on the use of expert elicitation, as well as recent DOE guidance, and I think there's enough individuals here, Bob Budnitz and others, who were involved in that study, I would say that this is consistent with that guidance as well.

Basically, the types of guidance that exists is relatively flexible at this point, but deals with the process of selecting experts, facilitating interactions, going through a process of issue identification, elicitation interviews, feedback and documentation. So we followed those basic steps.

Many of the results, the assessments, are in the form of, say, probability distributions or probabilistic expressions of uncertainty. Other assessments are verbal and are text that talk about the pros and cons of alternative models that might be appropriate for representing unsaturated flow through various parts of the hydrostratographic section.

The steps that were followed, again, these have been known in some parlances as the seven points of light, or the basic components of any expert elicitation type project that leads from the overall planning, development of a methodology team, through the selection of the expert panel, interactions, elicitations and final documentation.

I should point out that what has been found I think in recent years, say in the last ten years, on expert elicitation studies is that in fact interaction among the experts is a very useful thing, and in fact I would argue is a required element of a proper elicitation project, inasmuch as it allows for the sharing of information and views throughout the process.

Obviously, a lot of data have been gathered related to the unsaturated zone at Yucca Mountain, and much of the early part of the project is climbing a steep learning curve, listening to the principal investigators who have developed those data sets, and allowing for those from outside the project to climb that learning curve and to become proficient in the types of data that had been developed.

The questions that these panels are asking is relating to uncertainty, and these workshops, they're asking how high could it be, why couldn't it be lower than this. They're pushing ultimately for a quantitative expression of uncertainty as opposed to the best case or a single case. They're looking for the ranges.

So you'll see in the probability distribution functions that are developed, for example, in percolation flux, that they have long tails on those distributions if they feel in fact that they are uncertain parameters. That's what we want. That's what we're looking for in this evaluation.

We also, though, asked them at the end of it all, what could be done, if anything, to reduce that uncertainty. So we do provide, because this is a snapshot in time, we provide for the opportunity for that type of advice back. And, in fact, many of the items that were identified are already being incorporated into the ongoing data collection and testing program.

These are the members of the expert panel. I'll put them up. I think one real advantage, we have a couple of individuals who have been involved in the project to some extent before, Karsten Pruess, Ed Weeks, for example, and others who had very little experience at all in the Yucca Mountain project, but significant experience in this type of climatic, this type of environment in terms of the unsaturated zone processes, either in modeling efforts or rock properties or net infiltration type studies. So it, as a group, represented a broad range of expertise.

I'll quickly go through just to get the gist of some of the workshops and interactions that occurred. The early workshops deal with identifying the significant issues that have emerged through the process of past TSPAs and technical issues that are important just in general to unsaturated zone flow processes. Those are discussed and identified in the first workshop, as well as the data that had been developed.

The second workshop deals with alternatives, what are some of the uncertainties in alternative models and expressions of understanding of the unsaturated zone flow regime. This was an opportunity for a give and take not only with the panel itself, but bringing in proponents of alternative views to allow the panel an opportunity to hear the pros and cons of different interpretations.

A field trip was held to the ESF and Yucca Mountain vicinity. Obviously, this group, like any group of hydrogeologists, is one that wants to get an opportunity to see the relationships, not only within the tunnel, but we spent a lot of time in the Yucca Mountain area looking at the testing, particularly focusing in on work that had been done related to net infiltration studies in the local Yucca Mountain area.

The third workshop was an opportunity for the experts to provide their preliminary interpretations. This was their first pass at the way they're going to be dealing with the key issues that have been identified. They place them up as straw men for each other to discuss, and it tends to be a lively experience, and in this case, it was as well. It's an opportunity for what we call technical challenge and debate to occur. We always have a series of ground rules that don't allow for physical blows to occur, but we do allow for a lot of verbal interchange in that process. But these were the key issues that were discussed in that preliminary interpretations workshop, and obviously again, since they soon after that are going to be elicited for their uncertainties, that's the key part of the discussion.

The interviews themselves were held in one day sessions with each expert. These elicitation interviews were documented and became elicitation summaries that then were revised multiple times through the course of the remainder of the project and are appended to the report for the study itself. They become the basic interpretation or assessments that are made by each individual expert.

Feedback occurs following these interviews. A feedback package was provided that summarized all of the elicitations. One of the things that we found through the years is that you in fact can and should allow members of an expert panel to see the assessments made by their colleagues on that panel, as well as some sensitivity studies that had been requested were carried out at that time, and a summary across the panel of the assessments made of some of the key issues, like percolation flux, infiltration and spatial and temporal issues.

Finalization and documentation of course is critical, and those are documented. We have a final report that documents the process that was followed, the assessments summarized across the panel, as well as the results, individual assessments made by each individual expert.

I should say that our goal in this from the very beginning was to have a defensible basis for equal weighting among the experts. This is an area, the overall integration or aggregation process is one that has been debated through the years, and we've found that we can, through a process of interaction, equal data dissemination, an opportunity for feedback and a discussion among all the expert members, be in a position at the end to weight their assessments equally, as opposed to differentially among the panel.

Now, I'll end with a brief listing of the key issues that were addressed by the panel, and there are others, but these are issues that were essentially addressed by all members, and Bo will follow me with a brief description of the assessments by individual experts on the panel related to these issues.

First, it deals with the overall conceptual model or models of unsaturated zone flow system, or the important elements, how does moisture work its way through the mountain. Net infiltration, which is dealing with the surface water balance is a strong focus of the assessment, strong focus of the ongoing data collection and modeling program for Yucca Mountain. Dealing with the temporal issues there, how episodic is the process, how does it distribute it spatially and what are the averages over the Yucca Mountain block itself, what does that look like.

Lateral diversion, the top of the PTn, is an issue that has been discussed quite a bit, and it was a question that we asked all of the members of the panel.

The temporal behavior of the UZ system, we've talked about is it episodic or not. As moisture moves through potentially as pulses and severe storm events, does it become dampened as it moves down through the system, or is there a transient or temporal component that needs to be considered.

A number of alternative methods have been proposed for estimating percolation flux at the repository horizon. The pros and cons of those methods were discussed by each of the experts as well.

And then percolation flux itself, we assessed a spatial and temporal average over the Yucca Mountain block, and then dealt with the variability spatially over the block area as a separate assessment.

Asked them about the components of the flux, how much is in the fractures and how much is in the matrix by it's down to the TSW, and that can be an important consideration in terms of the conceptual model for flow.

Of course the fast flow component, the isotopic evidence, environmental isotope showing fast flow was discussed throughout the course of the workshops and was assessed asking all of the individuals. We asked them the question of what component of the flux volumetrically does the fast flow component represent, and I think that's an important consideration. There's need to separate flux from velocity, and I'm sure Shlomo or Gaylon will talk about that. There is a difference between the two. Velocity and flux are not necessarily the same thing.

Of course getting into the issue of seepage into the drifts, I should say here that we were dealing with ambient conditions. We're not dealing with the thermally elevated conditions. It's under present conditions, assuming just simply an opening at this point, a drift.

And we asked them questions on modeling issues and additional data collection that could be developed to help reduce uncertainties. This was a question that they, in light of the issues that they had addressed, what could be done in the reasonable short-term and the long-term to deal with and hopefully reduce some of those uncertainties.

So that's my discussion of the process.

KNOPMAN: Thank you very much. We'll take some questions. I just want to remind everyone that results are coming in the next presentation, so questions to Kevin should be focused on what he in fact covered.

CRAIG: Paul Craig from the Board. I have a methodological question or two, and it has to do with regression toward the mean, or the tendency of people to want to hang together into a group. Groups normally tend to like to do that and many committees don't like to have minority reports, this one included, and when one does occur, it can lead to a lot of discussion, as for example in the Timms Report and Tom Pigsford's refusal to regress to the mean.

So what I'm concerned about is when you run an expert elicitation kind of a process and you have people talking to each other, isn't there a tendency to have results which are going to squeeze matters down, squeeze the conclusions down into a narrower zone than (a) you would have if you didn't squeeze people down, but more importantly than that, maybe a zone which is so narrow that it doesn't represent the reasonable range of what people might be concerned about. And I'm thinking here particularly about stability of results, and what I perceive to be the reality that if you had run an activity like this a couple of years ago, the percolation flux that you would have gotten out of your expert panel would have been really different from what you got out of this one. And so I say well, gosh, if you do it again in three years, maybe it's going to go up again, or something else will happen. Can you reassure me on some of these points?

COPPERSMITH: I'm not sure I can reassure you on all those points. I can say to start with that that is an issue. That's one of the downsides of interaction. If you go back ten years or more to some of the expert studies that were done, there was minimal--and many of these related to nuclear reactors in the Eastern U.S. and earthquake problems and so on. Some studies were done with minimal interaction among the experts, with the thought being that you'll have a better opportunity to get an independent assessment, and you'll potentially have a broader range because they haven't achieved some sort of forced consensus or just, like you said, a natural desire to want to have a consensus. And I think that potentially is true.

The other side is to have a process that's highly interactive and recognizes the fact that in fact in these types of scientific evaluation, there's no true independence. We often rely on the same data sets, the same papers and so on. And, in fact, unless you have a nice interaction, someone's going to be using some data set that the others never hear about, or a paper that someone else hasn't heard about, and in fact that's even more of a problem.

So I think it's gone the other way. It's gone towards more interaction, but with a focus on once you get together, number one, we're not looking for any type of consensus. There is no need to and no desire to arrive at any preferred value or even a range that you all can agree with, you know, the type of process that has been described where you go in, put them in a room, close the door and make sure they come out with a PDF. We never forced that to happen.

So we are looking for, and we've tried to focus on, and a lot of the training was dealing with the tails we wanted, we want to force you out, don't start your elicitation with a central estimate, preferred estimates, start at the tails and try to broaden the distribution. So we're aware of those issues potentially.

Then dealing with the time, if we had done it three years before, we'd do it three years from now, you would hope for stability as well, but the only way that will be represented at this point in time is with a broad distribution.

So the point we can make, and hopefully we made with these experts, is we want you, if you're in doubt, express it. Often when we don't know much about things, well, here's a single value, and that's exactly the opposite of what we're looking for. If it's uncertain, it should be a broad value.

CRAIG: Let me, if I may, Ed, one more question, which is actually to the panel. I hope that each of you as you're going through your presentations will at some point address the question of how stable you think the present values are, and what is the probability, you can use the language that you use in your expert elicitation, what is the--it was Genghis Khan, if I remember, that's a word I think from the expert elicitation. Is that right?

COPPERSMITH: Yeah.

CRAIG: What is the probability that there will be major surprises that will cause you to--what do you think is the probability that you will revise your estimate significantly over the next ten years?

COPPERSMITH:P That has to be asked carefully. The question that you're asking is what's the likelihood that perhaps the mean will change or the expected value?

CRAIG: No.

COPPERSMITH: Or what's the likelihood that your new range will fall within the range that you've already developed?

CRAIG: I ask it as an individual question rather than as a collective question.

COPPERSMITH: Okay.

CRAIG: And I ask it in the context of personal surprise. What is the probability that you will be personally surprised by new information in the next ten years? Is that sharply focused enough?

COPPERSMITH: I see Shlomo shaking his head yes, and Gaylon is writing it down.

KNOPMAN: Well, we'll find out later on. Jerry?

COHON: You've done these before for other issues in this project?

COPPERSMITH: Yes.

COHON: And I'd be interested to hear your comparison of this process to those other ones. I recall in particular a presentation that you and other members of a panel made on volcanos, I think.

COPPERSMITH: right.

COHON: Which is very impressive for its thoroughness, especially in the selection of the experts and in the process that followed. Could you compare this and that?

COPPERSMITH: Well, I think the probabilistic volcanic hazard analysis was very similar overall. We had more time and more experts on that study. But, in general, the process followed was very similar. We went through--I didn't go into the details here--but we went through the same type of nomination process here to develop a larger pool of candidates. We had over 70 candidates that were nominated by writing letters and soliciting the nominations from 15 individuals. We put those together. We went through the selection criteria process and so on. The details are very similar there.

I think on the volcanic hazard analysis, I would say overall, it was a much more contentious group. These were issues that had been heartily fought, literally fought in the field. We had issues that, personnel issues. We couldn't have people speak the same day because they simply didn't want to be in the same room.

This group, I hate to say it's a collegial group, because Shlomo will get mad at me for doing that, but in general, this was a collegial group in comparison to that. Now, this is an issue that is not as contentious, but potentially more important in terms of the TSPA. This was, if volcanic was a Cadillac, I'd say this was a nicely running Chevrolet, because it had to be done in a shorter period of time, and the resources were more limited. But I still think that it handled all the basic elements that we had before.

COHON: Well, on the prior study on volcanos, was the group contentious because the issue was so contentious? I seem to remember that you went out of your way to put together a group that you expected to be contentious.

COPPERSMITH: Well, actually it wasn't so much the panel that we wanted to be contentious. It was the people we brought in and put in front of the panel. We wanted to be sure we had the range of view represented so they could hear that full range of ideas. And that's where it was difficult to bring them together, and even when we were in the field, they had difficulties.

But the issues there turned out, those things that were most hard fought, were least important when it came to the actual hazard analysis.

COHON: Like most academic arguments.

COPPERSMITH: Perhaps.

COHON: Thanks.

KNOPMAN: That's what I was going to say. It sounded like a faculty meeting, some inverse proportion of importance and time spent on issues. Alberto?

SAGγIS: This is Sagηϑs, Board. In your presentation, I didn't identify any transparency where the negative aspects or the potential drawbacks of this method were indicated. I assume that it is implicitly clear that the expert elicitation method in general is not the method in which you arrive necessarily at the truth. You just arrive at an estimate of what experts in the field think about the particular matter.

COPPERSMITH: Yes.

SAG$_{\gamma}$IS: I have seen some of the other material that's going to be presented. It's beautifully organized. It's tremendously appealing. But, again, us engineers and scientists, we know very well that we are smart enough to fool ourselves very easily, and again I would like to see perhaps when these things are presented, at least a list of caveats as well. That may perhaps put things a little bit more in their context.

COPPERSMITH: Okay. My major caveat would be that. This is an expression, it's a snapshot in time and it's an expression of knowledge and uncertainty at a particular point in time. We don't ask them to provide their assessment of the truth, and you hope that through this process you've captured the truth somewhere within that distribution.

The other part of it is that often, these studies are viewed as being a replacement for data, and the first thing that the experts ask for of course is data, and if there isn't much there, they say, well, I'm going to be highly uncertain. And of course that's exactly the right reaction. These are not a replacement for data in any way.

In fact, the question you ask is what can be done to reduce the uncertainties in terms of additional data collection. They are not a substitute for data.

KNOPMAN: Dan Bullen?

BULLEN: Bullen, Board. First, Kevin, I want to commend you on the undertaking to do all these expert elicitations. I know it's no small task, and I've been at a couple of them and have been impressed with the way they've been run. I do have a question as to how the project is going to use this.

I know this is geared toward VA, and you come up with a distribution in the mean and a number that's going to be used in the TSPA/VA calculation, and it's highly documented. I guess the question I have for you is how does that number change as more data become available? Do you have to do an entire other expert elicitation because we're trying to predict long-term performance, or can you say, well, I've got a hard number now because I can go in and watch the drips from the ceiling and I know exactly how much water is coming down through the fractures? Where is the process going to end? I mean, is this going to continue forever that we'll need expert elicitations, or will we actually find some case where the data will be insurmountable and we can all agree that, yeah, there's a lot of water or no, there's not a lot of water? What's your projection on will we have to do another one for LA?

COPPERSMITH: I'm not sure. I think one of the advantages of doing this and developing, say, some of the distributions that Bob will show is that you then have something that you can test in terms of the finding, new findings, to see whether or not it's changed significantly. Perhaps the mean is now changed in that distribution and you can do a sensitivity analysis to see whether or not that change in the mean affects the results.

You have a base case, an expression of uncertainty that might narrow with time if you're lucky. History has shown that it doesn't necessarily narrow symmetrically at all around our best estimate, as you know, but it gives you something to then test and see whether or not it needs to be changed.

The other part is that out of all the things that we assessed from this expert panel, and some are qualitative, some are quantitative, and so on, it may be that two or three of those drive the real performance of the system, and allows you to focus just on those, uncertainty reduction in just those, and maybe ultimately a re-assessment of the uncertainty in just those. So it allows you to focus in. I don't think you need to reconvene each time.

KNOPMAN: Any more questions? Staff? Leon?

REITER: Leon Reiter, Staff. I want to follow up on Jerry's question about differences between past elicitation and this new series of elicitations. One of the differences that I notice and other people noticed was in the past, after you had an elicitation, you had a feedback workshop, and here you were forced, I guess because of budget and time, to do that via E-mail or via mail and not have a workshop.

COPPERSMITH: Right.

REITER: Given the importance that you talked about in terms of expert interaction, how serious of a lacking is that?

