UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

PANEL ON PERFORMANCE ASSESSMENT
TSPA-VA

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NWTRB BOARD MEMBERS PRESENT

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Dr. Priscilla P. Nelson
Dr. Richard R. Parizek - Session Chair
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I N D E X

P R O C E E D I N G S

8:00 a.m.

BULLEN: Grab your coffee and take a seat. I'd like to get started. We have a very busy schedule today.

My name is Daniel Bullen, and I would like to welcome you to the meeting of the U. S. Nuclear Waste Technical Review Board's Panel on Performance Assessment. I'm a member of the Board, chair of its Panel on Performance Assessment, and most importantly, chair of today and tomorrow's meeting.

It's been several years since the Board paid a formal public visit to Albuquerque, so I'd like to take a few minutes outlining what the Board is. After that, I'll make some introductions and give the background and ground rules for today's meeting.

In 1982, Congress enacted the Nuclear Waste Policy Act of 1982. That law crated the Office of Civilian Radioactive Waste Management, or OCRWM, within the U. S. Department of Energy, and charged OCRWM with developing repositories for the final disposal of the nation's spent nuclear fuel and high level reprocessing wastes. Five years later, Congress amended the 1982 law and limited OCRWM to characterizing a single site for disposal. That site is about 300 meters under the crest of Yucca Mountain on the western edge of the Nevada Test Site in the state of Nevada.

In that same 1987 amendment, Congress created the U. S. Nuclear Waste Technical Review Board as an independent federal agency to review the technical validity of OCRWM's program and to periodically furnish the Board's findings, conclusions, and recommendations to the Secretary of Energy, to Congress, and to the public.

The President appoints our Board members from a list of nominees submitted by the National Academy of Sciences. With us today are six of the current Board members, or they will be at upcoming times. I'd like to introduce them now.

Dr. Paul Craig is a professor of engineering emeritus at UC-Davis, with expertise in energy policy issues associated with global environmental change.

Priscilla Nelson, who will be joining us later, is the program director and the Director for Engineering at the National Science Foundation. Her expertise is in rock engineering and underground construction.

Richard Parizek, who was delayed by his flight with Northwest Airlines, will be joining us a little bit later. Richard is--actually, he'll be here after the break. Richard is a professor of geology and geo-environmental engineering at Penn State and specializes in hydrogeology and environmental geology.

Alberto Sagüés is professor of materials engineering within the Civil Engineering Department at the University of South Florida. His expertise lies in corrosion and materials engineering, physical metallurgy, and scientific instrumentation.

Jeffrey Wong is chief of the Human and Ecological Risk Division of the Department of Toxic Substances Control at California EPA. He is a toxicologist with expertise in risk assessment and scientific team management.

And I'm Dan Bullen. I'm a nuclear engineer and a faculty member in the Department of Mechanical Engineering at Iowa State University. I also serve as the Director of the Iowa State University Nuclear Reactor Laboratory.

Also present are members of the Board's professional staff led by Bill Barnard, our Executive Director. And I would like to especially acknowledge a couple other staff members who made this meeting possible. First of all, Leon Reiter, who coordinated the planning for this meeting. In addition, I would like to express my thanks to Linda Hiatt. Linda, would you raise your hand in the back of the room? She's responsible for putting together all of the logistic arrangements for today's meeting.

I have some additional instructions and comments about how the meeting will be run, and I'll provide those a little bit later. First, I'd like to mention a few things about the topics of our meeting, which is TSPA-VA, that is, the total system performance assessment for viability assessment.

In 1996, Congress passed the 1997 Energy and Appropriations Act, which required the Secretary of Energy to provide to the President and to Congress a viability assessment of the Yucca Mountain site no later than September 30, 1998. One of the four elements to be included in the VA is a total system performance assessment based on the design concept, another element of the VA, and scientific data and analysis available by September 30, 1998, and describing the probable behavior of the repository in the Yucca Mountain geological setting in relation to the overall system performance standards.

First of all, what is TSPA, or total system performance assessment? TSPA is the principal method of evaluating the ability of the proposed repository, engineered and natural components acting together, to contain and isolate radioactive waste from the public and the environment. It is essentially a predictive-computational method of repository performance over time.

The DOE has devoted a significant effort to achieving the goal of developing a credible TSPA for the viability assessment. Data was synthesized, process models were developed, workshops were held to bring together the data collectors, process modelers, and performance analysts, and expert panels, consisting primarily of scientists and engineers from outside the Yucca Mountain project, were convened and their views on important technical issues were elicited.

The DOE has also assembled a TSPA Peer Review Panel to provide them with an ongoing review of the developing TSPA-VA. In the past, the Board has heard progress reports addressing different technical elements, in particular, the input being provided by expert panels. One could not help but be impressed with the willingness of DOE to expose all of their assumptions to outside review.

The purpose of this meeting is to see the results of this effort, the complete, or almost complete, TSPA-VA. The Panel's primary focus will be on the technical assumptions being made, the bases for those assumptions, and the validity and clarity, or transparency, of the analyses conducted. We are interested in the results and conclusions, however, we do realize that the TSPA-VA is not meant to be the final answer, but rather a snapshot of what the current state of knowledge can tell us about repository performance.

Most of the presentations will deal with the TSPA-VA Base Case, that is, the repository's expected performance, but we will also learn something about the sensitivity tests carried out to examine the effects of "What if" scenarios for alternative input parameters, models and design features and for disruptive events such as volcanic activity, earthquakes, criticality and human intrusion.

We have asked DOE to provide us with two overviews up front, the first outlining the basic structure, conceptual model, and the results of the TSPA-VA Base Case in a form which is understandable to those in the general scientific community not directly involved in the Yucca Mountain project. The second overview will detail the important topic of uncertainty analyses. We will then hear six presentations on the basic components of TSPA-VA, starting out with how much water can get down to the repository horizon at depth, how much of this water can actually get into the emplacement drifts and the effects of heat from radioactive waste, how well the waste package stands up to corrosion, what happens when the waste package is finally breached and water contacts the waste, and what happens if, and when, the radionuclides escape the repository and enter the ground water and reach the public. Of course, as you will see, these topics have much more complicated names, but that's more to please all the PhDs involved in the analysis so they can use their own jargon. These presentations should take us until about 5:45 this evening.

Tomorrow morning, we will start off at 8:00 a.m.

again with a presentation on the topic of traceability, in other words, the ease with which the outside reviewer can track down the models and data assumed in the TSPA, and the underlying bases for those models and data. We have asked the DOE to provide us with some examples demonstrating how this is achieved. This talk will be followed by a presentation on the treatment of disruptive events in the TSPA-VA, in this case, volcanism and earthquakes, and a wrap-up by the DOE telling us, among other things, what the basic message of TSPA-VA is with regard to repository performance.

To assist the Panel in its questioning, we've asked three individuals to sit at the front table with the Board members. They are Jean Bahr. Jean, would you raise your hand, please? She's associate professor of hydrogeology at the University of Wisconsin in Madison. Steve Frishman of the Nevada Nuclear Waste Projects Office, and Chris Whipple of ICF Kaiser. Chris is also the Chair of DOE's TSPA Peer Review Panel. We will close the meeting with comments from the Nuclear Regulatory Commission and the last thoughts by Jean, Steve and Chris, and Bob Andrews, leader of the TSPA-VA team. We anticipate finishing shortly after noon tomorrow.

After each presentation, there will be time for questions and comments. I will first ask Board members, and then turn to Jean, Chris and Steve, and then to the Board staff. If time allows, we will also hear from individuals from the audience who wish to speak. I recognize that there probably won't be enough time for public questions and comments after each presentation. As a result, we have set aside two 30 minute periods following today's lunch, and one first thing tomorrow morning, devoted to audience questions and comments. We will give preference to those who have signed up with Linda Hiatt in the back. So if you're very interested in making a public comment, please sign up with Linda, and we will address those people first.

It's time to start. Our first speaker to give us an overview is going to be Abe Van Luik. Abe is the senior technical advisor to the Assistant Manager for Licensing. He's responsible for the analyses of geological disposal system performance to evaluate whether the proposed geologic repository will meet applicable operating and long-term safety standards.

Abe?

VAN LUIK: I presume that I'm correctly wired. Okay.

I'm really pleased to be here. I had my doubts about being here. I was headed for the last flight to Albuquerque at Dulles Airport last night at 5:55. At 5 o'clock, I was still crossing the Potomac at about 45 miles an hour. Two minutes to flight time, I was in my seat. TWA was very proud of me.

The technical presentations don't start until we get to Bob Andrews. But what I wanted to do is quickly review the TRB's comments regarding the need for transparency in TSPAs. The TSPA-VA is a daunting task in and of itself technically, but on top of that, we are very acutely aware of all the expectations of all the different groups out there. So what I want to do is review what the TRB has suggested should be addressed in the TSPA-VA as far as the different audiences that need to be addressed, and then of course I'd like some feedback, if possible, at this meeting, or shortly thereafter, how well in the presentations that you're going to see over the next couple of days do you think we are addressing these expectations and comments, and do you have specific suggestions for improving the process and its presentation.

There were two questions asked of the TSPA-VA in the report to the Congress in March of 1997, the 1996 report. You can find them on Page 21. That last bullet is a footnote. But the point was made that regulatory agencies have one agenda. They emphasize demonstrating compliance with a standard using specific criteria.

There's also a technical community out there that's going to look at the validity of scientific and engineering assumptions. Also, there are non-technical decision makers, believe it or not, who may be concerned about the political implications of a safety analysis. And then finally, and this is not in order of importance of course, the public could judge the analysis on the sponsoring agency's reputation for honesty and openness. And of course, you know, DOE is a stellar performer, so no problem with that one.

Question 2: Does the TSPA itself generate confidence? Does it have the ability to withstand challenges brought about by new knowledge, changing assumptions, and if it is robust, it can meet these challenges. And a final point, and I think this is one that Paul Craig is fond of making; that you enhance confidence by having understandable analyses. A black box with dials does not inspire anyone.

The word transparency is often used, and we like to use it ourselves, and we like to use it enough that everyone thinks it's transparent, even if they don't understand it. But transparency was defined as the ease of understanding the process by which a study was carried out, which assumptions are driving the results, how they were arrived at, these assumptions, and the rigor of the analyses leading to the results.

We went through a series of about ten workshops where we performed abstractions of the process level models, and the observation was made that if these abstractions are fully understood, then the observers can develop a sense of confidence that the models are reasonable approximations of reality.

Specialists, on the other hand, may require detailed knowledge of a model and it's assumptions at the process level. And then again, the non-technical decision maker or the public will want a conceptual explanation, a big picture overview, conveying what a model does, why it's important and how the results are interpreted. And, of course, all of this can be illustrated by well chosen sensitivity studies showing the effects of making different assumptions.

The proper treatment of uncertainty, and the transparent treatment of uncertainty, I may say, is important to getting positive answers to the two questions that the Board posed. We're looking at different types of model uncertainty, parameter uncertainty, statistical uncertainty, and we are doing sensitivities to show the significance of these uncertainties. In some cases, we are making conservative assumptions, and of course the kicker here is do you have defensible uncertainty distributions. If you have a very narrow band of uncertainty and people say, hey, it's wider than that, you've already lost the battle of convincing someone.

And how about establishing validity using analogues and simplified calculations? Now, I'm very adverse to the use of the "V" word. Validity is not in my vocabulary, but it is in the Board's, so I have to use it.

A model is considered valid if it provides a reasonably accurate representation of reality, I think is what the Board said. Reasonable and accurate of course are potentially contentious. I prefer to add appropriate to the problem being addressed, as an important qualifier on those words.

One of the suggestions made by the Board, and that we're taking seriously, is to perform simple calculations capturing some of the main elements of the complete natural and engineered system to allow an easier scrutiny of the assumptions used.

As mentioned just a few moments ago by Dan, we are using outside expertise, and the Board feels that this provides views not necessarily found within our own little community. It increases the program's technical credibility if we follow the advice given, of course, and the caution is always there, and we get the same thing from the NRC, it should never substitute for scientific information reasonably available.

When we look at public acceptance, likelihood of acceptance is enhanced by transparency. I don't think there's any question about that. The Board urged an increased public involvement. We are struggling with that. And then the Board also observed that there are no simple or guaranteed ways of increasing public acceptance of an analysis for a project as technically complex and controversial as building a repository. There is just no simple solution to the problem. And here also we are getting some outside help on this public acceptance question on how to couch things so that we inform and not inflame.

Now, to summarize, the Board gave us some strong recommendations. The Department wholeheartedly agrees with the intent of the Board's suggestions, and the Department is trying in the presentations you will hear in the next day and a half to show that we are addressing these suggestions as well as we can, and we would like your feedback.

Thank you.

BULLEN: Thank you, Abe.

Questions from the Board? Actually, this is Bullen, Board, I had a quick question for you, Abe.