COPPERSMITH: Well, history, again there isn't a long history, but there's some history of these studies that the feedback process doesn't lead to very large changes. If you've had an opportunity prior to the assessment for interaction and for people to put out where they're going at this preliminary interpretation workshop, then largely people have a feel for what those assessments are going to look like. They have a chance to challenge them at that time.

The advantage of a feedback workshop is you come in, I did my elicitation, I did my assessment, and now I want to lay it out and let everyone look at it and see what they think. And you can do that in an environment, an actual workshop environment, and that can be very effective.

I think here, we put together a feedback package that went out. They had an opportunity to see what everyone else said and to make adjustments. I think another interaction would have been useful. My personal feeling is I don't think it would have led to very significant changes. It might bring closure in terms of discussion of key issues, but typically it doesn't lead to very significant changes.

KNOPMAN: Okay, thank you very much, Kevin.

We're going to move on now to actually hear the results of this process. Bo Bodvarsson is a senior staff scientist and the head of the Nuclear Waste Department at Lawrence Berkeley National Lab. He specializes in research on geothermal reservoir engineering and nuclear waste disposal. Bo?

BODVARSSON: Good afternoon. It's a pleasure to be able to comment and talk about the results of the expert elicitation.

To give you just some personal opinions in the beginning, when this was first proposed to me a while back, I was certainly not for it. I mean, who would want six or seven experts coming and questioning your model and approaches and all of that, and I learned since then that this has been quite useful, to a very pleasant surprise.

First of all, the nicest part is that they didn't find any huge holes in the model, I don't think. The second nice thing is they had a lot of suggestions about how to improve the model that we have taken into account in later, you know, iterations of the model. And, finally, they pointed out a lot of data deficiencies, which I totally agree with. So I think in all accounts, it will help the UZ model and PA make the UZ flow better quantified.

I want to start with a few viewgraphs that you don't have in your handout just to tell you a little bit about what the UZ model is, because I know not all of you are hydrologists, unfortunately, some other disciplines must be represented also, and so I wanted to tell you a little bit about the model and what it entails.

This is a description of the model. The UZ model is here, a model that Abe talked about and other people have talked about that is a very important process model that feeds the ambient drift scale models, the thermohydrology drift scale models, the mountain scale models of thermohydrology, and then the fuel transport, so it feeds a lot of different models in the PA arena. And the purpose of it basically to integrate all of the available data from the unsaturated zone into a single model, and when I mean all, I really mean all. I mean there is a lot of data available, but they all must be considered to calibrate the model the best it can be.

We then go through the calibration efforts. Again, all of these data sets, including hydrological, geochemical, pneumatic and thermal data sets, and then the primary purpose is to determine percolation flux, fracture matrix, components of flow, flow below the repository, provide this information to PA, but also very importantly, give us confidence that we have a tool that integrates all of this information together.

The components of the model, and I'll stand here on the side so you can see it, are just summarized here. This is the model block. It takes some input, infiltration properties, whatever, a lot of different input that come in here collected by USGS and LANL and other people, and then this box here is where we do the gridding, the numerical approach and input all of these properties. But then most importantly, calibrate against perched water conditions and extent and ages, the pneumatic calibrations, the verification of calibration, predictions, as was pointed out before, calibration against temperatures, isotopes, whatever we can, and then the output, the percolation flux, flow into drifts, fracture matrix components, flow patterns below the repository.

This is all done within the context of mathematical and numerical formulations that allow you some flexibility to pick different conceptual models and different mathematics, so to speak.

It also should be remembered that this is a snapshot, as was pointed out very clearly before. The development of this model started quite a while back, and we had our first real big iteration with a report by Wittwer, et al.--this is not really well focused, is it? Is that a little better--by Wittwer, et al. in 1995, where we took into account a lot of the bore hole data that was available then, but we didn't have any ESF. Last year's report, which is exactly the one that Gaylon and Shlomo and all of the other experts looked at, is last year's report, 1996, where we had a little bit more of the ESF data and more information about chloride 36, perched water and other input.

Since then, we have done another update of the model which just came out two weeks ago that considered a lot more of the geochemistry, a lot more of the temperature data, the fault properties and other data that had been collected in the ESF. And certainly the next one will take into account more of the data as we do the East/West drift and do more of the testing in the ESF. So this is basically a snapshot and we should realize that.

To answer a little bit the question about the predictions of the model, because I thought this was a really good one, unless you use a model to predict, you never know how good it is. The philosophy we have had all along is when you do a new bore hole, we predict saturations, moisture, pneumatic pressures, anything we can in the bore holes. While we were doing the ESF, we predicted conditions such as the pneumatic conditions, the effect of other bore holes, et cetera, et cetera, and DOE is now putting in place a new effort to predict the conditions we are going to encounter in the East/West drift, and that's going to include anything we can predict, including chloride 36 or whatever else, strontium, temperatures, whatever. So it's a good question.

So if the Board wants a copy of these viewgraphs, I would be glad to send a colored copy to the Board. I didn't want to reproduce them in black and white because I'm proud of these beautiful colors.

Now, I go into the real presentations that you have in front of you, and I'm just going to start with this viewgraph here that you actually don't have in front of you. I'm sorry. And I thought when I started to go through all of this, results of the experts, that I probably should point out what we are talking about in each case.

And this first one, like Kevin pointed out, deals with temporal issues of net infiltration, and what you see here are what the seven experts said on the right-hand side, if you can read it, but you don't have to read it because what is on the left-hand side is really the summary of what they concluded.

So I wanted to point out really the net infiltration is basically the bounding condition of the surface here which we are considering, and the experts said basically that the infiltration occurs primarily during severe storm events every one to 20 years, and essentially no infiltration between these events. And I think that's pretty much what Alan has concluded in his work also over the years.

Now, talking about spatial issues of infiltration, and now we talk about the little arrows and the big arrows, and where is the big infiltration and where is the small infiltration and what controls the spatial variability. The experts generally concluded that the map by Alan and his co-workers seems generally reasonable in large scale spatial variability. They expected more infiltration into the washes and less in the ridgetops. Alan's map has basically more in the ridgetops because it doesn't have the alluvial cover. It has the exposed fractures, and that's why the water goes down there. The experts expected a little bit more runoff and flow into the washes, and perhaps more infiltration there.

They also said that several of the processes that were neglected by Flint and et al. may be important, including the runoff, the lateral flow at the alluvium/bedrock contact, et cetera, and this needs to be looked at in terms of spatial variability.

The next one is lateral diversion at the top of the PTn, and for those of you that don't know, the PTn is really a non-welded unit here between the two welded units, the Tiva and the Topopah Springs, and the issue is when water goes through the fracture into this unit, is it going to flow laterally or is it going to go vertically through the PTn, and the experts said lateral flow exists, but it's limited to only a few tens of meters, or 100 meters at most, and that becomes then not very important, basically.

The likely places include the PTn, but also they pointed out some other places, such as perhaps at the top of the Topopah Springs here and of course the potential for lateral flow here due to infiltration in Solitario Canyon where you don't have the PTn.

Moving right along, the next one is really about pulses and temporal behavior. We know that the infiltration is episodic here, occurs every few years. And how far do these pulses go through the mountain? Do they end at the PTn, which moderates things, or do they go all the way through the system to the water table, is the issue.

The general agreement was that there is episodic infiltration and PTn dampens most of these pulses, and only in the case pretty much of the fast flow component, the chloride 36 component, the flow-down fault component, is where you may see temporal behavior going far down into the mountain.

Now, they were asked to put--and I witnessed that in some of the elicitations, and so did Ed Kwickless. Kevin demanded to get numbers from them in terms of percolation flux, and all of these different experts used a variety of methods to get that percolation flux, including the net infiltration volumes that Alan and co-workers had developed, including saturation and water potential within the PTn that Shlomo will probably talk about a little bit more later, because he used that a lot, including temperature gradients, because the percolation affects temperature gradients in bore holes, including chemicals such a total chloride, carbon 14, which are affected by water, including perched water conditions within the mountain, because as you know, perched water is a balance between what goes through the mountain, as well as what goes from the perching zone to the water table.

I listed these pretty much in order of importance, so that the more higher up they are, the more experts used these different ones. Gaylon happened to use all of them, I think, or most of them. He's very flexible in his evaluation of percolation flux.

In terms of distributions, I'm going to show you now distributions, and these percolation flux estimates are basically at the repository horizon, right here, which is basically the amount of water expected to be seen close to the drifts.

This is the values they gave for their distributions, and you can average them out to get the mean of some 10 millimeters per year, and you have some fifth percentile of one, and 95th percentile of some 30 millimeters per year as an aggregate for all of the experts.

When you look at the distributions individually, I hope you can see those. This is percolation flux here at the bottom. It's minus 5, zero, 5, 10, 15, 20, 25. The further out you get in the distribution, the higher values of percolation flux are estimated. You can see the experts varied significantly in their estimates by looking at how high these peaks go and how spread they are, and I think the two extremes are Shlomo's, which are higher and spreads out more, and actually Gaylon's, which is smaller values and less distributed.

When you look at the aggregate or the average or the total from all of the experts, this is from Geomatrix report, I don't know if it's finalized, but this is pretty much the final numbers, you get an average percolation flux PTF that looks something like that, and these are the average means for individual experts. It's on the order of 10 millimeters per year, plus or minus, but there is a long, long tail there that goes all the way to, what, 50, 60 millimeters, and I think in Shlomo's case, it goes to 70 or higher. Is that right; something like that?

Moving right along, the question is then here we have an average distribution of percolation flux, how about the spatial variability, and that means how much does it vary spatially in this region. And overall summary from all of the experts was that generally, you'll see the same thing at depth as what you see at the surface in terms of infiltration, because basically they said you don't have a lot of lateral flow, so it kind of makes sense.

They said, as predicted by models, they expect it to be smoother than what is here at the surface because of capillary equilibrium, capillary forces tend to equilibrate things, but some of them said heterogeneities in flow may develop at depth from focusing and un-focusing of flow.

Now, components of flow in the Topopah Springs, fracture versus matrix, and again we are looking at the repository horizon and we are trying to figure out, or the experts are estimating how much of the total flow flows in the fractures and how much flows in the matrix blocks. The following conclusions were reached. The matrix permeability is very low, on the order of micro darcies and, therefore, the matrix can only carry one to a few millimeters per year of flux. Therefore, the rest has to be in the fractures.

So 80, 90 per cent in the fractures; 10 per cent in the matrix on the average, or so.

Other issues, what about fast flow? What about the chloride 36? What about all of that? Most of the experts believe the fast flow is only a very, very small component of the flow, 1 per cent or so in most people's opinion, and that the fast flow, in Ed Weeks' opinion, may occur in many fractures. Most of the experts believe that the fast flow was more localized through major fault zones or fewer fractures than many fractures. So there was some difference of opinion among the experts regarding that.

Seepage into drifts--am I doing this too fast, or is this okay? Good. Seepage into drifts, we are now concerned with percolation flux around the repository horizon. We know only a fraction of that flux goes into the drift, and this is the important parameter. How much of this total water goes into the drift? If none goes into the drift, we don't have a problem with the corrosion because it's probably going to be dry conditions and all of that. But the question is how much seeps into drifts.

There were mixed opinions. Some said none. Others said all. Most of them said water that flows in fracture will generally enter the drifts. Matrix component will go around the drifts. Again, like I said, some believe no water will enter drifts. They also said that the area, the total area that will seep is a very small one, or 1 to 10 per cent, I think that's about the right number, something like that.

Now they also made a lot of comments on modeling issues with regard to the UZ model, and the infiltration. This addresses infiltration and is a summary of their comments. They believe, or some of them believe 1-d modeling is limited as it neglects runoff and lateral flow, but there is some differences of opinions. For example, you can read

Gaylon's comment here, 1-d finite difference model for net infiltration is OK. So not all of them agree in general on some of these things.

Bucket model may not be adequate. You need the mass balance model for infiltration, and they suggested that LBL develop a surface hydrology model.

UZ modeling issues, and infiltration modeling was how do you analyze all the data at the surface to get at this distribution that Alan has. UZ modeling issues addresses how do you evaluate total moisture flow within the mountain. There are a lot of good suggestions. They suggest that you need a dual permeability model above the PTn, and probably an equivalent continuum model below that. And let me describe these two models very briefly to you.

A dual permeability model is where you have two continua, I think is the right name, and one is the fracture continuum and the other one is the matrix continuum, and you solve for flow in both of these continuums, and they also can interact. You get an inhibition and flow between the two.

The equivalent continuum model uses a single continuum approach, but it takes into account fracture flow with approximations.

They suggested that fast paths need to be modeled and more faults added and the sensitivity evaluated. And just to tell you this now, I'm going to come back a little bit later after Gaylon and Shlomo have talked, and give you how we responded to these comments a little bit later.

They suggested transient component of flow needs to be modeled. They suggested investigate alternative models to the continuum models, such as discrete fracture models and Weeps models, more detailed fracture models. They suggested to look at the mass balance of perched water and the water table fluctuations. They suggested that if we are considering fracture flow, the predictability of its fracture flow should be modeled as random. And they suggested to perform uncertainty and error analysis of the heat flux and temperature data to get estimates for percolation flux.

So we will visit all of those in a later talk, with a talk about the response or reactions to the experts' opinions. I just want to finish here briefly with their recommendations regarding data collections.

They suggest to collect water potential, water content and hydrologic property measurements in the ESF, and a lot of their suggestions dealt with to use the exploratory studies facility more for studies. To make unsaturated conductivity measurements a high priority. Collect data on surface water balance. Inject water above a sealed room in the ESF to test for seepage, and actually as we speak, this test is going on in the ESF, and it was planned by DOE a while back. Run UZ model to examine effects of higher infiltration patterns and do many "what-if" studies, and I think this has been a very useful suggestion to me because we always tend to be in the mode of always matching data and matching data and matching data, and not taking the time to look up and say which of the data will break down if the infiltration and percolation flux is more than "X". So that was a very good suggestion, and I'll give you some feedback regarding that. Go and analyze pump test data for perched water bodies to determine drainable porosity. And I think the next viewgraph is the final viewgraph.

This is a continuation of the additional data collection. And they said continue infiltration studies was one of the recommendations. Do a thorough study of small drainage basins above the repository, more accurate measurements of water potential in the PTn. Do an infiltration study of the Solitario Canyon area, this area around here. Develop hydrographs of perched water. Perform large-scale experiments in the ESF with plastic sheets to investigate inflow, again, seepage into drifts kind of issues. Obtain more detailed temperature logs.

So this list of recommendations is not an agreed upon list by six experts. Generally, one of the experts recommended this, another one recommended that, so we had kind of captured all of their recommendations.

So that's about it for the experts' opinions, and

I'll be glad to entertain any questions.

KNOPMAN: Okay, thank you, Bo. That was an excellent summary of a lot of material. We'll take some questions now from the panel. Priscilla Nelson?

NELSON: Nelson, Board. Thanks, I enjoyed that.

BODVARSSON: Thank you.

NELSON: Many of the recommendations I very much enjoyed. I noticed one that Gaylon said, and I hope you'll say something about the future, and maybe you can respond.

The Board's been thinking a lot, and I believe you have as well, about the influence of ventilation. And when we hit this unsaturated zone condition, it seems the combination of ventilation and perhaps maybe some rock mass response to tunnelling, maybe some loosening, some opening of some of the apertures may be important, may be something that is actually very interesting in understanding the performance of the near-field environment. So from that, can your model take that sort of a response into account, and was this explicitly discussed in the context of the elicitation?

BODVARSSON: let me just clarify the question so I understand it totally, is that you're talking about now the effect of stress releases and aperture increases or decreases?

NELSON: I think in general, tunnelling is probably going to result in, in some places, general loosening, opening of apertures, loosening of the ground conditions, whether it's de-stress or loss of support, whatever. But it seems to me in terms of the unsaturated zone condition, that may have an impact on where the water is moving and where it's going. And if you combine that with ventilation, it may have a fairly powerful effect on the seepage into the drifts.

BODVARSSON: I may not be the best person to answer it, but I will give it a try. I think the ventilation issue is a really good one, and that can help us a lot in terms of seepage into drifts who have ventilation. I know DOE is addressing this issue.

With respect to the stress effects on the permeability, I have a personal opinion regarding that, and that is the following. the Topopah Springs has the permeability of five darcies vertically in the fractures, and one darcy horizontally in the fracture. If you are not familiar with darcies, this is like ten to the minus 12 meters squared, something like that. If you're not familiar with that, I can't help you. But that, to me, is a very large permeability and I really wonder if stress changes through to excavations are going to affect it tremendously, because it's so large. If it was a lot smaller, I would expect it to. That's my feeling. If others want to entertain these questions, please feel free, but that's the best I could do.

KNOPMAN: Dan?

BULLEN: Bullen, Board. I was interested in some of the experts' elicitation results with respect to seepage into the drifts where some thought that there would be some, some thought that there would be none. And I guess I'm interested in how your model handles both the spatial and temporal variability. Are there actually spots in the repository that probably won't see any water, and does your model predict that? And how does that interface with the other parts of TSPA?