You mentioned that you're using expert elicitation and trying to incorporate that into your transparency efforts. Have you also used the expert elicitation to decide what additional data might be necessary, and how you might get that to make the next step, which would be the suitability studies?

VAN LUIK: In several instances, we asked them specifically to give us the basis for their judgments and also to review the basis for our judgments and where we could improve that. So the answer is a qualified yes. Not in every instance, but in several instances, we took advantage of their expertise in these matters, yes.

BULLEN: Questions from the panelists? Steve Frishman?

FRISHMAN: Abe, I've got just a couple quick ones.

One is, and I asked this question once before, your bullets under question one, you recognize that non-technical decision makers may be concerned about political implications. In a previous meeting, I asked how you're going to present sort of the bottom line of the TSPA in the viability assessment, and particularly down to are you going to discuss uncertainty in like the Executive Summary, where I know a number of members of Congress who may not even read all of that, because uncertainty is a real key to it. Have you changed or thought more about that question that I asked you in the past?

VAN LUIK: Yes, and I believe that what you will see from Bob is some of the results that will appear in the Executive Summary, and we would request, since you have the opportunity, give us some feedback on what you think of that.

FRISHMAN: Okay.

VAN LUIK: But we're going to show basically the case that we think the regulator will want, which is a sort of fiftieth percentile, or mean case, and we will also show the mounds, the fifth and the 95th percentile cases, partly because of your suggestion.

FRISHMAN: Just one other. In your difficulty with the word validity or valid, you would like to qualify that meaning to, as you say, appropriate to the problem being addressed. What do you mean by "the problem being addressed"? Do you mean the mathematical problem?

VAN LUIK: No, what I mean is when you are trying to establish the validity of a forward projection of a geologic process, there is no way in God's green earth that you're going to be able to observe the result and check it against your projection. And so it's inappropriate to predicting long-term geologic processes to expect classical validity, which is that you make the prediction, you test your widget 40 different ways, and then see if you've met your prediction. That's the only thing that I have, is there is some baggage on the word validity that does not pertain and doesn't fit well with projecting things.

FRISHMAN: So what you're recognizing is that you can't validate these codes. What are you going to do in place of it?

VAN LUIK: What we are going to do is exactly what you're going to see here, which is to make transparent arguments and to look at processes and see where we can find either natural analogues or do in situ tests or observations or experiments in laboratories to support those tests. I think it's very telling that 10 CFR 60 twice calls for us to support our analyses. They don't say validate your analyses, and I think that was a very well thought out word. We will support them to the extent that we can. But to validate is a different animal altogether.

FRISHMAN: Thanks.

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

VAN LUIK: They want to get on to the technical stuff.

BULLEN: Okay. Well, actually, since we can stay on schedule and it will make me look like a good meeting chair, I'd like to move on to Bob Andrews, who's going to cut right to the chase, I assume, because we've got an hour and a half scheduled of Bob telling us sort of the meat of the matter.

Bob is the manager for Performance Assessment Operations with the M&O, and is responsible for developing and documenting the TSPA for viability assessment, and for planning the TSPA activities required for the EIS, the site recommendation report, and the license application.

Bob?

ANDREWS: Thank you, Dan.

This talk is about two themes, water and time, and I'm not going to have enough of either, but the repository's performance is dependent upon both, water and time. This was actually two talks that we've merged into one to hopefully show what's in the base case, not so much describe the bases for that, that will come up in the detailed discussions later on this morning and this afternoon, and then the results of a deterministic base case realization.

There is uncertainty in that. I think everybody recognizes that uncertainty. The uncertainty is throughout the system, and we'll discuss the uncertainty, both in Mike Wilson's talk with a range of results, and then in the individual talks that follow with discrete one offs or sensitivities, what if questions.

I wouldn't be up here if there weren't for a lot of people, and I wanted to first acknowledge them. There's a lot of graphics that are in here. We always are trying to, I think based on a lot of comments not only from the Board, but from a lot of people, trying to portray very complex processes and complex systems in as easily and understandable way, both for the scientific community, the technical community, public, et cetera. It is a daunting task, and I think we're making some strides towards that. I think we can always improve on how we display very quantitative information of very complex processes, and I wanted to acknowledge that there's a lot of graphics work that has gone on, and still to go on.

By the way, I won't go through all these slides, so you don't get too worried about the thickness of your packets. But there's four main themes. First, I want to summarize the key components that are in the TSPA and why they're the key components.

First, I think the--the second thing is the Board asked at least as a brief discussion of what is in the Reference Design that is the basis for the TSPA-VA, and then we're going to walk through one by one each of the significant processes and how they've been abstracted, and a base case, a realization that represents that process as it's being implemented in the actual calculation.

And then at the end, we're going to show a, given enough time, a fairly simple hand calculation that says although this system is quite complex, it can be boiled down to a handful of key things that explain the overall system performance.

We've struggled, as I think probably everybody has, in terms of how do you--given these definitions of traceability and transparency, how do you implement those in your documents, in your presentations, et cetera. And there's a number of methods, and maybe this isn't a complete set of ways of implementing, or ways of addressing the issue of is this transparent, is it traceable, and I've listed these. And I'm going to try to go through most of these, except for one, which is the data.

The data part within each model will be addressed in some of the more detailed discussions, and then I think it's this afternoon, or tomorrow morning, Cliff Ho will show a traceability example, going back from TSPA models, all the way back to the raw data that were used as the supporting basis for it, for one particular example. So these are potential methods, and we'll hit all of them throughout the course of this presentation.

Let me start right out by saying that the DOE in--I'm not sure when it was finally published, it's been revised several times--has several attributes of the repository safety. Those attributes are limit the amount of water that contacts waste, extend the waste package lifetime, limit the mobilization of radionuclides, and reduce the concentration of radionuclides as they go through the system.

As far as TSPA is concerned, those have individual processes involved, individual components, we call them, which really reflect individual models that are describing how the system evolves. And it can evolve from top to bottom, from the climate change at the surface, all the way back through ultimately to dose at the down side. And we're going to walk through each of these in terms of what's in the base case. Let me keep that one up there, and now try to show it a little more graphically on this slide. And water will be an important part of this presentation, like I said.

We start with climate at the surface. That climate produces participation. It's water that we're concerned about. Were there no water, there would be no problem, because water is the principal thing that dissolves waste. Water is the principal thing that transports waste, or could transport waste. That climate causes infiltration, and there's infiltration at the surface, and we'll discuss that, down through flow through the mountain, ultimately seepage into drifts such that it could contact the packages, now blown up over here. We have a number of processes going on in and around the drift and on top of the package and inside the package; thermal hydrologic processes, chemical processes, the seepage, which I mentioned, then the actual degradation of the package, the alteration of the waste forms, and ultimately the release and mobilization of nuclides and their release from the package.

And then we're back into the geosphere, if you will, with ultimately the potential for transport through the unsaturated zone, transport through the saturated zone, and ultimately uptake at the biosphere. The biosphere for our system is considered at 20 kilometers downgradient from the repository.

Let me walk through the basic elements of the repository. Design, that's in the VA. I have word slide, and then the actual, some pictures that relate to it. This is the conceptual drawing of the repository. Here is the ESF shown here. North is to the left. And we have the individual emplacement drifts. Those emplacement drifts are at about 25 meter spacing. They're five and a half meters in diameter. The average thermal load is 85 metric tons per acre in this conceptual design. That's the basis for the viability assessment.

The EBS, the engineered barrier segment for the reference design schematically looks like this. We have three different package types; one, the PWR commercial waste package type, one the defense high level waste, which is five cans with DOE owned spent fuel sitting in the middle, and then we have some BWR package types. There's some other special package types for other special wastes, but these are the three principal waste components.

We see that we have an invert in the drift. We have a pedestal made of mild steel that the packages will sit on. I'm not going to get into the emplacement stuff and the concept of operations. I think the Board has had other talks from the engineering community on those aspects.

The package itself, here the 21-PWR package shown as a representative package, consists of an outer 10 centimeters of mild steel, and an inner 2 centimeters of a highly corrosion resistant metal C-22 is in the referenced design right now. So that's our referenced design, which is used as the basis for our analyses.

This figures shown now conceptually how information flows from one process to the next. I'm going to walk through each of those processes in detail. This is only up here to show you that indeed we have a system here, and when you have a system, you have to think system and then go into the components that are in that system.

This figure is just slightly different. Those of you who are very astute maybe you've figured out that slight difference from what's in your package, and that is that this schematic of the repository horizon is flipped in your drawing. It has north to the right instead of north to the left, and in fact north is to the left on most of our drawings, not always. So we can replace the ones you have with this one so it's more graphically correct.

But this just walks through each of these models that we had here on the box diagram in terms of what is the information that came in, what's the information that came out of it, and how did it walk through this system, starting with climate, working down through seepage. Then we have on the next page--let me put that up on the other screen--what's going on in the EBS sort of components in and around the drift. Now we're at a different scale. Near-field chemistry, waste package cladding, waste form, mobilization of nuclides, and ultimately the release and transport of nuclides from the EBS, and then we're back to the natural system. Transport through the unsaturated zone, the collection of those nuclides in the saturated zone, and ultimately the transport out to the biosphere. We're going to walk through each of these boxes in the--I'm going to keep these out because I might come back to them.

Another way of portraying that if you're more into, you know, software of how information flows is we have a lot of external process models. These process models, whether it's a transport model or whether it's a thermohydrologic model or whether it's a geochemical model or whether it's a model of how the degradation of the package exists, provide output, if you will. They can be tables. They can be response surfaces. They can be actual output files from one model that are then fed into the TSPA model, which can be run in multiple realization mode.

Each of the outputs coming out of these things is uncertain, has a distribution, if you will. That distribution can be sampled and parameter values can be sampled. We can look at the fifth percentiles, the ninety-fifth percentiles, the fiftieth percentiles, the means of that distribution, and use those and see, okay, what's the impact now of that particular model, that particular component, that particular uncertainty on the overall system performance. So it just depends on how you want to look at it. If you want to look at information flow from a computer point of view, from how things are connected, or from a conceptual point of view, we've given kind of both alternatives.

Now, what I'm going to do is walk through each of the components in the base case TSPA, and I'm going to start with a conceptual description of what processes are going on and what processes we've tried to capture. Then I'm going to walk through the base case, if you will, the output of the base case, and then we'll walk through how that output propagates through time and through space as we get to, you know, the base case result, which is probably on about Slide 60. So some people are interested in the result first. If you're interested in the result first, you can turn back to Slide 60. I would encourage you not to, and just let us walk through to get there, because we'll be there in about an hour anyway.

BULLEN: Bob, can we ask a quick question?

ANDREWS: Yeah.

BULLEN: Alberto has a question on your previous slide.

SAGÜÉS: Sagüés, Board. On the TSPA-VA configuration, I don't know if you're going to be coming back to this, or this is the only time it's going to be shown.

ANDREWS: It was going to be the only time it's going to be shown.

SAGÜÉS: Okay. In that case, this seems to be at the core of what TSPA is going to be doing, namely, the relationship between the different components of the code. For example, very quickly, there are two waste package degradation boxes over there, and some of them, like the WAPDEG and the RIP; right?

ANDREWS: Well, this is providing a distribution of waste package failures, where failure is the first breach or the first hole, no matter what size that hole is. So it's providing results of the time variation of package failures. We have more than 10,000 packages, so we're looking at a stochastic distribution of time of first breach. So it gives that, and it gives the distribution of the area of the package that's exposed as a function of time. So that's where--and that's fed in.

SAGÜÉS: Okay. So that's waste package degradation and it is waste form degradation.

ANDREWS: And this is waste form degradation.

SAGÜÉS: All right. So there are three direct inputs to waste package degradation, the drift scale, thermohydrology, then the drift scale and zone flow.

ANDREWS: So we have seepage coming in. The presence of liquid water is important. The temperature and the relative humidity on the package surface are important. And the chemistry coming in of the incoming water is important.

SAGÜÉS: So that EQ3/6 is a chemistry module?

ANDREWS: Yes.

SAGÜÉS: And on the left is more of a temperature--

ANDREWS: Yes. This one, the thermohydrologic one is giving local temperature and relative humidity on the package surface. That same temperature and relative humidity are used in the geochemical calculations, but those are done external and then fed in.

SAGÜÉS: And when you have pH going down, is that the only input from that module into the waste package degradation, or is there something else?

ANDREWS: For the waste package degradation itself, that's the only input.

SAGÜÉS: So the EQ3/6 provides pH information?

ANDREWS: Yes. For a waste package, that's true. It's also providing information on the onyx drain carbonate content and pH inside the package.

SAGÜÉS: And how about things like chloride content, for example?

ANDREWS: Colloid?

SAGÜÉS: Chloride.

ANDREWS: That's not being fed in directly. What's happening in--you know, maybe I should wait until the waste package people talk. But inside the package at the contact between the corrosion allowance metal and the corrosion resistant metal, there's very complex and quite uncertain, you know, geochemistry going on at that contact, which is really driving the degradation rate of the highly corrosion resistant metal. That concentration, which includes chlorides, is very uncertain. So we, in fact, elicited that particular distribution.