BODVARSSON: That's a very good question. The basic model that we are talking about here is the mountain scale ambient model. In our latest report we just issued, we consider the entire model, and we also, in order to bridge the gap to the drift scale model, we have detailed two dimensional cross-section with hundreds of thousands of elements that consider individual drifts, and that would provide the bounding conditions for the ambient scale models that they're also working on at LBL to address this issue.

In the repository area, the infiltration at the surface varies from zero to 20 millimeters per year based on Alan's latest maps. So there's sufficient significant variability. When you carry that down to the repository horizon, that variability has diminished because of capillary equilibrium and all of that, but you still have areas where you have practically no flux going through the repository, just as was suggested, but the maximum flux now is much lower than the 20. It's now down to around 10 or less.

So you have a spatial variability in flow around the drifts, and we capture that really well in this two dimensional cross-section I told you about where we actually do a frequency diagram in terms of percolation flux that is available to a drift, to look at that variability, and we are now using that in the drift scale calculations to predict if any of that is going to do into the drift, what is the critical percolation flux for flow into drifts, how does it depend on the geological conditions, because the geological conditions have different fracturing and different conditions, and then now DOE is sponsoring a niche study in the ESF to actually examine the theory to prove it a valid theory.

KNOPMAN: Okay. Priscilla has a quick followup.

NELSON: I was deflected before. Does your model include ventilation as a possibility, and were the experts asked anything at all about ventilation?

BODVARSSON: The model, the UZ model includes air flow. It includes the ESF, so it includes the air flow in the entire mountain, and it includes the exploratory studies facilities. We have not explicitly put in the ventilation effect into the ESF, which we can do, since we have air flow in the entire model.

The experts, and Kevin, you'll correct me if I'm wrong, were not asked a lot about ventilation, but we really, in some of the expert elicitation, like Shlomo will tell you that he used some estimates made on maximum possible seepage into the ESF based on ventilation data that he got. So they looked at a lot of literature, so I'm sure they were exposed to a lot of ventilation type data.

Did that answer your question okay?

KNOPMAN: Jerry?

COHON: Cohon, Board. I'm interested in the procedural issue of how an overall summary conclusion is reached. For example, fast flow is only 1 per cent or so, did Geomatrix arrive at that overall conclusion, or is that your interpretation of the--it's Number 9.

BODVARSSON: Okay.

COHON: Is that your interpretation of the seven experts or is it Geomatrix?

BODVARSSON: No, that's my interpretation of the seven experts.

COPPERSMITH: Maybe I can respond to that?

BODVARSSON: Yes, okay, you can respond.

COPPERSMITH: This is Kevin Coppersmith, Geomatrix. In fact, I was going to make a statement anyway. I think one of the problems with a summary like the type that Bo has had to go through is that in fact it's a summary. There are seven interpretations related to each one of these issues, and because the left-hand column in these slides needs to be an overall summary for that issue, it by necessity is leaving out the full range, or leaves out even the flavor of the full range of interpretations for a given issue.

For example, the issue of the spatial distribution of net infiltration, while some experts would agree generally with the maps that have been produced by Alan Flint and others, other experts would say I don't agree with the spatial distribution in those maps for the following reasons; I would in fact argue for a different spatial distribution.

So it's difficult to put together this type of summary. I think the best thing to do is to look at the report and to look at the individual interpretations to get the true range.

Now, the challenge for someone like Bob Andrews is how do you then use something in TSPA, and he'll talk about how you're able to use that. You are looking at, no matter how you look at it, there's a range, and if there are three or four experts who agree on the central part of that range, there will be more weight to that central part, that central estimate.

I wanted to also, just a clarification, it's along the same line, when we deal with the aggregate distribution of percolation flux across all seven experts, that's not in any way an average. Those are combined together. Equal weight is given to each expert, and the tails of those distributions reflect the tails of the distributions of the experts. So they're not in any way an average, and Bob will talk a little bit about how those are sampled. Obviously, they need to be made discrete for a TSPA calculation, but they are a combination, an integration, assuming equal weights.

COHON: As a general matter, are summary conclusions, however they're represented, at all the responsibility of your firm, or is that entire the client's responsibility?

COPPERSMITH: In terms of the representation of the--

COHON: Right. You stand behind the seven individual statements?

COPPERSMITH: Yes.

COHON: Do you also--

COPPERSMITH: Yes, we have a chapter in the report, and again this is an M&O report that then goes to DOE review, that in that chapter, we summarized the assessments made by the experts. We first began with a discussion of what the issue is. Then we talk about, in general, here are where common conclusions are reached. However, here are the range of conclusions reached, and cite those. So we attempt in that discussion to represent not only the central--

BODVARSSON: I think your question is this comes directly out of a table in the Geomatrix report.

COHON: Okay.

BODVARSSON: This thing here comes directly out of that table. The summary of the expert elicitation was done by Geomatrix from the elicitations. So this comes directly out of that report.

KNOPMAN: What about the left-hand side?

COHON: Let me state it this way very specifically. Although I may not see the connection between fast flow is only 1 per cent or so, and the seven things I see on the right, do you stand behind that? I mean, do you agree with that summary? Is this--

COPPERSMITH: Yes. But, again, it doesn't come just from the right.

COHON: I understand. It's hard to do justice.

COPPERSMITH: Yes. I would say, for example, the fast flow component is believed to be a very small component of the flux, on the order of a few per cent or less. Maybe it's 1 per cent. But we can look individually at what the experts said. That's the general conclusion, yes.

KNOPMAN: All right. I'm just going to jump in with a question, and then go to Alberto, and it has to do with this viewgraph and the one before it where you talk about fracture flow probably carrying about 90 per cent of the flow, and then you're talking about fast flow only being 1 per cent. Connect those two pieces of information for us. Why is it that if you've got that much going through fracture flow, it is your fast flow is so small.

BODVARSSON: Okay, that's a very good question. The reason is the following, and let me see if I can find that viewgraph I put on the left-hand side for you there. It can be useful to explain it a little bit.

KNOPMAN: It's Number 8.

NEUMAN: Can I make a brief comment? I'm Shlomo Neuman, elicitation panel.

I think that the summary that you see here does capture much of what the various panelists think, but by no means does it capture accurately all aspects. In fact, in the case of whether or not the fast flux is a small proportion or a large proportion of the total flux, as I will show you later, my opinion is quite different.

KNOPMAN: Okay, thank you. Bo?

BODVARSSON: If you look at the--let's say they have a 10 millimeters per year flux, and we are saying, or the experts are saying that nine of the ten go through the fractures, one goes through the matrix, then your question is why is it that only 1 per cent of that is fast flow, is your question, and I think the answer is as follows, at least in my mind. I believe it's 1 per cent or less than 1 per cent.

Our model studies, LANL model studies, have shown similar things.

And why is that? The reason is when you do calculation with a model and you calculate probable time it takes to get from this point to here, and if you have to go through the PTn, which is a porous medium unit with 35 per cent porosity and it's 100 meters thick or so, it takes you several thousand years just to get through here, at least on the order of a thousand years to get through there.

The fast flow components, which is the chloride 36, tritium component, is a 50 year probable time, and we see that, or LANL sees it generally associated with faults or maybe structural discontinuities in the PTn where it goes through very quickly. We don't believe that that happens in very many locations compared to those locations where you don't have those discontinuities.

Secondly, and more importantly, the perched water ages that we see here, which we think is representative of the majority of the fracture flow, has ages on the order of 5, 6, 7000 years. We think that is more the average travel time for the fracture component of flow to get down through the mountain. And, therefore, if you can imagine in your mind a distribution going from zero years with a mean of 5000 years, or something like that, and then going to 10,000 or higher with the slower fracture component, that the 50 year component will likely be small. So that's how it is in my view.

KNOPMAN: Abe?

VAN LUIK: We kind of asked this question of Bo when he explained his new modeling results to us, and I think at that time, it was real helpful for you to describe the nature of the fracture network that you're talking about, that it's a distribution of very fine capillary type fractures, with a few large ones.

BODVARSSON: The other argument that Abe is talking about is the following. What is the fracture network of Yucca Mountain? And this is a very important one. Are there few major ones, or are there a lot of smaller tiny ones? I believe, and our report represents this, that there are thousands, if not millions, of features, small, deep through the fractures, driven by gravity and capillary pressure.

Why is that? There's a lot of data that suggests this. Number one, 8 per cent of the fractures are filled with calcite. 8 per cent of millions and millions of fractures is already millions of Weeps. Number two, whenever you close a part of the ESF, you usually have humidity go up from 40 per cent to 90 per cent or higher in one or two days. To me, if the matrix is so tight you cannot get this through matrix flow, that means close to these regions, there must always be fracture flow. So that means spacing very close together.

Thirdly, saturations in all of the geological units over kilometers are fairly uniform as measured by Lorrie's data. If they are 90 per cent over kilometers, over the same geological units, if you would have a lot of flow here and no flow here, you should have moisture tension and saturation with a high moisture tension and low saturation here, and the reverse here. So there is a lot of data to me that suggests that we have many, many Weeps going through the mountain that kind of all of it carries a very small amount.

KNOPMAN: That's good. We're running a little long here, but I don't want to cut off the discussion. So if it's okay with everyone, I'd like to let this flow a little bit. We'll just delay the break for a few minutes. Alberto has been waiting, then Dick and then--

BODVARSSON: One quick comment to explain this 1 per cent here that everybody is questioning. You take a look at Gaylon's, less than 1 per cent, Glendon 5 per cent, Shlomo at least in this version said fast flow component is small part of the total flux, Daniel Stephens, fast flow component is small, 1 per cent of the total flux. Chloride 36 is localized, only a small number of fractures could be carrying fast flow, and since there are millions of fractures, you come to the conclusion that this has to be a small number. That's how I derived that.

COHON: Fair enough. I mean, what prompted my question, this is Cohon, Board, two said they're not sure. One said 1 per cent.

BODVARSSON: Right.

COHON: Four said small.

BODVARSSON: I have to--

COHON: I mean, I take what kevin said before about that there's more to it than you can capture in a summary.

BODVARSSON: Absolutely. If I were to summarize what they agreed on, it would be an easy task. You wouldn't see anything here.

KNOPMAN: Right, no consensus driven process here. Alberto?

SAGγIS: Very quickly, what I would really like to know, and I'm sure many people would like to know, is we're going to have like about a hundred miles worth of tunnel drifts, and there's going to be about 100,000 square meters of a roof footprint, if you will. So that's about a million square feet.

Now, I would like to know how many leaks I'm going to have in there that are going to be the equivalent of, you know, a drip, consistent drip, drip, drip. That's the kind of summary I would like to see coming out of those numbers in some fashion, because that will give us an idea of how many of the containers are actually underneath something resembling a severe drip. Do we have in here something that may approximate that answer if you play with those numbers?

BODVARSSON: Well, remember the one that said seepage into drift? There was one that said seepage into drift, and that's probably the one the experts least agreed on. And that's pretty much what you are talking about. So I said here on top mixed opinion. Some of them thought there was no water within the drift, and others said all the water in the fractures will end in drifts. I think in all fairness, the answer to your question is controversial at the current time, and that's why DOE is doing these tests in the ESF where they actually introduce water above the drifts and see how much goes into the drifts.

So my personal opinion is that you will not see a lot of flow into the drifts because there are so many of these seeps, each one carrying a small amount, and that you are not going to see a lot of--

SAGγIS: Sure. But not seeing a lot is a relative term when you're talking about the distance from here to Yucca Mountain worth of tunnel. So not a lot may mean that there may be like 20 or 30 of those places right there on top of containers, and if 20 of the 10,000 fail, well, a lot sooner than the rest, I'm sure that that throws a lot of the projections dramatically out of kilter.

BODVARSSON: I think there's uncertainties here that DOE is testing right now.

COPPERSMITH: May I make one comment? This is Kevin Coppersmith, Geomatrix, again.

On that issue, I agree it's very uncertain. But there's two aspects to it I think that you're asking, and I'm involved obviously in the waste package degradation group, too.

Number one, this doesn't include at all the thermal hydrologic effects, the thermal pulse driving off of water and what will happen. This is ambient conditions. So presumably this would be either some far distant, either a hypothetical now or some far distant time period after everything has cooled down, and you're dealing just with the drift itself and no thermal halo around it.

But the question is not only what area would see the potential for seepage, and let's say it's small, I think in general the area that would be affected is small, say 1 to 10 per cent, but that doesn't answer the question that you the engineers would want to have. The question you're interested in is predictability. Is there a way to say where this seepage would occur, even if it's a small percentage, and how much would occur? And so those are very difficult questions. I would say in general, on the issue of how it should be treated spatially, I think two experts responded to that, I think Shlomo and Ed Weeks said that I think probably because of our uncertainties, it at this point should be modeled as a random process, not because it is necessarily truly random, but we're highly uncertain about the predictability of the spatial location.

And the issue of the total volume for a given seep is also very difficult. We have a well ventilated, as all the experts said, we have a well ventilated ESF that's capable of drying up, you know, high amounts of flux, maybe up to 200 millimeters a year are dried up through the present ventilation. So we simply can't use that as a basis to get at the volume. We can say that there hasn't been, you know, a water hose type seep during the time the ESF has been there, but otherwise it puts a very weak constraint.

Now, Shlomo hopefully will talk a little bit about where he looked at where ventilation was turned off and some of the effects during other time periods, and put some upper bounds, but again, it puts a large upper bound constraint only. So this is a very difficult problem. It was for the panel, and it will probably continue to be before there's more data.

KNOPMAN: Okay. Speaking of bounds, we're taking two more questions, Dick Parizek and then Victor.

PARIZEK: Parizek, Board. Kevin, did I understand that you might truncate extreme opinions, or extreme values in order to go toward a central tendency as advice to the

Program?

COPPERSMITH: You heard me say that?

PARIZEK: Well, I thought I heard you say that. If there are extreme values, you would tend to take the middle.

COPPERSMITH: No, absolutely not. My life is built around uncertainty characterization. To say that I would truncate, no, that's not true.

PARIZEK: I guess I just wanted to know how it would be used then, or whether you would put the extremes in the model?

COPPERSMITH: I think it's important, again, it's important in interpreting these results, the mean of the mean, each expert has a probability distribution, let's look at percolation flux. Each of them have a mean and a median and other moments of those distributions, and so on. They're not necessarily log normal. But they have tails to them, and some of those tails are very large, and all of the experts explain what those tails represent. In all cases, the tail by definition is a low probability set of circumstances. So when we say 30 or 40 millimeters a year, all of those experts who have a tail that goes out that far will agree that it's a low probability. But the issue is that they can find a set of circumstances that are still allowable, improbable with the present data, but still allowable within the present data that we have. And that's where they are on those, and that's what we're looking for in terms of that uncertainty characterization.

PARIZEK: that goes into a model run, though, somewhere.

COPPERSMITH: As Bob Andrews will show, some of those upper tails are going to be used to look at for sensitivity, for example. They can be used in a number of ways, but we will look at the results that would come.

PARIZEK: The reason I come up with that is if you have like an ESF, pre-ESF panel meeting, the concept of the model is it's generally pretty dry. Most people would have thought it was pretty dry. And then the modeling that was done with only well bores, '95 version, comes up with some finding. But now you put the tunnel in, and now all of a sudden, it's wetter, and then if you do more work, you might find it still wetter, that maybe the extreme values that a few people think might occur there, may be more common than what you originally perceived. I don't think you want to throw it out. I'd be worried if you threw it out prematurely, because a wet mountain was not the concept of the Nevada Test Site in the early days, or the mountain nearby. I think that's partly what's come out of the tunnel.

KNOPMAN: Okay. Victor?

PALCIAUSKAS: I'll just make a very short, brief comment. Clearly, the peer review--the expert elicitation captured what I think Kevin was emphasizing, the uncertainty rather than averages, and I think that's the great value of it, because it shows cumulatively about a 20 per cent probability that it's greater than 15 millimeters per year, and I think that's a very, very important result, because just two years ago, I remember the project was defending much, much lower percolation flux, with the saturation data and other data implied there's diversion in the PTn. Clearly, there was a large uncertainty with that assumption at that time, because you're up at least an order of magnitude.

So I think this realization that there is a tail, a real possibility of a higher percolation flux, maybe 20 per cent, whatever it is, it's a very, very important result. And I guess it's too early now. You'll talk about it after the next person, but I really would like to hear an answer to that at that time.

BODVARSSON: In the response section later on, we will tell what we did with the model, and then get into the issues like you brought up, or this gentleman brought up about what you believe over the next ten years the variability will be.

KNOPMAN: Thank you, Bo. We're going to take a ten minute break instead of a 15 minute break, and we'll start promptly at 25 after 3:00.

(Whereupon, a short break was taken.)

KNOPMAN: This part of our program is an opportunity to hear from two of the seven experts that participated in the elicitation process.

We're first going to hear from Shlomo Neuman, a hydrologist with the University of Arizona. After Shlomo, we will hear from Gaylon Campbell, who's at Washington State University, and following their presentations, we will get kind of a lessons learned overview from Bob Andrews and Bo Bodvarsson.

And, again I'll repeat what I said in the start of the session, we strongly encourage our panelists to ask questions of one another, respond to questions from the Board and Staff, and truly make this an interactive session. I think the first part went well, and I'd like to keep the pace up. Shlomo?

NEUMAN: Thank you very much. Can you all hear me?

I want to thank the Board for inviting me. And it seems to me that perhaps the Board has made it deliberate that it has invited two of the seven expert elicitation panelists who appear to be somewhat on the two extremes of the spectrum of opinions, and certainly in terms of where the peak of the distributions lie. One could perhaps characterize Gaylon, although I'm sure not politically, as a leftist, and myself as a rightist.

Very quickly, the project objectives you have heard about, I will not go in detail through that, except to mention that we were asked to characterize uncertainties in both model and parameter. And by model, I will mean primarily conceptual aspects of what goes into a model, not so much the mathematical model.

We were specifically asked to give our opinion on ambient percolation flux through the repository horizon. This was the primary goal. And then what we understood to be a secondary goal was some information or some opinions about seepage into the repository.

You have heard about the methodology. I will not go into it, except to emphasize once again that there was both interaction and freedom for us to select our own opinion. An opinion is what you should view it as, because we only had so many days to devote to this project. Most of that time was taken, or much of that time was taken by workshops, field trips and so on, so we were able to generate impressions. We were able to do some quick and dirty, very dirty for some of us, calculations, as you will soon see. But the rest is impressions and opinions.

So what I will do I thought is take you through my own very personal thought process, which is summarized in the elicitation appendix in about 15 pages or so. So this is a summary of that thought process and some of the calculations that go with it.

First, the very obvious, that there is no precedence for assessing unsaturated flow under such rock/climate conditions on such space time scales. We all recognize this, but nevertheless I feel, and many of us feel that over the years, a rick amount of generic knowledge has accumulated which should allow us, and here comes the proviso, given appropriate site and experimental data, to make intelligent inferences about subsurface flow at Yucca Mountain. And it is my opinion that for such inferences to be credible, our concepts, theories, models, though processes and calculations should be based as much as possible on actual data.

And it is for that reason that I'm going to base my calculations here today on data even though it is going to be entirely obvious that those data are highly uncertain. And in response to one of the questions that was asked by the Board earlier, do I believe my numbers, the answer is absolutely not. I will be most surprised if any of the numbers that I come up with would turn out to be true.

And for that reason, what I would like to ask you to do with me is follow my thought process and do not hang your eyes on the numbers. The numbers are the best that I can come up with today, given what I know, but I believe that with additional data collection, one should be able to do much better.

And just to illustrate how important data are, let's talk about what I consider to be one of the better understood processes relevant to Yucca Mountain, and that is heat flow. If there was enough and reliable data about temperature, heat flux, conductivity, heat conductivity, then I believe that our understanding of this process should be such that we would be able to derive from it credible estimates of moisture flux on various spatial scales. And it is my understanding that one of the USGS scientists is in fact going in that direction by taking a very hard look at the existing data.

Unfortunately, the data that we were presented with by February of this year, which is when the process ended, were not sufficient to convince me of the quantity and quality, not that they are not good or insufficient, but it's just simply not enough to do accurately what I'm proposing one should be able to do with those data.

That's, in my opinion, the most clearly understood process which with additional and better data, should be able to be very helpful. On the other hand, it is my opinion that the least understood process of relevance to what we are trying to discuss this afternoon is the process by which precipitation, rain and snow, is transformed into deep percolation below the root zone from where we think it will then percolate down more and reach the repository.

I think, and, Dick, you can see that my man is not smiling here, assessments to date based on near-surface measurements and models are not entirely convincing. In the last page, last two pages of my viewgraphs, of which you have copies, you will see why I do not know if I'll have time to get into the details.

More importantly than any technical arguments that one can raise against it, I worry about the fact that nowhere have assessments of this kind been verified on space time scales comparable to those of Yucca Mountain. So even if we have a good warm fuzzy feeling that the methodology may be correct and the data entering into it may be correct, we have no way to verify it, at least I have not been shown so far any ways to do so.

And so, therefore, I conclude that the key to unravelling the nature and rates of subsurface flow in the unsaturated zone at Yucca Mountain, does not lie presently at the surface. I have a feeling that no matter how much additional information we will collect at the surface, we will still face a huge amount of uncertainty in this area. Therefore, I propose that we seek the answer at depths.

And here, as you will see, Gaylon and I are on the opposite sides of the spectrum, and I'm not sure that this particular dichotomy of opinion, for example, came out very clearly from the summary. This is one example of where I think that you, if you're really interested in the details, you may have to dig into the individual write-ups from the elicitation report.

I'm, therefore, proposing a conceptual framework on the basis of which I think this kind of an analysis could be done, and I'm going to illustrate to you a very, very crude example of it.

I consider among the more reliable concepts, models and data that have been collected at Yucca Mountain those which relate to pneumatic monitoring and air injection. One reason for that is I myself am involved with air injection. I have a pretty good feel for what they can and cannot do. So maybe I am biased.

But if you do accept those, then there are many things that they reveal about the subsurface system that many of us recognize, and perhaps some of them may be new. In the welded units, in the Tiva Canyon and the Topopah Spring in particular, pneumatic data represent fracture and fault properties. Most of these happen to be relatively at low water saturation and, therefore, large portions of them are open to air flow.

The Tiva Canyon and the Topopah Spring are spanned by pneumatically, not necessarily hydraulically, interconnected networks of fractures and faults that conduct air with relative ease across considerable distances, though there is a strong indication of horizontal--as Bo has mentioned earlier, perhaps one to five, or so.

Pneumatic monitoring and injection data provide consistently high permeabilities, consistent among themselves and within themselves. Now, since their saturation is low relative to the matrix, this permeability is probably quite close to the network intrinsic permeability. So here we have perhaps information about the large scale as well as small scale injection, small scale, pneumatic monitoring, large scale, permeability of the fracture system in these units.

Because matrix permeability here is orders lower than this permeability of the fractures, flow in these units is most probably dominated by the fractures and the fault.

I also made a suggestion, which Bo has mentioned, that one should perhaps be able to get air filled porosity out of the injection data, and I'm sure that something of that sort is being attempted.

Here's where the man doesn't smile again, and that is that in order to be able to translate this into flow under unsaturated conditions of water, it is not enough to know the intrinsic permeability of the fracture system and the fact that it is a well interconnected system, but one would need to have a much better understanding of the modes, rates and directions of water flow through those fractures and faults within the welded units.

And unfortunately, there is extremely little information about both the mechanisms and the parameters that control this flow. You have a list of areas that I think make any predictions of flow of water through these fractures extremely difficult. Fractures are partly filled with minerals. We don't know to what extent water flows through the minerals, the water extends through open spaces, to what extent the intersections control flow, to what extent fracture planes control it, to what extent there is channelling, to what extent there is capillary film flow.

The one thing that the models do account for, though there is very little direct information about the parameters to enter here, is about fracture matrix, movement of water and perhaps chemicals between the fractures. This is the process that's included in the so-called dual permeability, dual porosity models.

So I reach another conclusion, and that is that the key to assessing the repository level percolation flux lies not within the fracture dominated parts of the system, but within the matrix dominated PTn and possibly within the ESF. And it is for that reason that I'm going to base virtually all of my calculations on information that comes from the PTn. We know so much more about matrix flow than we know about fracture flow.

We do have a unit which overlies the Topopah Spring which is matrix dominated. Why not use that unit as the main source of information for our calculation. You have a list of reasons why it is quite obvious I think that flow in the PTn is matrix dominated.

Now, we all know from the fact that bomb-pulse isotopes have been found in water both within and below the PTn, then some rapid flow must take place through the PTn. Mean seepage velocities through the PTn matrix is, as we will soon see, and I'm sure you know, too slow to account for the bomb signatures. Bomb-pulse isotopes within the PTn matrix suggest that fast paths in the matrix may exist, not only in fractures and faults.

Fast flow in matrix can take place for a number of reasons that I have enumerated here, and to the extent that this technical question is first, I'll be happy to come back to it later, but essentially I'm of the opinion that within the PTn, fast flow may occur both within the matrix and in faults and fractures, and it's going to be extremely difficult to distinguish between those.

This I am not sure has been taken into account in most of the models that I have seen as of February of this year. I have no proof that this is the case, but I strongly suspect that this may be the case. Such preferential flow channels may persist or they may adjust dynamically to variable surface infiltration.

Regardless of whether or not they develop this in fracture faults or the matrix, they will occupy a minute proportion of the rock volume and are therefore, in my opinion, unlikely to be observed in the field.

Now, this is what I said. I did not say, as the summary has indicated, that flux through fast flow channels is small. What I said is that the area occupied by those channels is small, and I'll come back to that and you'll see why I say that.

You did hear in the summary that there does not appear to be evidence to support or deny the existence of extensive lateral flow within the PTn, and so my calculation, therefore, is going to be based on the assumption that flow is vertical, though it is very clear that it must have at least some horizontal component.

And, again, I want to ask you to think with me more in terms of the concepts that I'm suggesting here than in terms of the actual numbers, and the reasons will be evident from this particular page here.

What I have taken upon myself to do, since the models that were presented to us did not focus specifically on the PTn, but tried to incorporate everything into them, the Tiva Canyon, the Topopah Spring part, and so on, I have decided that I really had no choice but to do my own calculation based entirely on the PTn to see to what extent it agrees or disagrees with other calculations that were done through other modeling efforts.

I fully recognize in doing this that fluxes and velocities vary considerably in space time and with direction, as well as with scale, but I'm going to ignore it for the sake of this very, very crude bounding calculation.

I'm only going to seek some ideas about mean vertical flux and velocity, primarily through the PTn, and I'm going to make the very crude assumption that the flow can be divided into a binary system, a slow system which would represent the bulk rock, and a fast system which would represent fast or preferential channels without necessarily saying much about what they are and where they are, though suspecting that it is in the PTn they may occur in the matrix.

What I have done is taken a summary table from a recent report by Alan and maybe Lorrie Flint, I don't remember who the others were, which contains summary information about matrix properties and state variables of seven PTn units. If somebody wanted to repeat this calculation in more detail, one should go back into the original numbers from which this summary derives and repeat this calculation on a well by well, layer by layer basis. I have not done that.

I have averaged the porosities, which I'll call phi, the saturation, S, within the PTn. I've taken the geometric average over the saturated hydraulic conductivity, which is the only one listed in that table, and came up with a number which you will see is about 20 times smaller than the number that Gaylon is going to use later in his calculation, and yet he will get a much smaller flux from his calculations than I do. Food for thought.

To date, there have not been any reliable experimental data on the relationship between hydraulic conductivity and saturation within the PTn. To date, I mean as of February. Or information about hydraulic conductivity at ambient saturation within the PTn, only what some people call data, but what is really calculated indirectly from curves of water content versus saturation through a model that is quite popular among hydrologists and soil physicists.

What I have done is called up Lorrie Flint and asked her what is the most recent word about these numbers, and she was good enough to send me some unpublished results from two samples, two replicates on each sample, which give information about hydraulic conductivity versus saturation. I have used this not because I believe the two samples are enough, but because this is the only thing that I had. And if I have a suggestion for further work at Yucca Mountain, my first suggestion would be please go out there, support Lorrie's effort and have her come up with hundreds of measurements like these, because we need them.

So what I did was used just the relative hydraulic conductivity versus saturation, did not use the absolute value, in order to tell me how to reduce this number so that it will correspond to a saturation of 50 per cent.

As you have already heard before, this is an example of data from one weld within the PTn. The suctions are extremely low, on the order of .1, .2, and quite uniform, and so I think there is agreement between many panel members that it is quite reasonable to assume that flow there is gravity dominated under a unit hydraulic gradient.

With these numbers, one can then calculate a matrix flux within the PTn. I give you 6 millimeters per year even though as I said I don't believe in the number, otherwise it would be 6.5 or something, and I considered this to be a lower bound, assuming that my numbers are correct, that all the data are correct. Why lower bound? It disregards fractures and faults. It disregards what I consider to be possible fast flow channels in the matrix. As we will see, it cannot account for bomb pulse signature. It disregards increase of K with scale. I'm taking small scale samples and applying them to the mountain.

Just to show that this is not entirely far fetched, I'm very encouraged by the fact that independent calculations by June Fabryka-Martin and her group, Andy Wolfsberg is here, and their group at Los Alamos show using a dual permeability model, something quite similar, and that is that the minimum flux that can explain minimum flux, meaning a lower bound, that can explain the observed chlorine 36 range from one to five millimeters per year. And you have a little bit more information there than what I'm going to show you about this.

There's agreement more or less with chloride balance calculations, even though I am somewhat skeptical about those calculations. Nevertheless, it's quite interesting that they come out to be similar.

Now, what can you say about velocities? The average volumetric water content within the PTn matrix is the product of the saturation and the porosity, I'm using my average values, I get .2, this gives me a velocity of about 30 millimeters per year, clearly too slow for bomb-pulse signatures, but agrees quite nicely with background chlorine 36 ratios, again, this is June Fabryka-Martin and her group, based on reconstructed atmospheric inputs, suggesting 10,000 to 13,000 years, depending on what depths you take, agreeing more or less with the 10,000 year span that it took to increase the values to those that we see within the ESF as background, compared to the bomb-pulse signatures.

So this is the range of 1,000 to 1,500 and, again, I'm not the first one to show this kind of agreement. These numbers are within the ballpark of what others have obtained.

KNOPMAN: Shlomo, excuse me. Could you maybe try to summarize in about five minutes so we'll make sure that we have time for lots of questions?

NEUMAN: Will do. Okay, let me go to the one thing that causes my estimates to lie much above those of my colleagues, and that is my way of getting an upper bound. If you believe that these numbers, or if you agree with me that these numbers might represent the lower bound, then the question is how can we possibly obtain an upper bound.

What I have used is the only piece of information that I thought was available to me. I don't like it, but I can't think of anything else. And that is according to Joe Wang of LBL, he has measured moisture flux into the ESF during weekends when ventilation was shut off, and he told me over the telephone that he has calculated this to be of the order of 50 millimeters per year. I have, therefore, taken this to be the upper bound. I considered it to be too high, but I was not able to come up with anything else that would constrain the flux for me from above.

The last point that I want to make, given time constraints, is that if one has information or estimates of the flux in fast paths, and the velocity in the fast paths, one should be able to estimate the porosity associated with the fast paths, which you could view either as the rock volume occupied by fast paths relative to a bulk rock volume, or more interestingly, the probability of encountering a fast flow path.

You have the calculations, you can check me on that, but I'm finding that within the Topopah Spring, the fast paths occupy relatively, or you have a probability of encountering one anywhere from three times to the minus four, to two times to the minus two. I believe the lower numbers more than I believe the higher numbers, but again, I have no real way to constrain them.

The question of how many of these and what your area is going to be cannot be answered because we have the product here of the area times the number, and unless we know one, we cannot tell the other, and I have no idea what any one of these might be.

And my final point is going to be that I am the one, or maybe there are others as well, but I certainly feel quite strongly that fast paths, both within the fractures and in the matrix, could occur under unsaturated conditions. You do not need full saturation in order for flow to be fast in some parts of the rock. I, therefore, do not feel that it is necessary to anticipate seeps into the repository, though I cannot say that they will not appear. However, the probability of those seeps I anticipate can only be less than, and certainly not more than the probability of encountering such fast paths, which I have calculated is in this range.

Do I believe the numbers? Again, I've used two values of saturated hydraulic conductivity because I have no others. I have used an upper bound based on measured moisture flux into the ESF because I have nothing else.

Is there a good chance of improving upon my calculating, making them more detailed and more reliable? The answer is yes, by all means. There's many more data, looking at the data in a spatial, both vertical and horizontal context, and I think that much of that data exists, but with an additional effort, it could be made much more complete.

Thank you. I'm finished because of the time.

KNOPMAN: Okay, thank you very much, Shlomo.

If my colleagues on the Board agree, I'd suggest we maybe serve up a few questions now to Shlomo, hold off on the bulk until Gaylon has his chance to make the presentation, and maybe we'll ask both gentlemen to stand up at the same time and take questions from us in that manner.

So a couple preliminary questions now for Shlomo?

(No response.)

KNOPMAN: Want to wait? Gaylon?

Our next speaker is Gaylon Campbell with Washington State University.

CAMPBELL: Well, when I got caught writing down the questions that were going to be asked, it reminded me of the way you tell the difference between a graduate student and an undergraduate student. When the professor walks into a room of undergraduate students and says good morning, the class responds with good morning. When he walks into a class of graduate students and says good morning, the students write it down.

Now, the information that I'll talk about here today is information that we have received in the three workshops that were organized for the expert elicitation, plus a couple of visits to the Yucca Mountain site, and personal communication with a number of the scientists who work on the Yucca Mountain project.