SAGÜÉS: So how is the chloride concentration or composition of the environment inputted into the WAPDEG box? Does it come through the--there doesn't seem to be an input for it. Is there?

ANDREWS: I might be wrong, Jerry, but I don't think there is--there is not. In the base case, there is not a chloride input of the incoming water.

BULLEN: Bullen, Board. We'll see a presentation by Jerry McNeish for an hour this afternoon on the more intricate details of how this part is done.

So can I give Jerry fair warning that Alberto will ask the same question this afternoon?

SAGÜÉS: Thank you.

BULLEN: Thanks, Bob.

ANDREWS: Okay. Okay, let me keep that slide handy.

Okay, back to water. I want to walk through conceptual, what's going on, and then the results of what's in the base case.

Conceptually, what's going on is we have climate at the surface. That climate for our purposes, although there is a change in temperature with time, the climate will change with time, it's uncertain how it changes with time, or the timing of how it changes with time, but it's a given that it will change with time, so it is in the base case. In the base case, that is a change in precipitation with time. That precipitation, some of it runs off, some of it evaporates, some of it transpires, and some of it infiltrates, and we're only concerned with that fraction of that precipitation which infiltrates, which is something less than 100 per cent of that which precipitates. And we'll come to that.

Given that it's infiltrated, now it's going to move through the unsaturated zone. And in the unsaturated zone at Yucca Mountain, the rock is variably fractured. Different units have different fracture densities, different fracture characteristics, and in fact, amount of fractures, and some of the water is in the fractures and some of the water is in the matrix. And the UZ flow model is going to give us essentially that distribution between fractures and matrix.

When I come down at the smaller scale, down right around the drift, some of the water which is in those fractures will seep. Some of the water which is in the fractures will go around. The fraction of water which goes around, you know, as a function of the fracture characteristics of the rock mass, the higher the permeability, the higher the suction, the higher the capillarity, the less water will infiltrate into the drifts, or seep into the drifts. And we'll come back to that one.

And then of course below the repository, water continues to move, and if any nuclides are released, it can carry it away.

So we have four processes indicated on here; the climate processes, infiltration, flow through the unsaturated zone, and ultimately seepage into the drift. So I'm going to keep that slide up there for a little bit and walk through what's in the base case. And I think as Dan pointed out, for each one of these components, there's a more detailed, a slightly more detailed presentation coming up either later on this morning or this afternoon.

So this is the base case, single realization. We go into all this uncertainty for each component, and there's uncertainty in the timing and the duration of climate changes. But for the expected value, when I go in there and say sample just the expected value, the mean value, this is what pops up. What pops up is that for the first 5,000 years, we're in the present day climate. For the next about 90,000 years, we're in something called the long-term average. The long-term average climate is roughly a factor of two times the precipitation that's there presently. I'm looking at precipitation now as a function of time. And clearly, we've dramatically simplified the precipitation change with time, and to a series of step function changes, generally present day, long-term average, present day, long-term average, and then occasionally every 300 or 400,000 years, there's a super pluvial where the precipitation is roughly three times present day precipitation.

When you look at this, you can see that the performance is going to be driven, especially the long-term performance, is going to be driven by this multiplier of two. It's driven by the long-term average, because 80 per cent of the time, we're in this long-term average climatic regime. So that's the base case climate. Based on that base case climate, we have infiltration. We of course can only observe, directly observe the present day infiltration.

So there's been a lot of studies that the Board's been presented over the years by Allen Flint and co-workers at the USGS to try to come up with the most reasonable representation of the present day infiltration given our present day climatic regime.

That map here shown in two dimensions is here, ranges from very low infiltrations in certain regions, especially where the soil thickness is higher, or the elevation is lower, up to more than 20 or even 30 millimeters per year in certain areas.

The average over this whole area is something like four or five. The average over the repository block, which is what this number is representing, is about seven millimeters per year average infiltration. The average precipitation at Yucca Mountain right now is about 160, 170 millimeters per year.

When I put that into this time variation of precipitation, I end up with an infiltration rate that's now also varying with time, and we have three infiltration maps corresponding to those three precipitation regimes. These other two maps are generated by using the same model as we have for the base case present day infiltration, and just changing the precipitation and other factors to get a revised estimate of net infiltration over the repository footprint.

Having that, we can go to percolation, which is the amount of water now moving through the rock. You could see from the previous slide, as well as the center portion of this slide, that my infiltration is spatially variable, so it's not so surprising that my percolation, my average volumetric flux per cross sectional area, is also spatially variable. And we tried to capture that. It's difficult. You clearly can't capture 10,000 points of percolation. That would be kind of crazy to think that we had that level of resolution on percolation variability. But we can try to capture some variability, and we've done that in six regions.

Why have we done that? We've tried to capture at to a limited extent some variability in geologic properties at repository horizon and variation in hydrologic regimes at the repository horizon, obviously predicted hydrologic regime, because we don't measure percolation at the repository horizon. It's impossible to measure, but you can get estimates of it from the calibration of the model, et cetera.

The other reason for varying it into six regions is to capture edge effects versus center effects on the thermohydrologic regime. So six is the number we came up with. Six is a magic number, you'll see, because we do the same thing in the saturated zone for a different reason. But we have six regions.

Within each of those regions, we have a distribution of packages, and we have distribution of package types, you know, for spent fuel, for high level waste, and for commercial waste. And you can see even within these six regions, the mean values are varying from six to--well, here's a low of four, up to 11 millimeters per year. So there's spatial variability in average percolation, present day climate at the repository horizon.

BAHR: Can I ask a question?

ANDREWS: Yes.

BULLEN: Go ahead. Identify yourself, please.

BAHR: Jean Bahr, University of Wisconsin.

Are you using in your model just the mean values, or do you use that distribution within those six zones?

ANDREWS: We have used the mean value when we go into seepage. But to accommodate the fact that there's some likelihood of higher values than that mean, we've adjusted the seepage model, if you will, to account for the probability of there being a slightly higher percolation flux than the mean of that distribution.

BAHR: And what do you mean by adjusted? You just shifted the mean a little bit?

ANDREWS: I didn't shift this mean. I shifted the actual seepage model, which is correlated to percolation to account for the fact that I can have seepage at really lower percolation fluxes than the model would have said. I think Mike will talk about that in a little more detail, how we went from percolation to seepage. So, no, we did not use this distribution exactly.

The next thing is seepage, which is this last part here. And seepage is a very tricky one, and we believe it's strongly a function of the actual percolation flux. So we developed a model, or I should more correctly say LBL developed a model of average net seepage for a range of different percolation fluxes. And because percolation fluxes vary with time and they vary with space, the seepage flux is going to vary with time and space. And there's two important outcomes of seepage. One is the distribution of seeps, the spatial distribution of seeps given a given percolation flux, and the other is the volume of water per seep, so the liters per year or cubic meters per year per seep.

What we've done is, and Mike will go into this in more detail, but developed a model which generally looks at only the fracture component, because the matrix Component A is small, it's about 10 per cent of the total flux is in matrix and 90 per cent is in the fractures anyway within the repository host rock unit, and also the matrix generally has very high suction, so the water very much likes to stay in the matrix, doesn't want to come out of the matrix into the drift, into the opening.

But within the fractures, it can come into the drift, but the propensity of water to come into the drift from those fractures is a function of the permeability of the fractures, as I said before, and the suction within those fractures. The suction within the fractures is difficult to measure the parameter, so it's quite uncertain. So there's going to be a range of different seepages that are more or less correlated with the range of possible fracture capillarity or fracture suction, and so you get a wide range. At any given percolation flux, you can have a very broad range. This is going from the ninety-fifty percentile to the fifth percentile of seepage, this now the seepage fraction where seepage fraction is the per cent of waste packages that get contacted by water.

If I look at just the mean for a particular region, and this is the center region that we looked at in the previous slide, that mean was about 11 millimeters per year percolation flux. In the present day climate, it increased to--I don't know what it was--about 60 millimeters per year in the long-term average. But if I look at the 11 here, you can see I have about 10 per cent seepage fraction, and that 10 per cent is this number. So in the present day, it's 10 per cent. In the long-term average, which is about 60 millimeters per year, it's about 30 per cent. And in the super pluvial, it's about 50 per cent. So these are the per cent of packages in one realization that can ultimately see water, liquid water.

BULLEN: Excuse me, Bob. Bullen, Board.

In your realizations then, are there packages that never see water, or does the probability move around so that all packages see water at some time?

ANDREWS: No.

BULLEN: There are packages that never see water?

ANDREWS: There are packages that never see water; that's correct.

BULLEN: Okay.

ANDREWS: Never see liquid water. They see humid air, but not liquid water.

The other important output from this is the seepage amount, the volumetric flow of water into the drifts. And we use that same model, that same representation of heterogeneous fracture network that Mike will show you, and its basis, which is tied back to some niche studies and ESF that relate--the volumetric flow right now in meters cubed per year as a function of percolation flux.

Then going back into my center region showing one distribution of volumetric flow as a function of time, at 11 millimeters per year, I have something like .03, so that's this value here, the lower value occurring during the dry periods, which the present day is a representative of. Then I go up to a .3 meters cubed per year, or 300 liters per year during the long-term average, and then up to a maximum of almost a meter cubed per year during the--

BULLEN: Quick question from Carl Di Bella, Staff.

DI BELLA: Bob, this is Carl Di Bella. Bob, could you clarify whether you're talking about an intact drift or a drift after some period of time that the roof has begun to fall in, and so forth?

ANDREWS: Good question. These calculations are from an intact drift. But the dominant thing we're looking at is the fracture characteristics and the surface area of the drift. And we acknowledge that this distribution is uncertain, and part of that uncertainty is how those fracture characteristics in fact might change with time. This is based on present day observed or inferred fracture characteristics. Clearly, it could change with time, and one of the ways it could change with time is, you know, the roof could collapse and the fracture permeabilities change, and fracture characteristics change.

In order to accommodate that, we've done a number of sensitivity studies of a range of different fracture characteristics that Mike will talk about. So acknowledging that seepage will be important, and it will be, you'll see that later, we tried to look at a range of different fracture characteristics and different fracture permeabilities which ultimately gave a range of different flow rate versus percolation curves, because that's the output of the model, or the assumption is what is this exact distribution. And it's quite uncertain here, even in the base case.

BULLEN: Chris Whipple?

WHIPPLE: Chris Whipple. Bob, just a quick question. My understanding is that the base case includes a steady state seepage through the drifts. Have you looked at in your sensitivity studies an episodic flow through the drifts?

ANDREWS: No, not yet.

CRAIG: Could you tell me how to translate cubic meters per year, which is either entering the tunnel or the package, into the number of--the flow per unit area of the package?

Is there a direct--I'm having trouble understanding the definition of this cubic meters per year.

ANDREWS: Okay, this is the cubic meter per year per seep that hit a package. So I've removed all of the zeros, so those seeps that were zero and had no seepage essentially so no packages were contacted, which we saw on the previous slide was between 70 per cent and 50 per cent for the present--well, for long-term average and super pluvial, and I've only looked at that fraction of packages that saw seeps, what is that seepage rate. So this is the volume of water for each of those packages that got a seep that hit that package per year.

CRAIG: So if I want cubic years per square centimeter of surface area, I divide this by the area of--

ANDREWS: Of a package, yeah.

CRAIG: That varies, however?

ANDREWS: The actual area of a package doesn't vary, but the actual opening size of a package does vary with time. So the fraction of water that got into a package is a fraction of this volume. But this volume is the volume of water that hit each package that got hit by dripping water.

BULLEN: Alberto?

SAGÜÉS: So the numbers then seem to suggest that about one package out of every three will be experiencing a drip, and that's about 50 gallons of water per year, or something like that. Now, is the modelling to the point where it says that it's going to be always the same package, or drips in this year on this package and the next year on a different package?

ANDREWS: Well, that was--

SAGÜÉS: I guess it's the same.

ANDREWS: Yeah, we're saying if it drips on this package, the propensity is that it will likely stay on that package. We don't move it from package to package with any kind of random moving of drips. Now, on a package it might move across that package so the whole package can ultimately get wet rather than one location on that package.

SAGÜÉS: Why is that? Why cannot it just keep on moving to the next package?

ANDREWS: It could, other than the fact it's about 10 meters away, and if there's a geologic control at all on seepage, which we tend to believe there's some geologic or hydrologic control on seepage, its likelihood of moving over a 10 meter range is less than moving over a 10 centimeter range. Plus, the 10 centimeter range, you know, getting back to Carl's question, the rocks will fall. The rocks will, you know, part at the liner/rock interface. And so the likelihood of drips moving over tens of centimeters is pretty high.

SAGÜÉS: Right. The question is somewhat important, though, in the sense that from a point of view of durability and corrosion and the like. There may be quite a bit of difference between the water dripping persistently on the same spot of the package, or moving about.

ANDREWS: That's true.

SAGÜÉS: But at this moment, we don't have really enough information to say what.

ANDREWS: That's right. We will do a sensitivity study, just so you know, of water staying on the same location for long periods of time, versus that same water volume moving over the package surface, which is what we have right now in the base case, that the whole package surface is contacted by water when there is a seep, and that will make a difference.

Okay, so having walked through the hydrologic regime unperturbed by any waste emplacement, now it's important to look at the hydrologic regime impacted by waste emplacement. And conceptually, of course, the waste is hot. Water is driven away for a certain period of time. Humidity is decreased for a certain period of time. And then ultimately, water comes back and humidity increases and seepage can begin.

So we have a varying time distribution of the thermal hydrologic regime in the drift and on the package and around the drift.

What does that look like in terms of a base case?

We have many different packages, many different package thermal outputs, and we have a spatial dependency on those thermal hydrologic regimes.

This plot simply shows schematically one set of packages in one particular region for one particular climate state. So with climate changes, percolation changes, especially between present day and long-term average, and the thermal regime is going to change. And because there's uncertainty in infiltration as well, the thermal regime is going to change. So we have a wide range of thermal hydrologic responses caused by both spatial variability, package to package variability, and uncertainty in infiltration, et cetera.

METLAY: Dan Metlay, Board.

The different lines are different colors for the different regions?

ANDREWS: And different packages.

METLAY: Different packages.

ANDREWS: So this is just illustrating package to package variability in a particular region. So we have a range of thermal hydrologic responses that of course not surprisingly are more significant in the first hundred or hundreds of years and become less significant as you go out in time as the thermal regimes and the hydrologic regimes start coalescing and the thermal profiles and the hydrologic profiles also start coalescing from package to package. And there can still be differences from region to region because I have different infiltration rates from region to region.

So we see--I mean, a key point on here, just for information, is the dividing line at about 70 or 80 per cent relative humidity. Things can start corroding and they corrode at a higher rate once you get to about 90 per cent relative humidity. So you can see not much on the packages are going to happen in the first few hundred years, maybe 800 years, and then the packages are going to start corroding after that, and as the relative humidity gets up to 90 per cent or so, they will corrode at a more rapid rate.

BULLEN: Bullen, Board. Bob, we've seen some interesting results on the large scale drift scale test on the variability of relative humidity due to potentially barometric pumping. As those data become more and more available, how will they be incorporated into, you know, your change in this curve to say that maybe we don't drive the relative humidity all the way down to 20 per cent for a couple hundred years, that there may be some variability between 20 and 50, and then that variability would also creep up the curve? Those kinds of data will be incorporated obviously by the LA application. But how do you incorporate them in this kind of case? Do you just do another analyses that says we have a different shape of the curve in the regions and packages, or how do you do that?

ANDREWS: Well, let me--there's a little bit of time lag obviously between collection of data, revision of a model, incorporation of that model with the reference design, and doing calculations and doing VA calculations, and then doing documentation. So obviously there's a time lag between real data and actual incorporation into a TSPA analysis.

So in a way, your question is going to be--we're going to document that and say this observation was made while we were doing these analyses. The impact on the model is "X." If there is an impact on the model, the potential significance of that could be "Y" but we probably are not going to quantitatively have time to evaluate it in the VA.

BULLEN: No, I agree for VA. I was just wondering how you take the data and then make the modifications. That's thee part I was interested in.

ANDREWS: Okay. In this particular case, we have a several tiered model, a mound scale, drift scale model of thermal hydrologic regimes that are connected and tied, because there's mountain scale processes going on as well. That drift scale model which is used to calculate these can be updated based on new observations. I mean, that drift scale model is the same model that's been used to predict the thermal hydrologic response of the test itself.

So as new data come in, if it warrants changing the basic process model, then they would change the basic process model. Maybe some of the parameters were, you know, within a certain distribution such that the observed thermal hydrologic response would be better matched or better predicted with a change in properties or a change in conceptual models. And then that would, if that were the case, which I'm not sure is the case, if that were the case, then we would use that new model to make forward predictions of the thermal hydrologic regime.

So there's always an iteration between data collection, improving your model, you know, incorporating it into now an assessment, an evaluation of thermal hydrologic response, long-term thermal hydrologic response, and then, you know, its performance significance or implications of that.

We are going to do--in fact, we've done, I don't think we're going to present them here, though, a range of different thermal hydrologic models to see for different thermal hydrologic models, what is the sensitivity of the result to that range. Does that range encompass what's observed in the current ESF tests? I'm not exactly sure. I would hope so because it's a pretty wide range of thermal hydrologic models that we're using. It doesn't have much of an impact.

SAGÜÉS: Are those absolute limits, or are they like 95 per cent type of limits for the highest and lowest possible cases?

ANDREWS: For the highest, they pretty much are the actual limits, because this is the design basis waste package that the designers like to look at lots of times, which is the highest thermal load. I think it's an 18 kw per package package, so it's a very hot package. So this is an absolute on that particular one.

What we've essentially done is taken eight, I believe, it might be seven packages that represent the different package types and say that represents the package to package variability, the package to package thermal output. So that's why we have eight lines up there.

BAHR: One more question on that. So the only thing that's driving the variability in those results is the package design. There's no variability in the seepage flux? There's no variability in any other process in that? So we're just looking at one average seepage flux and--

ANDREWS: Yeah.

BAHR: Okay. Because the real variability is going to be much larger because of all of the other variable processes?

ANDREWS: Much broader than this. Whoever is doing the thermal talk, why don't we clarify that. I guess that's Mike; right?

Okay, geochemistry. I don't have a nice conceptual picture of geochemistry, so I have to go straight to the chemical environment. And we're looking at the chemical environment at essentially four different locations. One chemical environment as the water comes into the drift, and it could be affected by the presence or absence of concrete in the liner, one at the waste package surface looking at reaction with steel, one as it's reacting with the waste form itself, whether that be a glass waste form or a spent fuel waste form, and again we have the invert down at the bottom, so chemistry down there.

So we have a range of different--these would be time in years--a range of different chemistries calculated external to the TSPA that are then fed into the TSPA, and these define the chemistries of the water on the package surface, and ultimately they'll define the change in chemistry inside the package as a function of time. This just shows one particular component, pH. We have other chemical components that are driving the performance that I haven't illustrated here.

Okay, now we have water. We have water that contacts the package, whether it be humid air or whether it be dripping, and that water has a certain chemistry, and so we can start the degradation process on the packages themselves. So conceptually, what's going on is we have the corrosion allowance metal, this 10 centimeters of mile steel which can corrode under humid air or aqueous conditions. Once the corrosion allowance material has been degraded, then liquid water or humid air can contact the corrosion resistant metal, in this case C-22, and it can start degrading. It can degrade by general corrosion or it can degrade by pitting crevice corrosion.

And now I think the question from Alberto is probably easier to answer. The chemistry of this fluid phase in this gap between the CAM, the corrosion allowance metal, and the corrosion resistant metal is very important in terms of the degradation characteristics of the corrosion resistant metal itself. It is observable, but we do not have tests right now. I think there's some tests ongoing to try to look at what that chemistry can be under a range of different external chemical environments, but for now, we've used an elicitation to derive what the possible chemistries are and the probabilities of different chemistries at that contact. And that drives the degradation rates of the waste package itself.

The output is in two forms. First is the time at which the initial pit or the initial patch. If it's a general corrosion that's failing as patch, so larger holes if you will, or very localized corrosion, crevice corrosion can also occur. So we look at both of those. And first, of course, we have to have degradation of the corrosion allowance metal, and then we have degradation of the corrosion resistant metal. And what you see here is that the corrosion resistant metal, which is now the time of breach of the package and now water can start getting into the package, starts at a few thousand years, but with a very low percentage of the total number of packages, until about 100,000 years, it's about 20 per cent of the packages that got dripped on. This is for dripping cases. 20 per cent of those packages have at least one breach, and we can see at a million years, they essentially all have at least one hole or pit through the package.

Because it's not very easy to see what's happening here in this first time period, we plotted the same information on a log scale, and we see that 1 per cent of the packages have at least one pit through them at about 8,000 years. It didn't come out very well, but this dotted line is the patches failing, and initially they're failing with localized pit corrosion, and then ultimately, they're failing with holes, with patches. The first patch goes through the first package in about 15,000 years, something like that, after closure. All these times are after closure.

So these define when waste now is at least potentially exposed to liquid water. It's the initial failure.

WHIPPLE: Chris Whipple. A question for clarification.

Are these time to failure curves for the whole mix of cans that both see liquid water and also see humidity?

ANDREWS: No, these are just those that see liquid water.

WHIPPLE: Okay.

ANDREWS: This is that portion. Leon?

REITER: Leon Reiter, Staff.

Bob, does this take into account the climate changes?

ANDREWS: There's a climate change that occurred in that first 10,000 year time period, which changes the percolation and, therefore, changes the thermal hydrologic regime. So there is a time switch of when, I forget if we used about 1,000 years ultimately of that climate change occurring, therefore, changing the thermal hydrologic regime, therefore, changing the package degradation. So the answer is yes.

BAHR: But there's no super pluvial?

ANDREWS: No.

REITER: You get the super pluvial at 300,000.

ANDREWS: But the time change from present day to long-term average occurs in the first 10,000 years.

REITER: Right. I'm just trying to see whether or not the climate change and increased percolation flux are reflected in the plot of waste package failure.

ANDREWS: No, this is those packages that get drips. If it got dripped, and the percentage of packages that get this changes with time. So we have to multiply that percentage of packages with drips by this distribution. I'm sorry, I should have clarified that a little better.

The other important output not only is the time at which the first patch or first pit failed important, but the time of package distribution, the cumulative distribution of number of patches or number of pits as a function of time, that's also important. And that's represented on this plot, which is just showing number of patches, which are a few hundred square centimeters in cross-sectional area. Those are general corrosion of the corrosion resistant metal versus in this case number of pits. Number of pits are about a square centimeter, so they're very small openings, and their pit distribution and the fraction of packages at four particular times.

Okay, having failed the waste package, walking through the rest of the system now, we can start exposing the waste form, or exposing the insides of the package to the environment that was outside the package before. Of course, it had the temperature environment, but now I can give it the aqueous environment that was outside the package.

What we have here is 21-PWR assembly, and shown up in blow-up is that assembly with zircaloy cladding on it, if it had zircaloy cladding, which 99 per cent of the pins, the commercial pins have, and then the degradation of that cladding, and then ultimately the exposure of UO2 fuel. If UO2 fuel is not exposed, then there's no release. If it is exposed, then the water may be able to contact it, and ultimately get to release. And this particular one is just a blow-up of an exposed fuel pellet, you know, showing that with time of course it degrades. There's surface area, there's either hydroscopic or actual liquid water that can contact that surface, and nuclides, you know, due to alteration of the spent fuel itself, nuclides released into either alteration products or into the liquid phase. And then if they're in the liquid phase, they can start moving in the liquid phase.

So we have these processes going on inside the package, and the first process that has to happen for commercial fuel, not glass, but for the commercial fuel, is that the cladding has to be degraded. So how does cladding degrade? Well, cladding degrades by high temperatures and can creep rupture at very high temperatures. If you get above 350 degrees C. it generally is assumed that the creep is sufficient that all cladding is removed. If you keep the temperature sufficiently low, then the probability of having creep rupture is also dramatically reduced.

But it can also fail by corrosion, but it's a very corrosion resistant metal, even more corrosion resistant than the C-22 inner layer of the package, and it can fail by mechanical degradation. You know, the rock can fall on it. You know, the package could fail sufficiently so that the pins can be ruptured. So we have a number of degradation modes that drive the degradation of the cladding. And ultimately, we have a wide distribution, because there is uncertainty in all of those degradation modes that ultimately ranges from a few per cent to 30 or 40 per cent of the cladding degraded as a function of time. And we're sampling from this distribution.

It's difficult to say which process is driving the degradation at any particular time, and rather than try to capture that, we say let's just capture the whole cladding degradation response as a function of time. Alberto?

SAGÜÉS: Yes, when you stated zircaloy is a hundred times more corrosion resistant than the C-22, I guess that that's the numerical assumption made over there. In what sense is it? For example, the rate of pitting would be 200 times smaller, the rate of progression of pits, or the time to perforation for a given thickness would be a hundred times greater in zircaloy than in C-22?

ANDREWS: Essentially, yeah.

SAGÜÉS: Okay. Which would be then a 0.2 millimeters thick sheet of zircaloy will be the same as a 2 centimeter thick sheet of C-22?

ANDREWS: That's a good way of looking at it.

SAGÜÉS: Now, the problem with that, you know, for total transparency and credibility, some would say well, gee, then when I put a sheet of .2 millimeters thick of some zircaloy, I'm going to double the life of the package. But somehow that doesn't make sense.