I don't consider at all that that makes me an expert on water flow in Yucca Mountain, but I think it may have some use to the project itself to see whether the complexity that's involved in this whole process, whether the knowledge that's gained can be transmitted, first of all, to people who have some knowledge of the specific things that are being done, and then finally to the general public. And so I think probably this process of expert elicitation has some value in that regard.

As Shlomo has pointed out, this is undoubtedly the largest project ever undertaken on modeling of flow in the unsaturated zone, that there really is no precedent for the things that are going on in this project. And in fact, use is being made of theories at kilometer scale that we're somewhat unsure of, even at the laboratory scale, and that measurements are being made on rock and rock properties that are even difficult to apply to soils that are much simpler systems to work with.

So you can see something about the level of complexity and the level of difficulty that the scientists are dealing with as they work with Yucca Mountain trying to estimated unsaturated flow.

Now, you've seen several versions of the kinds of questions that the expert panel thought they were being asked. This is my version of those questions, questions about the approaches that have been used, the reliability of the models that have been used to estimate fluxes, and then we were asked to estimate, to come up with our own estimate of the percolation flux. I feel quite a bit like Shlomo. I feel like I can respond better to the first questions than I can the last, but I realize that the interest of a lot of the people here, and those people who are trying to get on with the Yucca Mountain project is in the actual numbers.

I don't consider the expert panel to be very expert in coming up with the numbers themselves and would think that the scientists that have been involved over a period of years in the project would be much better sources of that kind of information. But I still succumb to the temptation to come up with numbers, and I'll show you a few of those, just as Shlomo did, as we go along.

Now, Kevin seemed to feel like the focus of the questions about modeling was on the Berkeley model. I didn't really understand it that way. To my way of thinking, there were many, many models presented to us over the course of the elicitation, and that I've tried to group those models into three groups here. One group is the numerical simulations, trying to estimate the percolation flux by numerical codes. But another approach to modeling the water flow through the mountain is by using the observations and measurements that are made within the mountain. And the third way to model is based on the tracer techniques that have been used and applied. And these last two, while the first method can attempt to predict what might happen in the future, the last two methods essentially tell us what may have happened in the past, or may be happening at the present time.

In the first category, a lot of different models were actually presented to us, and I've chosen three here to mention specifically. The Berkeley model that Bo talked to us about a little bit earlier here that you've seen some of the details on, the Los Alamos model that we haven't seen much of here, but I'll mention a little bit later in the presentation, and then the USGS surface water balance model that was mentioned by Shlomo and that I'll talk quite a bit about a little bit later in the presentation.

Now, there was a lot of difference in the amount of effort that went into these three different models. The second and third ones involved large modeling groups that have spent many, many days and years, in fact, developing these models, and a lot of effort has gone into those. The first model is essentially an individual effort, and has had a lot less effort and resource go into it. The interesting thing to me was that the results of the second and third models depend almost entirely on the output of the first model, since they use the output of the first model as the surface boundary condition for their models. So the spatial variation they use for their infiltration estimates, the amounts of infiltration are determined almost entirely by the results of that USGS model.

I think with any modeling effort of this type where it involved many, many relationships, many assumptions, it's very easy to take pot shots at it. You can say--I mean, I don't know of a model in existence that I can't shoot down in some way or other, a model of this type that involves this kind of complex relationships. But in general, I was very favorably impressed with the quality of the modeling effort in all three of these models. To my way of thinking, the significant variables had been taken into account. They had been taken into account in the proper way, and the results of the models, while not perfect, I think represent the best, or fairly close to the best that can be done at the present time.

In the second area, Shlomo has talked quite a bit about both of these, about the estimates that can be made from observation of weeps in the tunnel, and estimates that can be made looking at the unsaturated flow in the non-welded tuff. I don't need to spend much more time on that, but just to point out again that these give us historical information and they're useful for predicting what might happen in the future, only to the extent that we're willing to say that the future will be like the past.

Many different tracer techniques were used in the mountain. I've listed a number of them here. When water moves, it carries heat with it, and also carries solutes with it, and so by making measurements of the distribution of solutes or the distribution of temperature within the mountain, we can infer things about the flux of water in the mountain, and that's the basis for modeling water flow using these tracer methods.

Now, from these three groups, we obviously don't have time here to go into detail on any of these methods, and certainly not on all of them, but what I've done is to select three of them, one from each of the groups, and just talk a little bit about the modeling that was involved and some of the things that we can learn from that.

I've chosen the surface water balance model out of the three numerical models, partly because that's an area that I have quite a bit of experience with and have worked with a lot, partly because I think it's a central issue in all of the modeling that goes on on the Yucca Mountain project. Water balance is quite a bit like balancing your check book. You take into account the inputs and the losses, and your balance is the difference.

The balance in this case, the number that we want to know and can't measure directly is the percolation flux, the input is the precipitation and the losses are the evaporation directly from the soil, the transpiration from the plants and the runoff that occurs.

In order to make reliable estimates of the infiltration flux, we need reliable estimates of a number of different parameters that go into this kind of a model, the soil depth, the water holding capacity of the soil, the rooting depth, topography, infiltrability of the soil, and a number of other factors are here.

Now, if any of you have ever made measurements of any of these in the field, you will know that those are very difficult measurements to make, and to make reliably. On Yucca Mountain, the difficulties are probably at least an order of magnitude larger than they would be in normal agricultural fields and the kinds of settings that I'm more familiar with and these techniques were developed for. So the scientists who worked on this had a significant task, and we can expect pretty large uncertainties, even in the best estimates that could ever be made of these numbers for a site like the Yucca Mountain and its scales that are relevant to the Yucca Mountain project.

We also require reliable environmental data. Precipitation of course the input is extremely important, and potential evapotranspiration. To know the potential evapotranspiration, we need to know the solar radiation, temperature, vapor pressure, wind, and so on. All of these factors vary with location on the mountain, and vary over time. And so, again, the estimation of these values is going to be difficult, and even with a lot of effort, large uncertainties still will exist in the values.

Now, the kind of information that was available to make these simulations, at least that was indicated to us, are, even though a lot of effort has gone into collecting this kind of data, compared to the sorts of simulations that we might do in agricultural settings, the data looks pretty meager. We have temperature data from Beatty, Nevada and a number of other sites around Yucca Mountain that go back about 50 years. A very extensive precipitation study was done on Yucca Mountain, perhaps the most extensive ever done to determine the spatial distribution of precipitation, but that only goes back about 15 years. A soils map of Yucca Mountain was made showing the depths of soils and water holding capacities. But, again, the area is large and the terrain and soils are difficult to quantify. So even though a large effort has gone into it, large uncertainties still exist in all of these things.

Now, if we take this information and do a simulation, the result of that simulation is perhaps a little different than one might expect, and different than I would have expected I think before I tried doing this. Remember, this is a desert setting where the potential evaporation is much, much greater than the precipitation. And the thing that we tend to find is that at most locations and at most times, the infiltration or percolation is zero.

What I've shown here is results of a 50 year simulation that I did with the water balance model that's fairly similar to the one that Alan Flint used for the simulations he did for the infiltration map that he produced for Yucca Mountain. And this just is to illustrate that out of those 50 years, there were really only five years when there was any infiltration or percolation below the root zone of the plants. And this makes it so that even though there are large uncertainties in a lot of the inputs, really the controlling factors end up being soil depth, the ability of the soil to store water, and then the probability of pretty uncertain events occurring, the combinations of precipitation that will end up overfilling this storage capacity.

This is a transect across the mountain. The upper white line shows the elevation along the transect, so you can see the location of the Yucca Mountain crest, and then the red line, the lower line, is from Alan Flint's map showing the simulated infiltration. And you can see that at many locations, the infiltration is zero. Where it's zero, that generally is in the locations where there is fairly deep alluvium so that the water holding capacity of the soil is high.

The high infiltration values, you can see tend to occur around the crest, locations where you tend to get higher precipitation values, and where soil depths are relatively small. The idea here is, or the idea that I want to get across is that this process is both spatially and temporally highly variable.

Now, we were cautioned a little bit earlier about confusing uncertainty with natural variability, and I know throughout the time that we did this, we considered questions on this panel, those issues were brought up. But I have a hard time sorting that out. I have a hard time sorting out uncertainty from variability. There's a high variability that exists in both space and time here. In my mind, that results in a pretty high uncertainty about the number that I should give if somebody asks me what the flux is through the mountain.

I'll present some of these numbers in a little bit different way here. These are numbers that, again, came from that 50 year simulation that I did. You can see the mean value, the precipitation over the 50 years was 170 millimeters, but the precipitation from year to year varied from double that amount to half that amount, and with these other components the same thing.

You can see here that the evaporation component accounts for a pretty significant fraction of the precipitation, no matter what. And so if we're trying to set an upper bound on the possible infiltration or the possible percolation, we might be able to set that upper bound this way, and we probably would end up with a number not too different from the number that Shlomo came up with, the 50 millimeters or so as an upper bound.

We now go to estimates of percolation flux down in the mountain, trying to estimate what it is at present, or what it may have been in the near past. We can do a calculation, or I've done a calculation quite similar to the one that Shlomo did, and in the interest of time, the assumptions here are really the same ones that he has gone through, the gravitational, potential gradient is the main one that's acting. There's strong evidence from the measurements that are made to support that assumption, and also the idea that the flux has to be at least the value that we would estimate from this calculation because of this matrix at the non-welded tuff, and there may also be fracture flow that contributes in addition to this.

Calculation is based on Darcy's law, as Shlomo mentioned, dh/dz is zero, we're assuming. And so the flux is just equal to the hydraulic conductivity, the unsaturated conductivity. Shlomo estimated that based on the saturation. I'm estimating it based on the hydraulic conductivity, again using relationship that has been shown to work reasonably well for soils, but I think that we would want to consider highly uncertain for these rocks until we have additional data to support this relationship, or to derive some other relationship that would be more reliable.

Using the data that were supplied to us again, I came up with the saturated conductivity and the other parameters needed for that model, and from that, we can see here the fluxes that would correspond to several different values of the water potential.

Unfortunately, the measurements of the water potential that were made, both in bore holes and in the core samples that were taken, did not have sufficient resolution to tell us what the flux really is based on these calculations. The uncertainty in both of those is on the order of two bars, and that two bars covers this whole range of fluxes that we might be interested in.

And so my estimates of flux are based on, again, personal conversations with scientists where measurements have been made in the ESF, preliminary measurements of the water potential, and those measurements tend to indicate that the water potentials are in the range of .2 to .5 bars, which gives us estimates of flux of maybe 2 to 20 millimeters a year.

My strongest criticism of all of the projects that we reviewed relates to the measurements that were not made as the ESF was being constructed. This whole issue could be resolved pretty readily if we had had measurements of water potentials on the rock in the ESF as the ESF was being drilled. A lot of measurements were made, but those measurements were not made. They are being made now, but the rock has dried out sufficiently so that it's pretty hard to tell what the water potentials might have been when the tunnel was drilled. And so I think as we have additional drilling there, that a very, very high priority should be to gear up to make the water potential measurements, to work out instrumentation that will be reliable for making those measurements, and to make the measurements so that we'll know what the water potentials of the rocks are and what the spatial variability of those water potentials are.

Finally, the modeling by the tracer techniques, again Shlomo went through this, a very elegant technique, the chlorine 36 is generated by cosmic rays, has a long half-life. Modern ratio values of the 36 chlorine to the other isotopes are around five times ten to the minus 13, but around 10,000 years ago, they were about three times that high, and as a result of the recent bomb tests, they've gone very much higher than that. These are some of the isocore data showing that the high levels from the recent bomb pulse occurred from about 1952 to 1972.

A lot of sampling was done in bore holes and in the ESF, and this is one of the best things in terms of calculating water fluxes that was done on the ESF study, was to do the sampling there. Samples every 100 meters, and then also the feature-based sampling where sampling occurred near faults and fractures, and I think probably these data give us some of the most reliable estimates of what might be going on with respect to water in Yucca Mountain.

I'll show again this graph that Shlomo showed from Fabryka-Martin and others that shows that a lot of the chloride ratios are at about modern levels, that some of the ratios are high enough so that we're essentially certain that those are from bomb pulse, at least some water has reached the ESF within the last 50 years or so from the surface, and then a lot of samples in between those two values.

Now, these data by themselves can't give us--they can tell us in a qualitative way that water, the time that it takes for water to reach the ESF level, but quantitatively, we need to combine them with some other kind of model to come up with estimates of those fluxes. And the model, the results that were shown us were from this model by Wolfsberg and others using the Los Alamos solute transport model.

They used the dual permeability capability of that model so that they could have fracture as well as matrix flow, and didn't require equilibrium between the fractures and the matrix, which seems a necessity in this case, and they could implement then the fast flow paths in fault regions.

The result of that modeling effort do tend to place both upper and lower bounds on the flux that occurs in the mountain or may have occurred over the past few thousand years. And, again Shlomo went through some of the tables that relate to this. What they've plotted here is the relative concentration of chloride and the time, both on logarithmic scales for different values of the flux at the surface. When the flux is .1 or 1 millimeter a year, they get break-throughs that are at times beyond 10,000 years. If that were the case, then we wouldn't expect any of the modern water to have reached the ESF by the present time. And so that indicates that the flux has to be a larger number than that.

When they use 5 millimeters a year, then they get enough of the modern water reaching the ESF level so that that would correspond well with the observations that we see now, but would not account for any of the bomb pulse water getting there. When they include the fast flow, a very small amount of fast flow through the faults, that's when they see the bomb pulse showing up at that level.

And it's on the basis of this result that I claimed that the amount of water that flows in fast paths is a pretty small amount. In fact, there wasn't any. The only place that bomb pulse chlorine levels were observed were in association with the feature based samples, not the 100 meter samples. So they represent, while the chloride is there, it represents I think a fairly small fraction of the total water there.

Now, this simulation also provides an upper bound, in a way, for the flow through the mountain, because if the fluxes were much higher than the 5 millimeters a year, then these values that presumably represent old water, pre-Pleistocene water wouldn't be there. We'd expect to see all of those down at the modern water level. So I think this gives us both information about the upper and lower bounds of the water flow.

The conclusions that I would draw, or that I have drawn from the exposure that I've had to this Yucca Mountain project are these, and I tried to start off with the ones that I was most certain of. I think I'm essentially 100 per cent certain that downward flow occurs in Yucca Mountain. There's very little data to indicate anything to the contrary.

I have a high certainty that water reaches the repository levels within decades, and that's based on the chlorine 36 data. I don't have any other explanations other than fast flow for those observations.

It's likely that the fast flow paths are in the faults and fractures, based on our understanding of the flow physics that we have right now. We really don't have any other explanations for the fast flow.

Recharge is highly variable in space and time. I think that has been pretty amply shown, and I've put here a year in ten. That's not a very certain number, but at least I think the recharge or the infiltration occurs fairly rarely, and is the result of fairly uncertain events. And because of that, the short records that we have available to us for predicting precipitation events and even evapotranspiration make it very, very difficult to make reliable long-term predictions about the recharge.

The recharge occurs mainly I think under the shallow soils, or in areas where there's run-on, large amounts of water concentrated in small areas.

Flow has to be I think mostly in fractures, except in the non-welded tuff, because the matrix permeabilities are too low to carry the kinds of fluxes that we think occur in the mountain. I've given the range 1 to 20 millimeters a year. That range of estimates I think would be changed if you change the problem. If you said, well, is there any location on the mountain that could possibly have an infiltration or percolation greater than 20, I would certainly say that there is. I mean, I could agree with Shlomo's probability distribution if you can state the question in the right way. And my probability distributions are based on an awful lot of averaging over space and time. If we didn't do that averaging, I think that I'd have to broaden that probability distribution a lot.

Now, with any project this big, a person could generate a very, very long wish list of what they would like to see. I've listed a few of the things here that I think are fairly important. I would very much like to see accurate water potential measurements made on the rocks as the tunnel is being bored in the ESF. A lot of those measurements are being made at the present time, but I think some additional effort could be focused in that area. I think if we had that data, that would tell us an awful lot about what the water flow would be in the mountain.

That date, combined with the second item on my wish list, the same as one of Shlomo's requests, and that's for better and more reliable unsaturated hydraulic conductivity on the rocks, and especially the non-welded tuff.

And, finally, we haven't heard much mention of it, but I think a lot of effort could also be focused on the perched water data. I think there's a lot of information available there about water flow beneath the repository, and that information could be gotten at from the hydraulic data that are available on the core samples, and from the locations of the perched water bodies and using the Berkeley model to investigate that in the inverse mode, and I would suggest that more effort should go into that.

Thank you.

KNOPMAN: Thank you very much, Gaylon.

What I'd like to do now is ask Gaylon to stay up there, and if Shlomo would also maybe bring his slides up, we could get the lights up as well, open this up to questions for both of them from Board members and Staff. And I'll want to leave some time so that we do give Bob Andrews and Bo Bodvarsson a chance to summarize the lessons learned from the expert elicitation process, but I'd like to take advantage of having two of the participants here, and give us a chance to play them off.

Shlomo, do you want to come over with your overheads, just so you have them handy there? Jerry?