ANDREWS: What we have--I mean, the other thing you have to consider is the total surface area, because this now, at least for the corrosion part of it, is going to be a function of the total surface area of that cladding surface, because I have degradation that can be local and I have a stochastic process that says degradation can vary locally from point to point, because I have uncertainties and I have this variability in corrosion processes from point to point, chemistries, et cetera. And the total surface area of the cladding is very, very large in comparison to the surface area of the package, so the probability, it's not quite as positive, I guess, as you would have alluded to, because that surface area of the package is small in comparison to the surface area of, you know, thousands of pins sitting inside that package.

SAGÜÉS: But to put it the other way around, though, the more surface area you have, the more likely the probability of a pit?

ANDREWS: That's right. That's right. That's exactly correct. So the higher the probability of a localized corrosion of the zircaloy is with respect to the C-22. So it's not quite as simple as just looking at the rates and thicknesses. You have to consider the total surface area, and then the probability is also related to the total surface area.

SAGÜÉS: Okay. But conversely, there's sort of a not double standard, but two different ways of looking at it. The immediate question would appear then why not, if that material has such a great corrosion performance, why not, using that as a part of the overall waste packages, in addition, why not put it outside as a drip shield, and so on and so on. I mean, one could get those kinds of ideas, and the amount of material involved would be extremely small. You know, it's not going to be--nickel alloy is not going to be that much greater when you're talking about such a difference in performance.

ANDREWS: I'm not the cost guy. So I can't explain the costs of zircaloy and the difficulty in fabrication and construction and emplacement and checking of the zircaloy. You know, you probably should ask Dave Stahl or some of the designers that kind of question. And I think Dave is here and maybe we can answer that in the question and answer part.

BULLEN: Bullen, Board. When you make this a hundred times more corrosion resistant than C-22 comparison, is that comparison of C-22 to virgin un-irradiated zircaloy, or is it to what you expect the conditions to be after 60,000 megawatt days per metric ton fuel burn-up with oxide from the core and hydride and reorientation and all those other challenges that are the microstructural evolution of the clad, which is going to be a real bear to document, by the way? If you recall want to go to licensing with clad credit, you're going to have a real challenge associated with, one, making that correlation, but then making that correlation to virgin material versus that correlation to something that's been irradiated. There's a big difference.

ANDREWS: You know, there are limitless data on the corrosion degradation of zircaloy in a range of different environments. That's a very broad range of degradation characteristics. This factor of 100 is a rough factor. It goes--in fact 100 is kind of the minimum, and like 10,000 is sort of the maximum. We said, okay, let's go with what seems like a reasonable minimum on that. You know, does that complete range of 100 to 10,000 represent some of the uncertainty that you're talking about? Maybe. Maybe not.

We are going to do sensitivity studies. I mean, this is a critical element of performance. We want to incorporate it because it is part of the repository system. It is part of the number of barriers between water and waste.

It is there. We want to look at its importance. We want to remove it and see what the importance of it being taken out of the safety analysis is, or look at this range and see, you know, the fifth percentile, ninety-fifth percentile, how much does that range make a difference over a 100,000 year time period, over a million year time period.

BULLEN: Bullen, Board. But with the uncertainty that's associated with the data that you have, wouldn't it be more credible to add it in as a sensitivity as opposed to incorporating it into the base case?

ANDREWS: There's always an argument of what goes into the sensitivity and what goes into the base case. Things that, you know, there's a lot of data--there are a lot of data on zircaloy degradation characteristics under a range of different environments. You know, what we had to do was somehow compare it to another metal that we had a limited amount of data to.

BULLEN: Right. But actually I've looked at corrosion of zircaloy data, and when you ever talk to a vendor, they'll tell you that they'll give you six year data, because that's how long it's going to be in a spent fuel pool, and when you extrapolate to a million years, that data is less reliable, I guess is the way to put it.

ANDREWS: Yeah, I think we acknowledge that there's uncertainty in this component, as in many of the other components. But it is a component that is there, and it is there at emplacement. Now, how it performs over time is clearly uncertain.

BULLEN: Right. The key question is will it be there at a million years when you want to take credit for it, or whatever. And I think that's one of the cautions that I would have, that clad credit based on the fact that you don't have an NQA-1 quality evaluation of every tube as it goes in is a real stretch for a licensing argument. And I think that that might be a strong caution I'd like to issue.

ANDREWS: Okay. For licensing, I mean, this is a--

BULLEN: This is VA, but if VA carries on to licensing, then you've got to really take a strong look at what you're going to do when you do clad credit.

ANDREWS: I agree. I mean, let's back up a couple of steps here and realize that one of the purposes of the VA and one of the key purposes of the VA is to identify the amount of work the project needs to do to go from 1998 to 2001, 2002. Given that's one of the objectives, you know, one of the ways of evaluating that objective is within the performance assessment to look at a bunch of trade-offs, if you will, and some of those trade-offs are design trade-offs, some of those trade-offs are uncertainty in information and reliability, if you will, of that information, and what additional data if you did want to incorporate this as a licensing argument, what kind of data would you require to make that licensing case.

BULLEN: Right.

ANDREWS: I mean, it may be that it's a defense in depth argument, not a quote, unquote base case licensing argument.

BULLEN: That I would buy. The defense in depth is probably something you should use instead of the base case. In the base case argument, what you do is you introduce a great deal of uncertainty long term when you put something like this in, and you just acknowledged the fact that there would be that uncertainty. So maybe the defense in depth argument would be as a sensitivity as opposed to inclusion in the base case.

ANDREWS: That may in fact be the licensing argument. I'm not going to prejudge what's the licensing argument versus the VA. And in the VA, we're trying to put as many things in there as we felt reasonable, and in some cases, there are some conservative assumptions, and in other cases, we're trying to be more reasonable and we're trying to point to what additional information could be generated over the next two, three years to help refine models and have confidence in individual models. But first let's evaluate the significance of it and see if it makes a difference.

FRISHMAN: How do you deal with the question of how much failed fuel is going in in the first place? You know, the rate is very low right now, but it has been higher in the past. And also dry storage may have some effect on what you actually emplace.

ANDREWS: Yeah, the failed fuel at emplacement is between 1 and 2 per cent, which is generally--some of that is early creep, you know, some of that's early mechanical, and some of that is--some fraction of that is stainless clad, and some of that is initially failed fraction. Now, that initially failed fraction is like a tenth of a per cent, or something like that, at least from the industry average. It does range, you're right. You know, some of them are a half a per cent, but most of them are down in the tenth of a per cent or even less in terms of initially failed.

FRISHMAN: I thought with some of the older fuels, it was higher than that.

ANDREWS: In the older fuels, it is higher. But in terms of the fraction that those older fuels are in the total fraction, it's a pretty small fraction of the total.

FRISHMAN: Okay. Well, let's just do sort of a logistics question then. If you are taking oldest fuel first, you're putting it in the north end of the block where your infiltration is different from other places, fracture patterns may be very different, is there some way that you're taking account for the fact that you may have the most vulnerable fuel which you put way down on the bottom end?

You may have the most vulnerable fuel in maybe the system that is going to produce the highest releases.

ANDREWS: We could--

FRISHMAN: Are you averaging the whole repository, or are you getting down to the level of saying what fuel is where and what is it going to do?

ANDREWS: We're averaging over the whole repository right now for the viability assessment. The whole repository is averaged, you know, packages and averaged fuels.

BULLEN: I'm going to exercise the chairman's prerogative. This is Bullen. I see that Bob's got about 30 viewgraphs left and we've got about 30 minutes left, and so I think we'll try and defer questions until the end from here on out, if that's okay.

ANDREWS: Okay. I can go faster, too.

BULLEN: Well, don't go too fast. We'd like to understand each viewgraph.

ANDREWS: Okay. Once the clad has failed, the waste form now is exposed to--can be exposed to liquid water. We can certainly assume that every exposed surface is in contact with liquid water, so can degrade. You know, it could be that that's not true, that only a certain fraction of it is exposed to liquid water and the other fraction is just exposed to humid air. But we've said let's just assume it's exposed to liquid water.

And then we have a distribution of dissolution rates. Now, in this case, both of these for spent fuel as a function of temperature and as a function of two key geochemical parameters. It also is a function of--there's some other chemical function, I forget which it is, though.

Okay, another important issue. One the waste form has been exposed and it's started to dissolve or be altered, several things can happen. A secondary phase can form, and the nuclides can exist in that secondary phase, and that secondary phase can sometimes be mobile, and mobile in the form of colloidal transport. The probability of colloidal transport is a function and the stability of colloids is primarily a function of the ionic strength which is coming from the geochemistry predictions.

For TSPA-VA, right now we're considering plutonium colloids only as a surrogate for all colloids to look at their potential impacts. We're looking both at reversible and irreversible plutonium colloids. That fraction that's irreversible, i.e. once the plutonium is on the colloid, it stays on the colloid and does not come off the colloid, has been derived from some analog type information that's been collected by LANL and others at the NTS from the Benham shot there, which has been in the news recently of plutonium migration about a kilometer from the Benham shot. So that's been used to derive the fraction of total colloids or total plutonium on colloids that's irreversible. That will not desorb off of the colloid.

Another key component is the amount of nuclide that's actually in the dissolved phase. This is being driven by the solubility of that nuclide. We have some data that are very far from equilibrium, and we have some other data that are very much controlled by the processes at the waste form surface, short-term processes at the waste form surface.

Some modelling results say that the more reasonable range is somewhere between those two extremes, and it's that range which is uncertain that we're using in the TSPA-VA. So in this case, it's neptunium solubility. Neptunium, as we will see, is the key nuclide, and so the solubility of neptunium ends up being a fairly key parameter, although surprisingly it didn't pop out in the sensitivity analyses as such.

I have a word slide on EBS transport, because now once I've degraded the package, I've exposed the package, I've exposed the fuel and I've degraded the fuel, now I can start transporting it out of the package. So now we're going to start seeing results of an expected case. So walking through everything that we've talked about up until now, this is the first time we've seen nuclides and the first time we've gotten releases of nuclides. And I've shown four curves here. The top two are technetium. The bottom two are neptunium. Technetium and neptunium will be two key nuclides that we'll track through the rest of the system, and it's worthwhile looking at these four plots in some detail.

First, you immediately see they have different character. The very character of the plots is dramatically different. Technetium is bouncing all over the place, and neptunium is a nice smooth curve. So your probably immediate question is what's going on here? Why do they look so different?

Technetium has very, very high solubility. It's essentially being driven by the release rate of the waste form itself, so the dissolution rate or degradation rate of the waste form itself, which is also very high. For those intrinsic dissolution rates that I showed you on the previous slide, I think I had a bullet on there that said essentially the dissolution rate, intrinsic dissolution rate is on the order of a thousand years. So it's completely altered in roughly a thousand years, at least based on the laboratory data available.

So any time a package fails, it will release relatively quickly. And for the high solubilities, whether it be advection or diffusion, advection being liquid water through the package, or diffusion being due to a concentration gradient, the neptunium comes out almost not quite instantaneously, but on a plot like this, it's instantaneously. So this structure is essentially looking at the structure of waste package failures. It's the distribution of waste package failures that drives a distribution of technetium release. The advective release is about an order of magnitude higher than the diffusive release through the package and through the invert.

The diffusion coefficient for the invert saturations that we have is quite high. So diffusion is not as significant a barrier as one might have guess it should be. But it's about an order of magnitude less. We're just tracking them both separately in the EBS.

Neptunium on the other hand is very different. It's solubility limited. It's not so affected by the vagaries of package failures. As more and more package fails and as more and more waste is exposed and as more and more waste is in contact with water, it has a gradual increase. And as we will see when we go out to a million years, it continues to increase. More and more packages are coming on line. It has a very low solubility. It's sitting there waiting to be mobilized, waiting to be released, and it does as more and more packages fail, it does release.

So more or less what you're looking at here, although this is releases from the whole repository, is you're looking at waste package failure rates for technetium, causing the bounciness, and cumulative waste package failures for neptunium, so a very different shape. But also in the case of neptunium, the diffusive releases are about a quarter magnitude less than the advective releases.

Now, I had one bullet on one slide I think I skipped over pretty quickly, we do have some possibility of early waste package failures. That possibility is low. It's based on a number of things. It could be mechanical failure or it could be a large rock, a very large rock falling at early times, and it's very uncertain, so we gave it a very broad uncertainty band. For a given realization, for the expected value of realization, that caused one waste package to fail, to be breached, at about 1,000 years. And what you're looking at here in the case of technetium is that single waste package failing.

The reason that it drops is because of that climate change that we talked about that occurs at 5,000 years. And then it increases and as more and more packages fail, it increases until it reaches this more or less steady value, and it will be at that steady value while packages continue to fail and be breached. This is in activity per year, curies per year.

Having released it from the EBS, we now can transport it through the unsaturated zone. The colloids will move. They can be sorbed onto--or the colloids themselves can't be sorbed, but nuclides can be desorbed off the colloids and sorbed onto the rock, or nuclides can be sorbed either by matrix diffusion or by surface sorption on the fractures, sorbed through the fracture transport.