COHON: Cohon, Board. Actually, I have a question for Bo, but it will probably result in more discussion.

Bo, you had the two overheads that talked about future work, with the overall summary and the list. Was there any implied ordering to that list?

BODVARSSON: No, no implied ordering.

COHON: Okay. Dr. Campbell has already indicated what he felt were the highest priority things to do next, and Dr. Neuman, it would be very valuable to hear your reaction to his and your own list.

NEUMAN: Well, as I stated, I think that the highest priority is to obtain many more samples of the one piece of information which is critical for the kind of calculation that I've gone through, which almost does not exist, and that is hydraulic conductivity under ambient saturation within the PTn unit at many locations.

I believe that there's information about saturation. There is probably sufficient information about pressure heads, suctions, so that one can come up with a gradient, if not absolute values, at least a gradient, and I believe that a simple application of Darcy's Law to many sides across the PTn at many elevations should provide three dimensional information about the spatial distribution of ambient--there seems to be more or less a steady state--ambient fluxes within the PTn and across the PTn.

I want to stress a fundamental difference between the way in which Dr. Campbell has gone through the PTn flux calculation using Darcy's Law, and I have, in that he has calculated the hydraulic conductivity based on a formula, according to which you can do it if you know the pressure. I have not relied on such formula, although other formulas of that kind were in fact used in all the models for the simple reason that there are no measurements of actual hydraulic conductivity at ambient saturation. So one had to calculate indirectly, and the reason that my calculation was based on only two numbers is because I simply refused to use formula that have not yet been verified for the PTn matrix.

I think that what is needed is verification of these formula. It gains actual measurements, and then use that formula if it proves to work, or use it within the context of a probabilistic calculation if it proves not. This would give, I think, very good information about the distribution of what I consider to be the background flux in the lower bound.

As far as the upper bound is concerned, I do not believe that the chlorine data provided us is an upper bound, because the chlorine data do not yield fluxes directly. The chlorine data can be related to fluxes only either through velocity and/or porosity of the fast flow paths for the bomb pulse and the background for the background.

So I do not accept calculations for an upper bound based on chlorine, or for that matter, any other tracer type data. I do not think we have a handle on the upper bound. This is the reason why I used the one number that was brought to my attention, and that is the 50 millimeters per year that was measured within the ESF during weekends. Now, it most probably is influenced by the fact that the ESF is ventilated throughout the week. One would probably have to wait much longer than just two days of a weekend to get an ambient flux without ventilation. But I considered that to be the best way to go about obtaining a sustained flow into the open gallery, and I cannot think of any other way to obtain an upper bound.

Again, I am very skeptical that measuring psychometric measurements, for example, within the fractures in the immediate vicinity of the ESF would be extremely useful, because of our poor understanding of unsaturated flow in fractures, and we know that surrounding the ESF, the flow is controlled by fractures, except in a few places where the PTn intersects it.

So I'm not sure that that's the way to go, but I think that direct measurements could be extremely useful. So those would be my two priorities, the ESF and the PTn.

KNOPMAN: Dan Bullen?

BULLEN: Bullen, Board. We have a rare opportunity here with a couple of unsaturated zone experts, and so before I--instead of waiting until tomorrow to ask this question, I think I'm going to ask the question with respect to enhanced characterization of the repository block, which may be a little bit beyond the scope of what you want, but it does lead into what additional data might be necessary.

The Board has been on record as saying that we think that an East/West crossing of the repository block would be an important piece of information to have as soon as possible. Our purpose behind it was initially just to reduce the amount of uncertainty in the hydrogeologic environment. Specifically, my interest is wondering exactly this; how much water is going to flow into the drift in unventilated conditions, and can we measure or determine in some way water flux?

Now, as designed, I'll ask each of you do you think that the East/West crossing or the enhanced characterization of the repository block will provide significant data that could reduce the uncertainty in the unsaturated zone flow?

KNOPMAN: Gaylon, do you want to start?

CAMPBELL: Well, I'm not sure how it's designed. If it's done in the same way the ESF was, then I don't see that it would provide very much useful information. I think the potential is there for providing a lot of useful information if the right measurements are made, but that ought to be planned before the boring starts.

NEUMAN: I believe that any additional boring underground will provide useful information from the hydrogeologic standpoint, more the geo than the hydro, in terms of defining other faults, verifying predictions of where faults that have not been encountered at that level may or may not encounter the gallery, in other words, refine their ability to predict the three dimensional nature of faults that have been seen elsewhere within the system, learning more about the fracture distribution. I think it's all very useful. I think it's useful more in a qualitative than in a quantitative sense, unless, as Gaylon says, one comes up before the actual drilling with a very well thought out program which is geared toward addressing the kind of questions, and maybe other questions that are of concern here.

It is extremely difficult to characterize faults and fractures individually. We know that. And, therefore, it seems to me that, yes, it's important to know where they are. Yes, it is important to know how many of them there are, what orientations they have, and so on. Perhaps you will encounter weeps, something that I understand has not been encountered in any major way within the ESF yet.

Perhaps additional sampling of chlorine 36 data and other isotopic data might be useful. The question of whether they are entirely constrained to "features" I think is somewhat debated at the present time.

I am not convinced that they are. There is at least another member of our panel who I understand likewise is not convinced that that is the case. I understand there has been some encounter of chlorine 36, elevated chlorine 36 within the PTn matrix, and perhaps I'm mistaken, but that was my impression, and I think that that kind of sampling done systematically can be very useful, plus additional testing, air permeability and other types of testing, testing of water flow.

BULLEN: As a followup to that--this is Bullen, Board again--one of the concerns that I've raised about potential East/West crossing is that it's projected to be diagonally crossing the block from northeast to southwest at some distance above the repository horizon. And I guess the question that I would have is there a possibility that this crossing could compromise future repository performance? And if not, please explain why. And if so, would it make more sense to drill it at the repository horizon?

CAMPBELL: That's a question I don't feel qualified to address.

NEUMAN: The one way in which I could think that it possibly might compromise it is if it acted as a sink for water that would accumulate within portions of this gallery and would then form a very small perch zone. But I don't think, or at least I cannot think of how it would impact the overall permeability of the system. It might act as a sink for water, and in that sense, in terms of the flow regime rather than the flow properties, I think it might. But that's something that could be investigated.

BULLEN: Right. If it did so, would it not make more sense to put it at the repository horizon so it's not going to rain on my waste packages?

NEUMAN: Well, I think it's something to look at. It's hard for me, based on the information that I have, to recommend placing it anywhere, here or there. You know, I really don't know enough about it. But it's something that can be--this is the kind of thing that the model of the kind of Bo's should be able to help, if not in detail, at least in principle.

BULLEN: Then you've deflected to Bo. Are these kinds of calculations in the works, I guess is the way to put it?

BODVARSSON: Yes. Well, the calculations to predict what happens in the East/West drift are in the works, not the ones that say it's going to be detrimental to have the drifts 20 meters above the repository horizon. They are not in the works as far as I'm concerned.

A couple of comments, if I may, on their comments, and I happen to disagree with Shlomo on his evaluation of the East/West drift. I think the key aspects of the East/West drift are hydrological rather geological, because the repository is going to reside in three geological units, the middle non-lithophysal, the lower lithophysal and lower non-lithophysal, and it's very critical to get at the issue of seepage into drifts in these three units, because that's a key issue to PA and to design. And the East/West drifts provide us with an opportunity to do that and do studies in those three units with regard to seepage into the drifts and percolation flux. It's very important.

Secondly, I do not share your view that it's very detrimental to be above the repository. I used to. But when I think of the variability in the different units, you can argue that there is going to be a lateral variability in all of the units and a vertical variability in all of the units, and none of us know to pinpoint laterally where it's going to go, so why should we worry about vertically exactly where it's going to go.

NEUMAN: If I can make just one comment? You cannot disagree with me, Bo, because I don't have--for the simple reason that I have never been presented with the rationale or the plan for this tunnel. So I'm really addressing everything just with the information that--

BODVARSSON: No, I agree, I couldn't possibly disagree with you.

BULLEN: I would be very interested in hearing, as a member of the Board, your information that caused you to be converted, I guess is the way to put it. I have concerns about the fact that if you put a transport pathway above the repository and we're worried about percolation from the top down, that it would make more sense to go right into the repository horizon so that if I do find some area that would be flawed or some area that's going to be a fast transport pathway, and I make a mistake, then I'm only causing a problem below the repository, and I don't put any waste packages in that drift.

BODVARSSON: I understand your point. My point would be the following. The Topopah Springs is heavily fractured, with 40 fractures per cubic meter, a fracture every half a meter. Permeabilities everywhere are high, 5 darcies, 1 darcy, 10 darcies, with variability on the order of high meters. When you get past 5 meters, you don't have variability any more. You see signals over kilometers in terms of pneumatics.

The tunnel above it is an opening that, if anything, will create a capillary barrier to flow and maybe make the percolation flux go around it rather than accumulate in it, unless you do something drastically to make it accumulate, and I don't think there are plans for that. A small tunnel on the order of six meters going over the repository region, which is very large, with the possibility of diverting the very small amount of water we expect to go through the mountain, I don't think will focus flow at all in terms of local variability. So that is my belief.

BULLEN: The only other followup question that I'd have then is how would it affect the thermal pulse if I'm moving large volumes of water around and I've got essentially somewhere above the repository, the potential to divert that water in some means?

BODVARSSON: That's a very good point. First of all, the thermal pulse, you have vaporization, vapor goes up and then preferentially goes along the tunnel, that might cause substantial differences in the thermal response. I agree with you there.

BULLEN: I just want to make sure that these kinds of questions are asked before the decision is made as to what you're going to do.

BODVARSSON: Absolutely. I hadn't thought about that. That's a very good point.

KNOPMAN: Priscilla?

NELSON: I'd like to ask you a question about--well, first of all, I think that this has been a wonderful discussion and I think everybody has raised wonderful questions, and I hope that we can get some of them answered at some point in the future.

Are there any plans within DOE to answer any of the open questions that have resulted from the elicitation?

BODVARSSON: Well, our next talk, Bob and I are going to give you a feedback on how we have responded so far and what the plans are.

NELSON: Good. Thanks. Then I'll wait for that one.

But most of the focus here has been on vertical flow coming through from the top of the mountain. What do you think about the Solitario Canyon side of the block in terms of being an entry point that doesn't come in from the ground surface over the mountain top?

CAMPBELL: I don't think it's a significant source.

NEUMAN: I would not be surprised if it was a partial source for the perched water which is being found across the side. I don't think that the perched water necessarily comes only from Solitario Canyon, but I would suspect that it could definitely come in part from there.

NELSON: And if so, would that make a significant impact on your understanding of how water flow occurs through the mountain in the vicinity of the repository?

NEUMAN: Certainly below the repository, because I understand that the perched water zone is below the repository.

By the way, one reason why the panel has not said very much about the perched zone is because we were asked specifically to make predictions regarding the repository horizon. And so issues concerning the possibility of--we did discuss the perched zone, many aspects of the perched zone, but we have really not been asked to do any calculations, or for that matter, to examine in detail any calculations that relate to how the perched zone might travel through the underlying layer.

KNOPMAN: Paul?

CRAIG: Craig, Board. While you both put in numerous provisos about looking at the actual numbers, nevertheless, the numbers are what many folks are going to focus on, and you're both here because you are at the extremes of the distribution. So what I'd like to do is to get some understanding as to whether the differences in your distribution estimates, as showed up in the earlier graph, should be viewed as significant by us, or are these real differences, or if you get together over a beer, will you say, my gosh, our uncertainty bounds totally overlap and these are not significant differences? How should we think about the differences between you?

NEUMAN: Should I try to answer?

CRAIG: Please.

NEUMAN: I think that there is overall convergence of opinions that I hear, both among the panel members and others, about flux value. Anywhere from 5 to 10 millimeters is the number which is currently in vogue, maybe even a little bit less. Let's make it, rather, 1 to 10 millimeters.

I think that where the difference of opinion lies is in how high might it be and what is the distribution, that it would be one thing or another. The way I arrived at my distribution is very simple. I calculated a flux of 6 millimeters per year which flows smack in the middle of the accepted range today. The difference is that I took it to be a lower bound, plus I recognized that it could be one or ten very easy, and the only reason it happens to be five or six is because those two particular values of hydraulic conductivity that I used maybe by quirk gave me the number which everybody likes at the present time. But I take it to be a lower bound.

Then comes the question what is the upper bound? I have not yet been convinced by any calculations of upper bounds, other than the 50 millimeters per year that I have taken from the moisture flux calculations into the ESF. So the thing is very simple. If you take the 5 or 6 millimeters per year to be the 5 percentile distribution, the 50 to be the 95 percentile, allowing 5 per cent above, 5 per cent below, if you consider the flow is essentially cavity dominated, therefore the distribution of the flow is very closely related to that of hydraulic conductivity, which we generally accept to be log normal, I therefore propose a flux distribution which is log normal with those two ends fixed. The rest comes out.

KNOPMAN: Alberto? I'm sorry.

CAMPBELL: I think this relates to an earlier question I think that you asked about if things were revised again, would our estimates of flux go up again. I think we had at least one speaker who addressed the issue of the history of these numbers, from the standpoint I think some of the panel members had the same idea that the numbers were going up and up and up as more information came in. Actually, whoever it was that looked at this, and I don't remember who it was, went back to some of the earliest estimates that were made, and in fact those were very much in the same ballpark as the ones right now, and so those very low numbers that we heard several years ago may have been more wishful thinking than the result of actual calculations or data.

I think you asked whether if new information were made available, if that would surprise, I mean, if the number came out to be 50, if that would surprise us, or if it were 100, would that surprise us.

I was pretty surprised by the fact that there were maybe eight different methods that were presented to us for estimating these fluxes that I thought had some credibility, and I was amazed at how well those different methods agreed with each other. You probably should be surprised at how well the estimates that Shlomo has made today agree with the ones that I've made.

But it's surprising that in an area where the uncertainties should be so large, the estimates come out to be pretty much the same values, and these estimates come from pretty independent methods, so it's hard for me to believe that it's more wishful thinking.

I felt like there were a number of ways of putting an upper bound on these. At least for the present climate, we can put an upper bound on just based on the precipitation, that we can't have numbers above the precipitation except in very local places where there might be run-on. So if we average over the repository area that the upper bound has to be the precipitation less the evaporation, that kind of number turns out to be not too different from the upper bound that Shlomo came up with based on the calculations from evaporation in the ESF, to my way of thinking.

There's some other ways that we could put an upper bound on. If somebody came along and said the actual number is 100, I would tend to be very, very skeptical of that kind of number. I just don't see how that's possible in a desert situation.

If somebody came along and said, well, I've got a location here where it's 50, I might be willing to believe that number. But averaged over the total repository area, I can't see any way that the numbers can get much higher than the 10 to 20 millimeters a year that we seem to all, or the 1 to 20, let's say, that we seem to all kind of converge on.

If the climate change, I mean, we look back at climates that have existed there, it turns out that we're right on the edge of a point where the infiltration can go up pretty rapidly if the amount of precipitation goes up. And so if you went to a Pleistocene type climate, you could say that the fluxes would be at least twice what they are at the present time, and I'd certainly believe that.

So I don't know if that maybe is talking all around your question, or if I've hit--

NEUMAN: Can I enter this?

KNOPMAN: Sure.

NEUMAN: Okay. My perception is that the numbers to which we are all converging represent the average flux through the bulk of the rock, and this is why we are converging, because those numbers may be captured by temperature data, provided that the calculations are based on good measurements of temperature, flux and so on. They are captured by the kind of Darcy's Law applications that the two of us have done.

I am a little bit more skeptical about surface based calculations. Locally, I think that it can be very reliable. On the mountain scale, I am quite skeptical for reasons that are in your notes. But I think we are converging on that.

Where we are not converging I think is on the question of how much higher could it be. Now, if you ask me intuitively what would I think the range would be, I would be fully in agreement with Gaylon, 1 to 20 speaks to my intuition. Given a rainfall which is on the average of 170 millimeters per year, clearly I don't expect the recharge to be more than 20 or 30 per cent of that. But then I'm not willing to go by my intuition alone, because I find the processes to be too complex and, therefore, sometimes counter-intuitive. And so this is my conflict here. I don't believe that we have reached consensus on the upper bound. I believe we are focusing in on the background for it.

KNOPMAN: Alberto?

SAGγIS: Yes, this is for Mr. Neuman. I look at your estimation of the lower bound in here using the value of K. Now, do I understand correctly that this analysis could have been made also with samples extracted from cores done way before the ESF was constructed?

NEUMAN: This has nothing to do with the ESF.

SAGγIS: Nothing to do with the ESF. So why wasn't this estimation made then, say, ten years ago?

NEUMAN: You would have to ask Lorrie, if she's here, I don't know, when the samples were taken out. But I don't believe that the permeability of the PTn would be affected by the construction of the ESF. Is that what you're suggesting?

SAGγIS: No, I mean not the construction phase, but the fact that the ESF permitted additional samples to be extracted.