I'm going to show one kind of example, and then we'll show the results of incorporating it into TSPA. This just shows through the three different climate states we have, present, long-term average and super pluvial. The expected value parameter set distribution of arrival times from the repository horizon down to the water table. And if you look at the fiftieth percentile arrival of mass, you know, in the present day climate, it's not quite 10,000 years, but close. But as I increase that percolation flux due to climate change, going to long-term averaged, it's less than 1,000 years and super pluvial is even less than that. We're just pushing more water through there and more water is going in the fractures, and the fracture velocities are quite high, and the likelihood of having fracture transport also increases.

So when I roll that into the actual TSPA, and I'm just looking at technetium here--there's a slide later on that's going to be neptunium, which it was supposed to have been a different neptunium slide, but I screwed up at the last minute--what we're looking at is the release of technetium to the water table from that exact same case I showed you before, as a function of time.

We saw before that--and now I've broken it up into six regions in the saturated zone. I'm trying to collect nuclides at the saturated zone and then I'm going to transport them in the saturated zone. So I have six different regions, they were shown on that very first schematic, which are not exactly the same six regions as the six regions in the repository, but we liked the number six, so it's still six. And we are tracking each of those six separately, and in fact transporting each of those six separately, and we see that if I sum these, it's about 10 to the minus 2 curies per year, which is actually the release rate from the EBS.

So because there is no sorption of technetium, all I really have is a slight delay. And, in fact, that slight delay is on the order of a few thousand years, or less, during the long-term average. So if you compare this plot to the other plot, they are very similar. All I've done is move them to the right a little bit in the unsaturated zone.

Having gone through the unsaturated zone, we'll go through the saturated zone. Here's all the processes going on there. They're very similar, but now it's in the saturated rock, not in the unsaturated rock. I still have the sorption. I have the difference between the volcanic units and the clastic units, the sediments, the point of release here and the point of extraction here at 20 kilometers down gradient.

NELSON: Nelson, Board. I have a question.

Can you go back to the previous slide, just for clarification? Can you explain to me why it is Region 5 which has the highest release rate?

ANDREWS: Yeah, let me--that's a good question. This is Region 5 right here, and there is, even in the long-term average climate, there is some lateral flow from west to east through the Calico Hills and some of the vitric layers in the unsaturated zone from the model, and that left to right, if you will, or west to east movement causes slightly increased capturing, if you will, or release in this region as opposed to some of the other regions. So it's being dominated by this slight lateral diversion of flow in the unsaturated zone, which is now carrying the nuclides, and the nuclides are going with the flow and then coming down here.

NELSON: So even though Region 6, for example, may receive higher precipitation and flux, you expect more seepage through the near field in Region 5?

ANDREWS: No. Now, I'm looking at the saturated zone. i'm capturing things in the saturated zone. Let me go back to this one. I may have gone over this a little too quickly.

We've discretized, if you will, the repository block into these six regions to try to capture at the repository horizon variability in percolation, variability in properties, and also variability in thermal hydrologic response. So at the repository horizon, we've broken it up into six regions.

In the top of the saturated zone, we want to capture all of the mass, all of the release that would have gone between the repository and the saturated zone. So in so doing, we've discretized it up into six blocks, which are now based on the lithology, the geology in the saturated zone, not the geology and hydrology in the unsaturated zone. So we broke it up differently to capture that geologic variability in the saturated zone, and now we're just collecting mass, if you will, collecting those particles of nuclides that would have been transported from anywhere in the repository block into the saturated zone. And it just happens to be that we have a slight west to east lateral diversion due to the dipping of the strata of flow in the unsaturated zone that tends to concentrate by about a factor of five or so the nuclides in that particular zone.

Okay, where was I? What's going on in the saturated zone is we have two things. It's being transported laterally in the saturated zone, and there's some potential for dilution, albeit small, in the saturated zone. Dave will go into more detail on the saturated zone model, but essentially we have these break-through curves which are now going into concentration as opposed to mass release, and this is just for a unit release at time zero, what is the arrival time, if you will, for different nuclides in the saturated zone, and given that unit release, what would be the concentrations in the saturated zone.

Those are then used as input to a prediction of saturated zone transport, which I thought was going to be this slide, but in fact I redid a neptunium from the unsaturated zone, so I don't have a plot of the concentration in the saturated zone as a function of time, but that's essentially what's being calculated, is concentration versus time in the saturated zone based on curves such as I just showed you.

BAHR: What is the magnitude of your unit release for the--

ANDREWS: That was one gram per year. One gram per year was released. Gram per year times number of years. So it's a constant release rate.

BAHR: Okay.

ANDREWS: Going into the biosphere, you've got a lot of things happening there, different uptake mechanisms, different dose pathways to the average individual, and what of course results is a dose conversion factor that takes from concentration to dose, and then we get results. So now we're on slide whatever I said we're going to be at when we got to the results, 52.

So this is the expected value, single realization mean parameter set results, and I'm going to look at it in three different time slices, 10,000 years, 100,000 years, and then a million years, and walk through what caused these results.

Over the 10,000 year time period, remember I had that initial package that failed at about a thousand years. My other packages started failing at 4,000 or 5,000 years by pits and started failing by 15,000 years by patches from that one package failure. Nothing obviously is transported until the package has failed.

So this initial break-through, if you will, is being driven by that initial package failure, that premature failure, if you want to call it that, or unknowns that caused something to fail early.

This little dip here is the 5,000 year climate change causing there to be an increased flux. When I have an increased flux, I have increased flow through the unsaturated zone, and things can dilute, in fact, in the unsaturated zone, because of increased volumetric flow in the unsaturated zone. I also cause increased package failures and increased flux into the drifts, so there's a slight, once I've gone past that little downward trend, there's a slight increase after that has occurred. So that's what's causing this little waviness right there. I'd point out that the only nuclides occurring over the first 10,000 years are the very highly soluble nuclides, technetium and iodine.

The trend, as you can see, at 10,000 years is upwards, and when we look at 100,000 years, you'll see how upwards it is. We get this plot. Again, single realization. Mike is going to walk through a range of uncertainty off of this single realization, and the speakers after him are going to talk about sensitivity off of this single realization.

We still see for the first 40 or 50,000 years technetium and iodine are dominating. This pattern here that you see of technetium and iodine is totally driven by the rate at which packages fail, the rate at which packages come on line. Our time step in this is I think 300 years, so each 300 years, there's a certain number of packages, and it varies from time step to time step a little bit, not much, but that little bit of variation from time step to time step is what is causing all of that structure in the technetium and iodine dose.

Once I get beyond 40 or 50,000 years, now neptunium is coming out. It is slightly sorbed. It has a low solubility, and it's delayed with respect to iodine and technetium, which come out pretty quickly. They're not delayed at all. There's no sorption of them in any of the units from the sediments, back through the tuff aquifers, back through the unsaturated zone, and it ends up being the dominate dose contributor over the 100,000 year time period, peaking over 100,000 years at about 5 or so millirems per year. But it is still climbing.

This plutonium here is the irreversible plutonium colloids. Plutonium by itself has a very high sorption, so it only migrates at sufficient distances as a colloidal particle. So this is colloidal plutonium starting to come out here.

And then finally, at a million years, we saw that increasing trend going on after 100,000 years. That trend continues to increase. More and more packages are failing. More and more fuel is being exposed. Neptunium is still the driver out here. You have to get out to 300,000 or 400,000 years before this plutonium, colloid plutonium starts showing at least some structure. The humps and valleys are all due to the climate changes, the quick climate changes, either more water gets on the packages, more packages have failed and more water can get into the packages, so you have an increased bump causing some of that structure out there. There's always a little time phasing of when the actual peaks occur because of different sorption characteristics of the rock for the different nuclides, but they superpose to give this as the total. So you can see the peak in this particular realization is about 300 millirems per year out at about 300,000 years.

WHIPPLE: A quick question?

ANDREWS: Yes, Chris?

WHIPPLE: On that million year scale, are you still getting any benefit from cladding out in the many hundreds of thousands of year time period?

ANDREWS: It doesn't really impact us. I mean, everything is being driven by--you'll see when we get to the cladding sensitivity--everything is being driven by neptunium, and the neptunium is a function of the surface area, not so much a function of the surface area exposed as it is the solubility of the nuclide and how long it's releasing from any given package.

WHIPPLE: The water is saturated?

ANDREWS: The water is already saturated, yeah.

WHIPPLE: Okay.

ANDREWS: And it stays saturated for a very long time.

Okay, I think this next word slide summarizes what I just said in terms of the time periods and what are the dose contributors over those different time periods.

Now, if we have time, it's maybe worthwhile to try to walk through as an example, I think, of what drove this result, in particular looking at the 100,000 year result. We could do the same thing for a million, but let's look at the 100,000 year result. And let's look at some of the key factors.

The percolation flux, the average percolation flux ends up being a key factor, and its integral or sum over all the repository block ends up being a fairly key factor. Because it's the long-term average climate that dominates, we can look at just the long-term average climate. Those little time windows in there where it's dry are not driving the performance. It's the long-term average which is there 80 per cent of the time.

The seepage flux is a small fraction of the total volumetric flux. That we showed on that one plot. The rate at which packages fail and the cumulative number of packages which have failed over a particular time window ends up also being important. The rate at which it fails is important for technetium and iodine. The cumulative number over a particular time window that you're interested in ends up being important for neptunium.

Neptunium solubility ends up being important for neptunium, not for other things. The waste form surface exposed, which is this cladding kind of per cent, at least for the first few tens of thousands of years, ends up being important for technetium and iodine, but not for neptunium, and you'll see why. I won't present it, but somebody else will present it later. The waste form dissolution rate ends up being important for technetium and iodine, not so much for neptunium, because it hits its solubility control.

The EBS seepage flux, which is that percentage of the flux which gets into the package, which is a function of the package surface area exposed, ends up being important because it's going to be this number times this number that are going to control, times the number of packages that have failed, that are going to control the actual neptunium release from the engineered components into the saturated zone.

The dilution factor in the saturated zone is going to be important. It's a range of from 1 to 100 right now, and ten is the mean of that, or the mid point of that distribution. And then the dose conversion factor will end up being important.

And if you can just bear with my arithmetic, I guess, a little bit, for neptunium, very long half-life, so it's not decaying appreciably in the time frames that we're looking at, especially over the 100,000 year time period, it's not decaying. Its inventory is roughly 10 curies per package, or 15 kilograms per package.

The release rates are driven by that solubility and that volumetric flux that got into each package that had a hole in it. So this is the release rate per package. The total release rate over at least this time period, I'm just looking at the 100,000 year time window, is roughly 2 grams per year per the whole repository. I'm going to dilute that first in the unsaturated zone flux, the total flux through the unsaturated zone during a long-term average, and I'm going to dilute it in the saturated zone slightly, and then I'm going to multiply by the dose conversion factor and calculate 5 millirems per year.

So I think, or I hope you can see that the keys are solubility, the flux, both the seepage flux into the drifts and the seepage flux into the package, the flux through the unsaturated zone, and ultimately the dilution in the saturated zone. So all of those are key components, and when Mike walks through the distribution of results and what drove the results over a range of different parameters base, you'll see these, with the exception of neptunium solubility, which didn't come out as an important parameter for some reason.

So kind of as a lead in to the following talks, I put this slide back up because although we've walked through a case, an expected, if you will, mean value parameter case, there is uncertainty in almost every parameter and in almost every model, and what we want to do is walk through in the next seven hours, whatever we have today, the sensitivity associated with at least some of those key components, maybe not all because we didn't have time to produce slides for all of them, but we will look through most of them. So a one off in some cases, and all of these things are varying, with the exception of dilution from pumping, in the multiple realization cases that Mike is going to talk about after the break.

So in summary, what I hope I've done is first describe conceptually what's going on in each of these boxes. So break the system apart and then describe conceptually what's going on in each. And then describe what's in each of those boxes, what's in the base case for a single realization. We conducted a simple back to the envelope just to see roughly did it match our expectations, and the future talks will address first these three components, and then seepage and thermal hydrology, and then we're going to hear about waste package degradation and then we're going to hear about near field and waste form processes and then we're going to hear about UZ, SZ and biosphere. So that's how we've broken out the more detailed talks that follow.

So with that, I'll stop and try to answer any other questions.

BULLEN: Thank you, Bob. I think we'll take about five minutes here to just ask a few questions, and limit it to the Board and the Panel. Nelson, Board?

NELSON: I want to thank you first of all for information presentation which will require a whole lot of pleasant digestion, I hope, or I anticipate. But I've got two questions that it's perfectly fair game to say they will be covered later in response.

First is, I guess relating to the including of the cycles to the present day drier model, you referred to that not being a critical driver in the results of your analysis; is that correct? I guess two things out of that. One is are future analyses going to put less accent on that, and perhaps not include that cycling back to present day?