NEUMAN: Yeah, but I'm talking about samples from bore holes drilled from the surface way above, or taken way above the ESF. So I'm not sure I see the connection between the ESF and those samples.

SAGγIS: Right. Right. In other words, this analysis could have been made way before the ESF was ever drilled.

NEUMAN: Oh, absolutely, and should have been made.

SAGγIS: And then why are we seeing it as a revelation now that the expected flux would be, say, 6 millimeters per year? Why wasn't this concluded many years ago?

NEUMAN: I don't know.

SAGγIS: I guess that's my question.

NEUMAN: But I can guess, and the answer to that would be that the concepts that people had about flows through the mountain have changed over time. I think early in the game, there was a belief that the matrix and faults controlled flow through the system. I must say that at that time, perhaps this calculation should already have been done based on the matrix, but I think the big question was, well, even if we learn about the matrix, what about those faults, they are going to control everything.

Things have changed and thinking has changed I think over the years. First of all, it has been found that the faults are not saturated with water, something that perhaps was obvious from the beginning, but they essentially conduct air much more than they conduct water.

It has also been found that within the welded units, the flow is fracture dominated, and so the focus of the analysis now centered on fractures. And I think the PTn was forgotten to some extent in the process.

To me, it's very surprising that none of the models has based itself on the kind of calculation that I have pointed out here, which is the simplest one that a hydrologist would do, and I think the reason for that was building as much complexity, build as much complexity into your model as you can because it's fracture dominated, and that has kind of overshadowed this very simple notion that there is a layer up there which might be controlled, but I'm speculating. I really don't know what the thought process was. I was no part of the Yucca Mountain project.

See, this is the beauty of coming into the project from the outside, as well as the curse. I don't really know.

KNOPMAN: Okay. Bo, why don't you respond to this, and then I'd like you and Bob to move on.

BODVARSSON: Just a couple of comments on the unsaturated hydraulic conductivity. You know, this is done with a centrifugal machine that was just obtained like a couple of years ago, or one year ago, and it has had difficulty running. There were some problems with the machinery, I think, or something like that. So the Flints have been trying to get samples through it, but haven't been successful. The project has been funding this effort for quite a few years now, so that's with respect to that.

With the second part, why we are not doing the same calculation you are doing, Shlomo, I think the following is the reason. In every single model, including the LANL model and Berkeley model, Sandia model and USGS models, we do the same calculations that you are doing. They're included in all these models, because we have all the layers in the models. We have flow through those layers, and they require some percolation flux to match saturations which are 50 per cent.

Now, there is one measurement or two measurements of a hydraulic conductivity, one of seven members of the PTn unit, so this is one of those seven thicknesses which is highly heterogeneous and all of that, and to base a percolation flux calculation on that only is, in my view, somewhat questionable.

NEUMAN: I am having a major problem with that. I don't know if you want us to continue this debate.

KNOPMAN: Well, go on.

NEUMAN: A major problem with that, and that is the model, your model, the UZ model, is driven by Alan Flint's surface infiltration map. True, you have tried to calibrate it, but you found that your calibration is quite insensitive to values of saturation and pressure, and the pressure head values are questionable at points. That's point number one.

Point number two, and I made this in my overhead, is that my calculation is extremely uncertain because it is based on two samples. So is your model based on those two samples, in fact, not even on those. Therefore, this problem permeates the entire modeling process. Either you believe the driving infiltration map, which I'm having difficulties with, and then do additional calibrations to update the permeabilities so that you get fits to whatever you can fit. There's a problem of fitting to the chlorine data because you would have to come up with additional parameters which have not been measured, such as porosity, interaction between matrix and fractures, and there's a lot of unknown parameters.

So if I am having a problem, we all are having the same problem, except that the difference is I am having more difficulty than the UZ project seems to have in accepting the surface maps as being the driving factor in the models. That's where I think we differ. Otherwise, the data lack that I am suffering from, we all suffer from.

KNOPMAN: On that note--

BODVARSSON: I agree with you.

KNOPMAN: Bo and Bob, I apologize for the lateness of the hour here, but I thought it's important that we let the discussion proceed. But we have a very short amount of time now, so if you will compress your presentation, it's five after 5:00. I'd like to get this closed out in about ten minutes, if that's possible.

BODVARSSON: Right. Bo Bodvarsson, Lawrence Berkeley Lab. I have one viewgraph, and in this one viewgraph, I'm going to tell you how we have looked at and responded to some of the expert elicitation requests. And remember they were listed on several viewgraphs, the UZ modeling suggestions and others, and this is the recommendation by the expert panel, and this is the action that we have made.

We start here with the first one, develop a surface hydrology model for Tuff 2, and that's what we did actually during the expert elicitation. We have tested it on a 2-D cross-section in Wren Wash. And then we have an evaluation plan, FY-98 planning, to look at the three-dimensional basin, look at all of Alan Flint's--collaboration with Alan and his team, and try to match some new and old data and everything to see what kind of infiltration rates we get at the surface.

The second one, Dual-K model is needed above PTn. ECM model is adequate below that. We have Dual-K throughout the entire unsaturated zone, and we have a bunch of sensitivity studies on these issues. This is in our FY-97 milestone that this came out.

The fast paths need to be modeled and more faults added and the sensitivity evaluated. In FY-97 model, we match all of the bomb pulse chloride 36 data, and we have added some more faults, according to the geological framework model.

Transient component of flow needs to be modeled. We have performed extensive sensitivity studies of transient flow in the FY-97 model that you will get a copy of very, very quickly. We conclude that transient pulses do not go below PTn except close to pulse, basically.

Investigate alternative models to the continuum model, the weeps model, or something like that. We have proposed a new activity for FY-98 planning to look at alternative models.

Model must balance the perched water water table fluctuation. Perched water balance is included in the FY-97 report. We had it actually in the FY-96 report. It helps us with the percolation flux issue in terms of ages and chlorides and all of that.

The water table fluctuation, this is one exception. We don't believe that the water table fluctuations are due to anything in the unsaturated zone. It's more a bounding condition, because the flux through the unsaturated zone is so low, it didn't affect it. So we didn't want to address this too much.

Predictability of fracture flow through the model. Current fracture flow is modeled using the Dual-K continuum with all or some random fracture flowing. So this follows this suggestion.

Now, this relates back to Shlomo's, perform uncertainty and error analysis of heat flux and temperature data. We developed an analytical model for the FY-97 model that actually does evaluate the temperature data and allows for uncertainty and error analysis. That model gives us the percolation flux, you know, which we were in the planning of doing, and gives estimates on the order of 1 to 10 millimeters per year, which is consistent with the current estimates.

Finally, but perhaps most importantly, Shlomo is talking about the upper boundary of the flux, and this is Jim Mercer's suggestion and it's a very good one, don't always match data. Don't always match data. Do the "what if" calculations. And in our FY-97 model, we did a bunch of "what if" calculations, and they suggest very strongly that the data becomes, or the model becomes very inconsistent with the data if the percolation flux is higher than 15 to 20 millimeters per year. The temperature starts to break down totally. The total chloride contents in the perched water in the Calico Hills, in the PTn, you can't get the total chloride contents in the PTn, the strontium values, everything starts to break down very heavily when a percolation flux becomes higher than 20 millimeters per year.

So I believe, based on our results and our models, we firmly believe that is the absolute--or not absolute--and I'm looking at Kevin, Kevin is all into these uncertainties, so I'm trying to word it correctly. There's a very low probability that the flux is higher than, say, 15 to 20 millimeters per year on the average under current climatic conditions today.

KNOPMAN: Thank you.

BODVARSSON: So that's my viewgraph. Bob? I took three minutes. You have seven.

KNOPMAN: Five minutes.

ANDREWS: Well, we asked for it, we got it; right? So now what are we going to do with it? Let me go quickly. I have some example slides at the back, but first off, to re-point out something Bo presented to you earlier, the UZ flow model itself is feeding four other important models. The actual stuff, the flux, the drift scale, it feeds into the thermohydrologic regime because it's directly impacted by the UZ flow model and our confidence in that. It affects the mountain scale thermohydrologic response, and ultimately would affect UZ transport. So this flow model we're talking about has four important customers that are used within performance assessment to evaluate total system performance. I just wanted to point that out.

What are we going to use? Are we going to use this PDF? Yes, of course we're going to use this PDF. Let me talk about how we're going to use that PDF. You know, the experts did two very important things for us I think. One is they evaluated uncertainty. They did quantify to the best of their ability with the available information as of February when they were elicited, the uncertainty in an average of percolation, and five out of the seven felt they could do it for an average of net infiltration. The other two said net infiltration was more or less the same as average percolation. So they did quantify uncertainty. They met that objective that Kevin laid out for you.

They also did another very important thing. They did to the best of their ability evaluate the variability of that average, that variability in space and that variability in time. And now we want to try to bring both of those two aspects, both the uncertainty characterization, appropriately weighed by the probabilities that they elicited, and the variability that they determined. So let's walk through that.

First, in the net average infiltration rate, there was an uncertainty ascribed to that. That uncertainty was presented by Bo as a PDF. I'm going to come back as an example, and maybe I should do that now, as an example, I want to be clear here, I put it at the back because I thought, well, maybe we aren't going to have time for this, but it's clear that some of the questions relate to it. So, effectively, we have--rather than the whole PDF, we'll try to capture it by the appropriate weights that were elicited. You asked why I want you to sample off of that PDF? Well, that's a pretty easy one and that is because I have four component models. Each of those component models has embedded in it an incredible amount of complexity as well. So I don't want to have complexity on complexity here, so I'm going to appropriately weight my PDF of, in this case, percolation, but because, you know, five out of the seven said percolation equals infiltration, and in fact the other two said it, too, it ends up being the same as infiltration.

What we have is the percentile on the actual CDF, if you will, and then the weight, which is more or less what's going in the PDF. So you see that there we always focus on the 50th percentile, if you will, or the median of that distribution, being around seven, which is well within the range of Bo's current model which said between five and ten as the best estimate. But there's uncertainty in that best estimate, and that's what we're trying to capture. So we're trying to capture that uncertainty.

As an example, we have a temporal and spatial variability in that infiltration, as was elicited, a very key aspect. What are we going to do? Well, we're going to use alternative temporal and spatial distributions that were determined, and propagate those through, because we don't really care about infiltration variability. We care about percolation variability. We're going to propagate those two through down to the repository horizon and get the temporal and spatial distribution of percolation, which the experts as elicited thought would be a very dampened version of the spatial distribution and temporal distribution at the surface. We expect that to be the case also. Modeling results that were presented to the experts showed that, some from Bo, some from the LANL folks.

Let's go on to seepage, although I have a slide in here on spatial variability of percolation flux. Let's talk about that. Well, no, let's go right to seepage.

For the seepage, we have a distribution in space that came from the experts that was propagated down to the repository horizon of the net average percolation. We then asked them to the best of their ability, and of course that was based a lot on ESF observations and a lot on inference and a lot on their understanding from other systems in unsaturated media, to the best of their ability, they came up with more or less this range. Shlomo had an actual value in one case that was a little lower than this .1 per cent, but a percentage of total area where they expected they might see seepage. So there was an expectation and there was a might in there, but there's a range.

What we would intend to do is use actually the spatial distribution of percolation flux, and a slight redistribution of that, which might be more in some areas, higher than the average, and the average now is uncertain, we've already talked about that, higher in some areas, lower in other areas, use that in conjunction with a drift scale sort of model, which evaluate the percentage of percolation which would seep, and see what kind of a range we get, and that's what we would intend to do. That's coming from them. They had the range of something less than .1 per cent to something around 10 per cent of area, and we would try to accommodate that range.

We presumed that that range reflects their median distribution on percolation. Therefore, the last slide, in areas where there is higher percolation spatially, one would expect a higher percentage of that to seep. In areas where there's lower percolation, one would expect a lower percentage to seep. In times when you have the higher percolation from the PDF, you would expect a higher percentage of that to seep. So in that 10 per cent of realizations that have 30-something millimeters per year average percolation flux, you would expect a much larger percentage of the area to actually seep. So that .1 to 10 per cent reflects the median.

We did not ask the experts to correlate percolation to seepage. We only asked them seepage and seepage percentage, so that's the numbers we got. So, with that, I'll stop.

KNOPMAN: Thank you. I'm very sorry we had to cut that off.

Does anyone have any pressing questions? Otherwise, I think I'd like to thank all of our panelists for a superb job, doing a lot of thinking on your feet here. We have one question?

PARIZEK: Parizek, Board. Of the percentages of, say, .1 to 10 per cent, could we see what portion of that might be faults with washes with thin alluvium versus just faults, in trying to figure out the footprint area that could have these fast paths?

ANDREWS: that .1 to 10 per cent was more at the repository horizon level, so there was no correlation between that and surficial expression, if you will. There's also, I don't think, any explicit correlation of that to faults per se. I think it was just an expectation range of possible seepage. There might be local heterogeneities in the matrix properties, there might be heterogeneities in the fracture properties. It might even in fact be fault locations, but I don't think they made a correlation there.

KNOPMAN: Thank you.

COHON: My thanks to all of the speakers for an excellent afternoon, and my thanks also to Debra Knopman for chairing the session and doing such a fine job.

We come now to a point of our meeting which is very important to the Board, and that's the opportunity for members of the public, people representing other organizations not necessarily represented in the program, to comment.

Now, we recognize we're late, and I apologize for that, especially to those who have hung in there in order to have a chance to speak. And, in addition, we have a very long list of people who had signed up.

Now, there is another public comment period tomorrow at noon at the conclusion of our meeting. Is there anybody who is signed up who would like to volunteer to defer their comments to tomorrow?

Thank you. I know who you are. Thanks. I feel like an oversold airplane, offer you, you know, $300 in travel credits. Well, what you can have is a lifetime privilege, right to speak before our Board.

Thank you very much for your cooperation.

Now, one more scheduling issue. Because the hour is late, is there anybody who is signed up who still intends to speak today who has a time problem, as in they've got to get somewhere? That's a good reason. Of course your plane might be oversold and--let me then ask you to step forward and go right ahead. You're on. Please identify yourself.

BUDNITZ: My name is Robert Budnitz, and I don't represent anybody but myself for this comment. I'm very concerned that the Board needs to keep asking the following question. Whenever an option is placed before the project to make a change in the repository design or something, they have to keep pressing as to whether that design change makes it analyzable or less analyzable.

You can't build a repository that isn't analyzable to the satisfaction of everybody knows who, and I'm concerned that analyzability must be a design criterion, not just a desirable, but a criterion for everything that is being designed. And I know that that is the case for much of this, but unless everybody who's, like you, from the outside pressing them about this, keeps pressing them about it, it's liable to be forgotten somewhere along the line, somewhere up the line, and that would be a damn shame. Analyzability needs to be a design criterion.

COHON: Is there something that's come up recently that you would consider to be not analyzable? Do you have a specific example?

BUDNITZ: Does anybody in the room think that as it stands, it's analyzable? But that's not a change. Thank you very much.

COHON: Assuming that no one else has a scheduling problem, we'll simply proceed now in order of those still on the list for today. Okay, Sally Devlin?

DEVLIN: My name is Sally Devlin, and I'm from Pahrump, Nye County, Nevada, and I really owe you an apology because when you all came to Pahrump for your last NWTRB meeting in Nevada, I had completely lost my voice so I couldn't say goodbye. So thank you for coming again, and I want to welcome all the new Board members, and I hope it won't take another four years for you to come down to Pahrump, because we have beautiful weather and everything else for you. And so welcome again to Nevada.

The reason that I'm here is every time I've come to these meetings over the last four years, we've had, today for example, 16 percenters. Now, there's some of this stuff from Bo and so on, that I would like to find out more, and yet I have no way of communicating with them, because all you put is who they are and what organization that they're with.

Now, I don't know names and addresses, and I'm just a little stakeholder and I'm always looking for 800 numbers and I'm really tired of calling DOE and saying who is this and who is that and how can I get ahold of them, and then they give me some number up at Hanford or Idaho or something. My bills are too high and I am a (speaks Spanish) that's for my friend there, anyway, and I'm just saying that you want the public to participate and yet you don't give us the information that we really need, and that is my suggestion.

Also at the Pahrump meeting, and this is another suggestion, whenever something came up, the public was allowed to speak. Now, looking at this agenda, you're adjourning tomorrow at 1:30. Had on each of these things the public been able to ask questions also, this might have gone to 5:30 tomorrow, and I really feel that there's a lot of information that you people who work together and don't communicate with other groups, and I'm not going to yell at all the acronyms as I normally do, but I am saying that I really feel this is a type of communication that this group needs, and there are a lot of people here who are very knowledgeable from the public.

I brought with me, for example, a map from the test site. This was presented to the CAB group, also to my hydrology group from the University of Nevada, and when they were asked where did you get this flow map of the water from the test site, and of course you know how I yell at you about being your own little island, they said it came from classified material.