And secondly, I'm wondering, because I didn't see climate in the last biosphere sort of aspect, if we cycle down to a dry climate, would not there be different withdrawals, different biosphere interactions, and are those included in the dose calculations that you're doing?

ANDREWS: Let me answer the second one first, because that's more easy. We did look at, or the SCIC folks who are responsible for the biosphere part of what we've gotten, they did look at other different climates that would be equivalent to a times two, times three precipitation change in this particular region, and look at estimated kind of food consumption habits, water use habits in those other locations. And their preliminary analysis is that the dose conversion factors in those different regions are not dramatically different than the dose conversion factors we've been using based on the expected water use, land use, vegetation use, eating habits in this area.

So we've looked at it. Will we continue to do a sensitivity study on that latter part? Probably, because I think everybody will ask well, wait a minute, the climate changed, you changed everything else, why didn't you change the biosphere. So we probably will continue to address it.

On the first part, I didn't mean to imply that we won't continue to use some kind of cycling with climatic change. You know, whether or not we keep the cycling we have now, whether this is the most representative, you know, going into licensing, or a wide range of alternative patterns of climate change, you know, should it be more sinusoidal, for example, should we look at the durations and extend the durations of climate periods over different periods. Mike is going to show I think some sensitivity to different assumptions on durations of climate state to see whether it's important or not, and you can judge for yourself how important it is. But we have not done the sinusoidal variation of climatic change.

NELSON: Okay. Just one other thing. Some things were averaged spatially and some were not in terms of how it was included in the model. Are we going to be able to, based on what is present at VA, understand for example how the UZ source term to the saturated zone varies spatially? That will be part of the output?

ANDREWS: But spatially now is in those--what we're going to do in those six regions, so that spatially we will have, and then breaking it out by the different package types, you know, we'll look at those. We want to look at spent fuel versus DOE owned fuel versus glass, so look at the different waste forms and their different releases. We'll cut this in a lot of different ways, but we've only discretized spatially into those six, and then within those six, we've discretized it more by package type and waste form type to capture the right inventories. So, yes, you'll see some of that, but maybe not at the level of granularity somebody would like.

BULLEN: Jeff Wong, Board?

WONG: In relation to the biosphere, your exposure, was it constrained simply by the model that you chose? You chose to use GENNI-2. So were there--I'm trying to figure out how you structured your exposure scenario. So, therefore, I guess my next question would be do you have a diagram like you had on Page 15 for the exposure for the biosphere?

ANDREWS: Yes, I think Dave has one that shows, you know, how from a given concentration of water, the various exposure pathways that we considered, both inhalation, ingestion, and if it was ingestion, which exposure pathways we looked at, you know, the food chain, vegetation chain, et cetera. We weren't constrained, I don't think. We tried to use what from the site survey were reasonable eating and water consumption habits of people living in that area.

WONG: So you have it broken down by pathway, what would be the greatest contributor to dose?

ANDREWS: Yes, we could. I don't know, we haven't looked at those, but I think we could.

WONG: Okay, thanks.

ANDREWS: Not which organ--

WONG: I'm talking about the intake.

ANDREWS: Yes, and then we could look at that.

BULLEN: Well, thank you very much, Bob. Oh, Steve, do you want to do a quick one? We've got coming attractions, and I'll keep us on schedule. A quick one; two minutes.

FRISHMAN: Yeah. You didn't look at the sensitivity for drips onto the waste package and thermal hydrology, and you have the range of seepage fractions and percolation flux. What happens if you look at the analog that we had looked at, the Papoose Lake Sill, that sets up a situation where you actually have a preferred flow path back to the heat source, and you have plugging in other areas, so you actually have funneling of infiltration back to the individual heat source. That drastically changes your whole picture. Have you looked at that paper and tried to figure out whether it has any significance to what you're seeing as fracture flow after the thermal impulse?

ANDREWS: We haven't incorporated that in any model.

FRISHMAN: I think you ought to take a look at it.

ANDREWS: Yeah, it might be worthwhile to look at focusing of flow. That of course is going to imply, given that I have the same volumetric flow moving through this system, I'm just redistributing where that volumetric flow is.

FRISHMAN: Right, and it's going to come back to the packages, at least from what that analog indicates.

ANDREWS: It could, or it could go between--

FRISHMAN: Well, the suggestion is it come back to where the heat originated, because of the dynamics of the reflux while the heat is rising and then dropping again, and plugging of fractures, because you're running hot fluids through.

ANDREWS: We need to make a point of looking into that. Yeah, I agree.

FRISHMAN: I'd take a look at that, because that's the thing that I found most interesting in that work, and the one analog we have says that the situation is very different from what you're using as a base case.

ANDREWS: That could be, yes.

BULLEN: Paul Craig, last comment.

CRAIG: Craig, Board. Well, I've been looking at this from the point of view of how it's communicating, and I think you've done a remarkably good job. There's a whole lot of information in there. I obviously have lots of comments, but it does detail. It was really nice. Thank you. You've really put a lot of effort into it and it paid off.

BULLEN: Okay, thank you very much. We'll now take a 15 minute break. Why don't we try and get back here by 25 after.

(Whereupon, a brief recess was taken.)

BULLEN: Could I ask everyone to grab a cup of coffee and take their seats, please? We'd like to reconvene.

I'd also ask the Board members to come up front, please, and take their seats.

Before we begin the next session, I'd like to remind the Board members and the Panel members to speak into the microphone so that we can get an accurate transcript of what goes on in this meeting.

Our next presentation will deal with the uncertainty analyses that have been completed to date on the TSPA-VA, and the presentation will be made by Mike Wilson from Sandia National Laboratories. Mike is currently a principal member of the technical staff working on total system performance assessment for the Yucca Mountain Site Characterization Project, and he's going to provide us with an overview of the sensitivity analyses completed to date.

Mike?

WILSON: I've had trouble with my voice the last couple of days, so I need this microphone.

Okay, I'm going to talk about uncertainty and sensitivity analysis of the probabilistic trends we've made, and you'll notice in the talks to come, we've come up with little icons to put on each one of them. So I made up my own for uncertainty analysis.

I want to give some credit where credit's due, the people that were actually doing most of the work that I'm going to talk about.

First, a quick run-down of what uncertainty analysis is. It's a method of quantifying the uncertainty in the releases. And the way we do that is my a Monte Carlo analysis or Monte Carlo simulation, which means that we assign probability distributions to uncertain input parameters. We run them through a set of models and at the end, you get out a probability distribution of the peak dose, or whatever other performance measure you're interested in.

Typically, you'll see that we present this probability distribution of the outputs in terms of a CCDF, a complementary cumulative distribution function, on a log-log scale which tends to really amplify the small probability high release tail.

As Bob already mentioned, the performance measure that we're using is the peak dose at 20 kilometer distance, 20 kilometers downstream from the repository, and we've been concentrating most of our attention in the analyses on 100,000 year periods, but sometimes we focus in on a 10,000 year period and sometimes we look at longer, million year periods.

This is an example of our base case results for 100,000 year period. First of all, there's--this was a 100 realization run. There's only 80 plots here, not that you'd ever be able to tell that. There were 20 realizations out of 100 have zero releases in 100,000 years because of low corrosion, low amounts of water hitting the containers. So this is the other 80, and you can see that while the kinds of expected value realizations that we tend to show a lot, like Bob showed a lot of, are all well and good, the fact is there's a wide variety of behaviors that our system can have, depending on the combination of the uncertain parameters that are sampled.

The next step then is to look at the CCDF of a peak dose, and that means for each one of these curves, you take the very highest point, that's the peak dose, and you take that point and make it part of the distribution. So here's one of the peaks up at this level. That's the very highest one in this run. Here's one of the peaks over here. There's a lot of different places where they peak and a lot of different amplitudes, and this is what the distribution looks like.

This curve right here represents those curves that I just showed up there. It comes to the axis here at the point eight level because there were 20 per cent of the realizations that had no releases. The highest one is up at about 1,000 millirems per year. There's a median. The median is about here at .1 millirems per year. The mean is considerably higher. It's over here at about 30 millirems per year. When you have this kind of a distribution that has a long low--the mean is always much greater than the median.

I've also shown on this plot the results if you took a 10,000 year period or a million year period, and looked at the peak dose over those periods. The 10,000 year period has even more zero dose cases, and it has much lower peak doses, as you'd expect, as Bob showed, pretty much always. And as you can see from the plot I showed previously, the calculated doses are generally increasing rapidly at 10,000 years. So most of the peaks are well after 10,000 years.

And once again, if you look at a million year period, you usually have quite a bit higher peak doses than you had for the 100,000 year period, because a lot of the things like neptunium are still increasing at 100,000 years.

I'm going to show a lot of different ways of looking at the data. This shows the contribution of the most important nuclides to the peak doses. For a 10,000 year period, the doses are basically entirely by technetium, iodine and Carbon-14, because those are the fastest radionuclides. They all have very high solubility, and they are non-sorbing. So they travel through the system fast.

For the 100,000 year period, you get a large contribution from neptunium in addition to the iodine and technetium. Basically, you have about a third of the peak dose on average is from neptunium, a third is from technetium, and then the other third is the zeros and a little bit of the other things.

One thing that's interesting and is different from the kinds of analyses we've made in the past is that there is a small contribution from plutonium here, and that's because for the first time in the TSPA, we're trying to model colloidal transport of plutonium.

I don't have a pie chart of the million year results, simply because they only came in a few days ago, and it was too hard to try to redo all these plots. So I put million year results in a few places, but most of my discussion is going to be on the 100,000 year period, and that's kind of where we want to focus most of our attention anyway. The million, that's getting out to a really long time.

This shows more detail of those pie charts, or of the 100,000 year pie chart. This shows the distribution of the contribution of nuclides to the peak doses, and you can see, for example for neptunium, there's about a third of the time where neptunium is almost all of the peak dose. The shape of it is quite a bit different from these other ones. And then you can see here that a few per cent of the time, Plutonium-239 contributes most of the peak dose.

I kind of like scatter plots. This shows the scatter plot of the time of the peak dose within 100,000 years, and the value of the peak dose. And I think it helps to explain things a little bit. For one thing, you can see that there's a number of very early peak doses, even before 10,000 years, and those are basically caused by the juvenile failures that Bob talked about.

Then you have a trickle of failures throughout the whole period, and then you have a big cluster of peaks between 90,000 and 100,000 years. Those are, for the most part, caused by the change in climate. We have a change in climate from this long-term average climate, back to a dry climate in the range of between 80 and 100,000 years, and the change in climate often causes peaks in the dose curve, so you tend to get peaks there.

And then there's a number, quite a few of the peak doses that are actually right at 100,000 years, indicating that it's not a real peak at all. It's still increasing at that time. And I want to call attention to this little pink dot down here in the corner. That represents the zero release cases that are actually off the scale, and I've assigned 100,000 year time to them. They have no peak really.

I also wanted to mention that these early peaks are the ones that are dominated by technetium and iodine, and these late peaks are the ones that are dominated by neptunium.

This shows a plot like that for the million year simulation, and you can see that there are very few actual peaks below 100,000 years. Almost all of the million year peaks are at later times, and in fact they're clustering around these two times, which are the times of our super pluvial climate in the model. So the super pluvial climate is what is determining the peak dose over a million year period.

And on the million year period, they're almost all dominated by neptunium releases, though once again, there is a small, a few per cent of the cases that are dominated by plutonium colloids.

Moving on to sensitivity analysis, there's a lot of--when you have 100 release values and you have all the input parameter values that go with them, there's a lot of analysis you can do to try to correlate different things, and it can tell you a lot of useful information.

The uses of sensitivity analysis are, one, to actually rank the input parameters, the uncertain input parameters, according to their effect on the peak dose, and that tells you what parts of the system or what models are the most important, and perhaps need more attention or more data, or whatever. And that gets to the second bullet, which is it can help to guide future model development and data acquisition. And they can also be helpful, these kinds of analyses can also be helpful in making sure that things are consistent. We have a linked set of a number of models, and data is transferred from model to model, and doing consistency checks helps us to be sure that that's being done right.

Some of the kinds of analyses you can do with Monte Carlo output are, first of all, look at scatter plots. That's a nice visual indication sometimes of whether a parameter is important or not. By a scatter plot, I mean plotting the peak dose value against the value of the parameter. And if you can see a trend, that means that that parameter is having a strong influence on the peak dose.

Stepwise regression analysis is a way of quantifying that. It actually fits a surface, a planer surface or a hyper-planer surface, to the relationship between the peak dose and the parameter values. We generally use the rank values when doing this rather than raw values because that works better when you have non-linear effects. By ranks, I mean the ordering. You take the lowest value and assign it one, the second lowest value and assign it two, and so on like that.

So if you have any kind of an increasing or decreasing function, the ranks will represent that very well. If you have a function that goes up and then down, the rank transformation won't help you much.