Now, there's a lot of stuff that should not be classified, and you should be aware of this. This is on the tritium. I asked about the water that was used at Livermore. Maybe that came from the test site, but I remember John throwing out a whole hydrology thing because it was done in the lab. Where did the water come for the testing of the metals? Did it come from the mountain? Was it full of chlorine? Was it full of tritium, whatever?

So these are questions, and I think we could have better communication with us stakeholders. Let us know who you wonderful presenters are, and you've all become good friends, but how do we communicate with you? So you can certainly help us with that.

And thank you and welcome again to Nevada. Come back very soon. Let me know where we can get ahold of you. In the Congressional book, your numbers are not there, so we can't get ahold of you so I can yell at you.

COHON: Thank you, Ms. Devlin. Let me just--Ms. Devlin, let me just point out that from my experience here, the most effective way of communicating with people is during the coffee break and also during the meeting out in the hallway.

So one should not be shy, and I know you're not, in approaching presenters during the breaks. Thank you.

Willis Clark?

(No response.)

COHON: That's the problem with being local, you've got local--well, maybe not, maybe he had to leave. Maybe he'll come back tomorrow.

Judy, I'm going to mispronounce your name again, Judy--

TREICHEL: Treichel.

COHON: Treichel. Sorry.

TREICHEL: Judy Treichel, Nevada Nuclear Waste Task force.

I think there has been a classic example here this afternoon, and I'm sure that there will be some more tomorrow, of the need to make the schedule less a priority. There was a lot of good discussion here about stuff that's not known yet, and that's one of the things that I want to reiterate again about the viability assessment. It's becoming more important in the terms of what it is rather than what it's going to say. And the centerpiece of this viability assessment is going to be TSPA, and all of the previous TSPAs were sort of where are we now kinds of documents. They were intended to sort of explain what you knew and what you needed to find out, and I was delighted to find out that some of the expert elicitation, the most important thing that came out was what you still need to find out, rather than just taking what you had and coming up with a judgment that would be plugged in some place.

There's serious doubts, fear and loathing that we here in Las Vegas have as far as the viability assessment, and you hear it pop up in very subtle ways all the time. There were several things that Lake Barrett said earlier this morning that I thought were interesting. He said that Congress endorsed the program plan in the 1997 Appropriations Act. Well, the implication there is that if Congress funds the Yucca Mountain project after the receipt of the viability assessment, then Congress would be, therefore, endorsing whatever was there and whoever defined it in whatever way that they did.

And the viability assessment also links with suitability when it says in the pending legislation that the President can, upon consideration of the VA, determine that the site is not suitable. So it appears that if the Administration does not make that unsuitability determination then, that they would be considering the site suitable.

Also when Lake spoke earlier, he mentioned the end of the Cold War. He mentioned the need to dispose of weapons materials, that the international community on nuclear waste was looking to be guided by U. S. actions, and I'm sure that there is a whole long list of imperatives, why it would be nice to have a repository or nice to have a solution to the nuclear waste problem, but there aren't any of those imperatives that make Yucca Mountain either better or worse.

So when you're considering Yucca Mountain and what it can do, what it's can't do, whether or not it's suitable, it doesn't make any difference who needs it, how bad they think they need it, or how many people feel that it's needed. It just has to be judged on the basis of its suitability and whether or not it can do the job.

We're continually told that we have to do the right thing for this generation and for all of the generations to come, and I certainly agree with that statement. But what I feel is the worst thing that we can do is that if we build a repository that will not safely isolate waste for all of those long periods of time and then having taken an irreversible step, left people far into the future with something that they can't correct. And I feel that there's going to be way too much weight given to the viability assessment, and also not just TSPA, but as far as the cost estimates, and that the perfect example on over-emphasis on cost and on schedule is the fact that the question was asked earlier today when Lake was up there talking about whether or not there would be a full public review of the viability assessment, and he said no, that if you went out and you did the kind of thing you did with the site characterization process, which probably now is less important than the VA, that you'd wind up adding a year. And I think it would be very difficult to explain to somebody in the future if you made a terrible error, that it was because you couldn't wait a year, or you couldn't put another line item on the budget.

So where we talk about saving a little time here or saving a little time there, or tightening up a schedule here and there, this is a tremendously important step, an unprecedented step, and I don't think those can be used as rationale at all, and I would certainly hope that the Board would not buy into the philosophy that we have to do something. That's what I hear all the time in front of audiences before they really start to talk about it. There's always this feeling, well, we've got to do something, and there's always the response from DOE that, well, we'll do the best we can.

But if you've got an experiment here and if it can't afford to fail, which this one can't, then you really can't do it. You can't throw it up for grabs, you can't try, you can't do the best you can, because you're going to be harming people that you don't even know yet, and I would think that the problems we're discussing today would seem very small in comparison to people who are left with a bad site out there after we've gone ahead and just done something.

Thank you.

COHON: Thank you. Gary Vesperman?

VESPERMAN: I'm Gary Vesperman representing Fusion Information Center, Incorporated in Salt Lake City.

The Yucca Mountain nuclear waste repository is now technically obsolete because radioactive waste can be ameliorated by using a brand new technology called plasma injected transmutation. At this time, tests of plasma injected transmutation are achieving 50 per cent reduction of radioactivity in less than one hour of process. More improvements are expected to be forthcoming.

To save billions of dollars of federal tax money, the Yucca Mountain project can and should be terminated as soon as possible. I'm submitting for the record my written comments. If time allows, and at your request, tomorrow noon, I can explain in a non-technical manner the basic principle of plasma injected transmutation.

COHON: Thank you, Mr. Vesperman. If you'll give it to Ms. Einerson there? Thank you very much.

Is David Stahl still here, and if he still wants to say some more, we'll be happy to hear from you.

STAHL: Thank you, Mr. Chairman. I have a technical question, and the reason I wanted to ask it today is I wasn't sure that Shlomo or some of the others that might be able to respond would be here tomorrow.

As a result of the waste package degradation, expert elicitation that we had last week, one of the things that we were dealing with was the potential for steam sparging of the atmosphere in the first few hundred to a thousand years. And the reason I would direct it to Shlomo is he talked about pneumatic pathways.

So the question comes down to this. What would you anticipate the oxygen partial pressure to be, or the CO2 partial pressure to be during this period of time when steam is being evolved as the repository heats and then begins to cool?

NEUMAN: I am sorry, but I don't know.

COHON: That was Shlomo Neuman who said that.

Would anybody else like to respond to David Stahl's question?

(No response.)

COHON: No takers. Well, thank you for posing it.

It appears that I may have acted hastily, like many airlines declaring their planes to be oversold. I mean, we're still crowded, but other than one last commentor today, that's it. So I'd like to go back to you, Linda Lehman, and ask if you'd like to talk this evening, or if you would prefer to wait till tomorrow.

LEHMAN: I'll wait till tomorrow.

COHON: Okay. Our last commentor this evening is Tom McGowan. Mr. McGowan and I have been negotiating all day on the length of time he would take, and we agreed, Mr. McGowan, right, that you would aim for ten minutes, and in ten minutes I would raise my hand, and then at 15, no matter what, you'd stop. Right?

MC GOWAN: Now, we have been negotiating, was that your phraseology?

COHON: Yes, all day.

MC GOWAN: It was unilateral.

COHON: Well, it was nevertheless a negotiation.

MC GOWAN: Mr. Chairman, I beg your indulgence. May I approach the podium or the bench? Because I need something to lean on for that long.

COHON: Sure. Would you like to sit down, or would you prefer up here? Okay. This is good, because this way you're closer to me, and when that ten minutes is up--

MC GOWAN: God bless you. There's an opening incidentally at the MGM Grand.

Okay, Honorable Mr. Chairman, esteemed members of the Board, key staff and meeting attendees, my name is Tom McGowan. I'm an individual member of the human and universal public that you hear about residing in Las Vegas, Nevada. We'll set the whole tone here.

Following today's wealth of presentations, it occurs to me that neither geology, hydrology nor thermal loading, but rather geochemistry may be the principal key and crucial determinant of both the mobilization and release and the concentration of migratory transport of radionuclides to and throughout the human accessible environment. If you've heard that before, stop me.

The near infinity of alternatives potentially applicable to repository design and the EBS infers the conceivability of an inner casing comprised of seamless spandex with an outer casing of carbon 60 for ensured optimum effectiveness. And why not?

Furthermore, the gulf of this equilibrium, evident by the ambiguity among the array of expert elicitations, suggests the need for and advisability of an uncertainties based unified field, consistent with the highest damage of human spiritual intuition, pure imagination and an abiding faith in a supreme being. It appears to me that you're approaching that realm of jugernaughts.

As the neurosurgeon said just before the patient went under the anesthetic, "Now, if memory serves me," followed by, "What do you say we take his appendix out through his ear," nevertheless, having said that, I do appreciate your kind indulgence to articulate my ten to fifteen minute presentation pertinent to the issue of nuclear waste, consistent with the spirit, purpose and intent of the mandated and NAS/NRC recommended policy and process of early and full public participation as being key and crucial to the quality/integrity of the mandated commission, and particularly as indicative of the positive prospect of public acceptance of the end product of the purportedly priority imperative initiative if and as being appropriate.

I'm particularly mindful and appreciative of the eminent and prestigious context of the Board and of the entire meeting assembly in terms of their respective echelons, disciplines and extent of dedicated interest and concerns. Accordingly, I don't presume any extent of intrinsic collegiality whatsoever, but readily admit to relatively undisciplined context as an unaffiliated individual member of the lay public, a classic paragon of the conceivability that a hybrid composite of mutual understanding between the objective, logical and impersonal scientific community, and the unscientific illogical, emotional and individually subjective lay public, presents a formidable challenge, characterization and is closely akin to the subjective agenda driven, frenzied, incoherence of attempting to navigate without an egregious incident the indistinguishable bicycle lanes of Paris, Tokyo, Bombay or Beijing. And I cleaned that up.

You like this change of pace? If you don't, say so. You do? Dr. Bullen, and incidentally congratulations each and every one of you. You're doing a great job. You had a full plate today, thanks to your negotiator over here. Very little relief, really.

But I would indicate that my personal agenda, and ideally yours as an official body, commendably comprised of an enthusiastic young--offset by the ancestral wisdom of seemingly elderly but still active--we are wholly dedicated anyhow to the protection and preservation of the public health and safety and in the genuine best interests of current and future generations respectively and inclusively. And I would indicate those future generations are not aliens from a distant planet or strangers from a distant land. Guess who they are? They are our direct decendents, our progeny. They may even cause some of our most closely held hopes, aspirations and dreams to endure. God forbid they hold all of them, but they may hold some of them. That's called the persistence of human consciousness.

We're not talking about nuclear waste at all. We're really talking about humanity. In that sense of mutual dedication and regard, I would offer the caveat that essentially a member of the lay public is uniquely advantaged, is ultimately blissfully ignorant or, in context, is relatively unencumbered and unconstrained by any directive, mandatory observance of the traditional boundaries and parameters consistent with the regulatory compliance, scientific and technical bases or other officially deemed norm acceptable regiments and protocol, which ordinarily accrue to scientific, ecological and legalistic communities, invariably pursued as self and mutual--and particularly upon the encouragement used in the public who are as readily inclined to question basic assumptions, rather than to accept as irrefutably valid and reliable any and all assertions and consequences of hypothetical modeling and/or ideally, non-stochastic scientific determination and concomitant rarity.

As deemed reasonable, his unlettered and non-degreed graduate of Seaton Hall Prep., you may remember that, I'm credentialed solely as an expert layman and expert human, which means imperfect, but I am much too human to admit that. And I'm reminded that in the wake of exhaustively self-taught review, and the Board's partial assimilation of the entire 2500 year history of scientific discovery to date, which began with the firm assertion that the earth was the center of the then limited and finite universe, which in primitive myth was promptly dispelled and followed within a mere 500 years by the equally firm assertion that not the earth, but the sun was the center of the universe. By then dimensionally expanded to universally include the variably looming near-field planets, and the seemingly--thereas deemed equal distant, five stars, which copious database imminently reinforced the conceivability, and forgive me, substantially most, if not all, of scientific knowledge is essentially theory as yet remaining not disproven. I could be wrong.

Thus founded on a mutually consensual and considerate basis for the eventual development of a reasonably intelligible communication media or paradigm pursuant to convenient implementation by and between the scientific and lay public communities, while simultaneously preserving the integrity of the respectively discrete and autonomous disciplines, dialects, vernaculars, understandings uniquely pertinent to each and both. I will now immediately revert to type by offering the heartfelt and unswervable-- that (a) none of us is smarter than all of us combined. To the best of my understanding, that is confirmed by the eminent Bucky Fuller, who insists that unity is plural somehow; (b) there is no such thing as almost pregnant, almost scientific, almost responsible or an almost site suitability, licensable, deep geologic underground permanent repository for the storage and disposal of high level nuclear waste, spent nuclear fuels and fissile materials, either at Yucca Mountain, Nevada or elsewhere nationally or anywhere within the terrestrial geophysical domain, and in typical layman's context, intractable. Which serves the immortal words of Pogo, "We has met the enemy, and that is us." Further elucidated by the illustrious Dr. Seuss, who in address of the incessantly preoccupied dog trot society in government, recommended "Stop, dogs, stop, you're running the wrong way. Now turn around and run, dogs, run." I thought it was brilliant. And that neo-paradigm alternative ended happily ever after in a timely and effective manner, at least in story books of innocent children, whose minds are not yet littered and contaminated by the pressing priorities of mundane and practical exigencies and other pesky extraneous concerns.

In the real world accurate perception, readily understandable by all but the certifiably comatose among the scientific community and the lay public alike, and wherein changes in the universal constant, it's axiomatic that the terrestrial geophysical domain is naturally in a state of however temporally and/or spatially variable, dynamic flux from inception through the eventual cessation of the geologic time scale continuum, which accounts for the universal scope and tendency and incisiveness of the spectrum of physical scientific disciplines inclusively, and also perhaps to the activities of the DOE.

Consequently, it's scientifically and technically impossible to guarantee the invariably safe, secure and intrusion impervious storage and disposal of high level nuclear waste either at Yucca Mountain, Nevada, elsewhere nationally or anywhere on the planet. And I don't make those rules. That just happens to be the way it is.

I admire your persistence in attempting to do the impossible simply because you were told to. I would admire you a lot more, instead of being just responsible good soldiers, you would also take the time to look at the dual aspect of human nature and be responsible good citizens, responsible enough to stand up and say to the Congress, to the President and the people of the United States and of all mankind this can't be done. We need to find a better way. And my guess is if we can't--my guess that you can, and if you can't, I will, because you can and you can do it by tonight before you go to sleep.

There's a viable alternative to deep geologic underground permanent repository. It's quite simple really. Perhaps the greatest irony is that if DOE succeeds in their venture, and they very well may and it's just a question of what the man said with the shoe horn putting the foot into the shoe, you've just got to keep squeezing, or take an ax to it, one or the other, not the shoe, the foot. But there is a viable alternative, and I happen to prefer reduction, transelimination process, but that's not tomorrow morning, that's within the next 30, 40, 50, 100 years, but it's there, and that's what's going to be the alternative. We have to get scientific about this.

In the meantime, you're going to have 109 above ground retrievable storage facilities. You might as well get that decided right now. As I said, the ultimate irony is this. If DOE succeeds in its TSPA/VA repository design, EBS and the rest of the contrivances, amended guidelines, revised relaxed standards for EPA and US NRC, they have in that case proven beyond a shadow of a doubt that there is no compelling immediate or continuing need for a deep geologic repository either at Yucca Mountain or anywhere else. Therefore, God speed, good luck in the conceivability that you will require an eventually welcome--I want you to prove that it works. Therefore, you don't have to do it here, really.

And I'm not Nevada centric. As I said earlier, this is not a Nevada centric issue. It's not even a national issue. It's a human and universal issue enduring for the rest of human time. Once we get that straight, we're not standing around here trying to figure out some way to defer the inevitable. A gentleman stood up here earlier and say it's not going to last forever, and he was right. So why bother? Why not just put it into the ground right now? Ted Schatz did that. They directly injected it into the human accessible environment. It will come under the commonly underlying groundwater, along with whatever comes out eventually from the repository, if there ever is one.

What is this syndrome we have of running the wrong way?

My final statement is this, and by the way, Dr.

Cohon, once again I want to thank you, and all of you fine people. You know, the Canadians have the French and we have you. So I love you, but I won't necessarily always be with you. I say simply this. Don't store it and inject it, however deferred, into the human accessible environment. Eliminate it completely and permanently from the terrestrial geophysical domain, and in so doing, take one, however primitive, however tentative and however courageous, step down from the tree and let's now move forward, and we'll cross that threshold that leads to the brilliant horizon of human achievement, challenge and opportunity that awaits throughout the third millennium. If you can't do it, I'll do it for you.

Thank you so much, sir. I think I had ten seconds left. God bless you. You're wonderful.

COHON: My thanks to all the commentors, and my thanks to all those who participated in today's meeting. We are now recessed. We'll reconvene tomorrow morning at 8 o'clock here. Thank you.

(Whereupon, the meeting was adjourned.)