You can rank the influence of variables on the outputs by a number of different measures, including partial correlation coefficient, standardized regression coefficients, and the contribution to the variance, which is delta R squared. We're putting most of our emphasis on the PCC, or partial correlation coefficient, because that is better when you have correlations among your input parameters. If you have things that are correlated, like for example the percolation flux in our model and the seepage flux in our model, and you do the regression on both of them, the fact that they're correlated can confuse the system sometimes, and the partial correlation coefficient is a way of taking that correlation out and ranking them on their impacts with the correlation to the other parameter taken out.

Another way of doing sensitivities, not with the Monte Carlo results, but aside from that, is to look at single discrete cases in which you change a parameter or change a model to some alternate conceptual model. And I'm not going to have any of those in my talk, but there's going to be a number of them in the following talks.

It's also very helpful to look at not just the final dose value at 20 kilometers, but the sub-system values, the release from the waste form, they're released from the waste package, they're released from the EBS, they're released from the unsaturated zone, and that helps you to see which of these different variers are more important and which of the parameters associated with them are important within their own realm. And lastly, looking at how the partial correlation coefficients varied over time gives you some interesting information.

This shows the results of a rank regression analysis of the peak doses over a 10,000 year period against all the input variables. And there's over 100 input variables. Some of them we've lumped together and done different things with so that there's actually 99 variables done in this regression, and what we find and what will keep coming out through all of this is that the fraction of waste packages contacted by seeps is what shows up as the most important variable to the peak doses. And that is primarily because of its influence on the waste package failure.

In our waste package failure model, the packages fail much faster if they have liquid water dripping on them than if they're dry.

The second one is also waste package related; the mean corrosion rate of the inner C-22 layer. That has a very high correlation with the 10,000 year results, and the fact that 27 per cent of the realizations had no releases at all within 10,000 years is very much related to these two parameters. When you get no releases within the 10,000 years or the 100,000 years, it's in realizations that have very low corrosion rates or very low seep fractions.

Then the 10,000 year results are very related to the juvenile failure fracture, or number of juvenile failures also. A lot of the times that is what dominates the 10,000 year results, because most of the corrosion failures haven't occurred yet in 10,000 years.

Then the saturated-zone dilution shows up as having a significant effect on the dose, which you would expect. It basically is directly related, or inversely directly related to the dose.

And then lastly, above the cutoff, there's really 99 variables on this list, but we want to cut it off at some point. When you get to the partial correlation coefficients that are very low, and somewhere around here is where a reasonable cutoff is, you start just getting spurious results at some point, but these are all ones that are sensible and are not spurious.

Percolation flux is on there, and the thing that's really interesting about this is it has a negative correlation with the peak dose in 10,000 years, that is, higher percolation fluxes tend to have lower peak doses. And the reason for that is once again, related to our corrosion model, and something that is perhaps not intuitively obvious, is that when you have higher percolation fluxes, that tends to cool the repository. You get a lot of cooler water infiltrating and making the temperatures lower. The lower temperature means lower corrosion rates, and the waste packages take longer to fail.

Of course higher percolation fluxes also tend to mean that a lot more waste containers are getting wet. But that effect is taken into account by the fact that this one is way up here.

Now, the same list for the peak doses in 100,000 year period looks fairly similar. The fraction of waste packages contacted by seeps is on the top again, even a little bit higher partial correlation coefficient. The mean corrosion rate of C-22 is still number two.

Another one that shows up is the corrosion rate variability for the C-22, where for the purposes of doing the modelling and regression analysis, we're parameterizing the effects of the C-22 corrosion by these two parameters. We set up a matrix of runs in which we have--each realization has a range of corrosion rates, but the width of that range is varied according to this parameter, and the mean value of it is varied according to this parameter. And it really isn't surprising that both of those are important to performance, because they both affect especially the number of early failures, and up through the first half of the curve.

And then the number of juvenile container failures is still showing up even for the 100,000 year case, and I think that's mainly because quite a number of the peaks are at early times, as you saw in the scatter plot earlier.

Just because I like scatter plots, I'm going to show you two more, the top two parameters in that 100,000 year list. This is the scatter plot of the peak dose in 100,000 years against the seepage fraction, and LTA stands for the long-term average climate, since that's what we kind of focus on since that's the climate that occurs over almost all of that 100,000 year period. And you get a nice triangular distribution of the dots here, and the implication of that is that when the seepage fraction is low, that pretty much always means that the peak dose is going to be low. But if the seepage fraction is high, it's still possible for doses to be low because of other factors, because the corrosion rate is low, because this and that and the other. And, in fact, you can see that the zero values are distributed all across this range. Obviously for the zeros, the seepage fraction has no influence. Those are ones where the container hasn't even failed.

And this shows the scatter plot of that mean C-22 corrosion rate against the peak doses, and you can see we're doing it in a discrete fashion. What we've done is to come up with a matrix in which we varied for three discrete cases the C-22 mean corrosion rate, and in three cases that variability width. And what these are is they're taken from the non-discrete distribution of corrosion values. For the purposes of picking the cases, they took the fiftieth percentile value from the distribution of C-22 corrosion rates, and the fiftieth percentile value and the ninety-fifth percentile value, and you can see that there's a wide range for all three of those, but the low corrosion rate doses tend to be clustered at lower values, and the high corrosion rate doses tend to be clustered at higher values, and that's why you get that good correlation in the final results.

And then I've also done this for the million year period, and it's getting more dominated. The performance is getting more dominated by a few containers. We're kind of down to the basics here. Basically, the fraction of waste packages contacted by seeps is dominating the release rates from the waste packages from the repository. The saturated-zone dilution and the biosphere dose conversion factor, it's kind of all collapsing down to those three important things.

The correlation with the seepage fraction is even higher over the million year period.

And then this is another example. I've shown you three, the results of the PCCs for three different time periods. Now this is another way of looking at the variation with time more--not so discretely but, you know, varying smoothly. This shows the partial correlation coefficients of some of the most important parameters, with the doses at a given time. So this point, for example, represents the correlation with the dose at 10,000 years and not with the peak dose over a 10,000 year period.

Before, we were looking at the peak over this period, the peak over this period, the peak over this period. Now we're looking at individual times and looking at how the correlation varies over time. And I think the most interesting thing you see is that the juvenile failures have an extremely high correlation with the dose for the first few thousand years, and then they drop off. It's not surprising, but it is confirmation that things are working the way they should.

And you can see that it takes a few thousand years for the influence of the seep fraction and the mean corrosion rate to built up, but then they remain high over basically the whole million year period. And the dilution factor in the saturated zone also stays fairly high, though it becomes less important around 100,000 to 200,000 years, and then gets more important again when you get out to a million years.

To summarize, I think it's important that there's a wide range of behaviors of the system. You can see that from the horse tail plot I showed at the beginning. You get dose curves that are covering a large range of values, and the shapes of the curves are also very different. For 100,000 years, most of the peak doses occur after 80,000 years, and a lot of those aren't even really peaks. They are representing cases where the dose is still going up.

The ones that are local peaks are caused by the change in climate at that time. And you get some peaks that occur even before 10,000 years because of juvenile failures.

For a million years of simulation, you could see very clearly on that scatter plot that most of the peak doses are associated with the super pluvial climates. At early times, the doses are dominated by technetium and iodine. At late times, they're dominated by neptunium.

A few per cent of the time, plutonium colloids dominate the peak dose, and the most important uncertain parameter depends on what time period you're talking about. And for a 100,000 year period, they are the fraction of waste packages contacted by seeps and the C-22 corrosion rate and its variability and the number of juvenile failures, all of them waste package related things.

BULLEN: Thank you, Mike. Questions from the Board? Paul Craig?

CRAIG: Craig, Board. I'm trying to relate the summary section to the Figure 4, which what it looks like with a four year old with an etch-a-sketch. I wonder if you could pick out some of the--

WILSON: This one?

CRAIG: Yeah, I wonder if you could pick out some of the weirder curves and explain what in the world is going on there?

WILSON: Weirder curves? Well, here's one that you don't start to have any releases until about 90,000 years. That's a realization where the corrosion is probably low, so that there aren't any--and it must not have a juvenile failure, and the corrosion rate is low, so you don't get any failures until then.

CRAIG: Now, the really weird ones to me are the ones that oscillate.

WILSON: Yeah, you get all kind of weird oscillations, and that's really--I think Bob was trying to explain that earlier. You are actually seeing individual waste package failures in some of these curves. You get one waste package fail, and you get a pulse of releases out of it, and then that pulse dies down, and then another one fails, and you get another pulse. And that's representing a lot of this oscillation, if not all of it. More? I'm not sure what to say beyond that. It does make some of the curves very complicated.

BULLEN: Nelson, Board?

NELSON: There's a lot of input parameters that go into the fraction of waste packages parameter that you've been investigating here. To what--I mean, I can imagine there being the input of the percolation and the climate and the heterogeneity, the spatial variability. To what extent can the TSPA-VA results be used to investigate some of that behind this factor which is some important, the input parameters behind that factor, so that we can understand what those important assumptions are that are affecting the results?

WILSON: Well, it's difficult to do that with these results because they have been rolled together into these two parameters, the seep fraction and the seep flow rate. We are doing some additional analyses where we varied the parameters that went into those so that the distribution of those two parameters is different. And that will give us a little bit of insight on it. I don't have anything like that to show you at the moment. But as you say, there's a lot of things that go into it. The things that we are actually looking at variations of are, for example, the fracture permeability variation around the drift and the fracture apertures basically. We're doing variations on those, and there will be a little bit more discussion of that in the talk on seepage later on.

NELSON: Just as a quick followup, how important is the mapping into six zones important?

WILSON: I don't think that has a big influence on the results. We haven't done any analysis to specifically address that question. But, in fact, over the last few months, a lot of what we're doing has collapsed down. For example, in the waste package modelling, after a while, it became clear that the waste package failure curves for those six regions were essentially the same, and so in these final results, we're only doing the calculations once instead of six times, and that's true in a few other areas. Like the temperature variation between the six regions isn't that great. There's some difference and that will be shown later, there's some difference, but not a huge amount.

There is a pretty big difference in the amount of percolation or infiltration in those regions, and you would think that that would be important, but it's not showing up as a really big deal.

NELSON: I guess that's the kind of insight I was thinking I was going to see. But that's fine.

WILSON: We haven't done that much analysis of the individual regions yet. All of this I'm showing is for the six lumped together. We do have results for the six individually, and we intend to do some analysis of that, but we haven't really yet.

BULLEN: Sagüés, Board?

SAGÜÉS: Yes. Concerning the variability of corrosion rates, presumably you have cases with different corrosion rates or corrosion performance, say for the corrosion resistant material, does that take the form of, say, an average corrosion greater than some sort of a statistical deviation assumed for it?

WILSON: Right.

SAGÜÉS: And I've just finished with a durability investigation with a large number of corrosion rates, and much to my surprise, the data were beautifully distributed in a lognormal distribution, for example, and that makes of course a big difference if it is a lognormal or if it is just plain normal distribution, and the like. How are those corrosion rates distributed?

WILSON: I think it would be better to leave that to Jerry's talk later on. The basis that we're using for the corrosion rate distribution is the expert elicitation that was done, and I can't tell you specifically what the shape is, but maybe Jerry can. It just looks like a distribution to me. I haven't seen a picture of the PDF. I've seen CDFs and it's always kind of hard to tell exactly what it is with a CDF.

SAGÜÉS: Okay. So you've given a distribution curve from there, and that's the one that you're plugging into. Okay. The other question was I didn't see a cladding performance as being identified as an important variable. But looking at the previous presentation, it looked like the corrosion performance of the cladding appeared to have had a big effect. Was that not implicitly included in what you were doing?

WILSON: Well, that brings me to something that I meant to say actually, and that is that these kinds of sensitivity analysis results depend not just on how important the parameter is, but also on what the uncertainty is. And it turns out that with the cladding model we have right now, there's not that great of a spread of uncertainty in it. Whereas, these ones that are showing up as really important are things that have very wide ranges of uncertainty, and that is a big part of the reason that they're showing up as so important.

SAGÜÉS: Thank you.

BULLEN: Paul Craig, did you have one more?

CRAIG: Yeah. I think it actually relates closely to that. It has to do with trying to figure out what's important. You mentioned that there were 99 variables that you looked at. When we go back to Bob Andrews' Page 16, he had a list of 20 parameters, and I'm trying to understand how to relate the 20 parameters on his list with the 99 on yours, because his overview on it is what one would hope would pull out the parameters that are really important. And maybe there's a way to relate these two pieces of information.

WILSON: Well, I think basically what it comes down to is that out of those 99 parameters, 94 or 95 of them just don't matter that much to performance, and you could get the same results if you just assigned them a single value instead of sampling them from distributions. That's something we don't necessarily know at the beginning, so we need to do this with all the sampling first, and find out how important the different things are. But, yeah, if we go forward, there's some of these that we could probably reasonably not bother to vary in the analysis. The kinds of things Bob was listing were the things that we think are the drivers of the performance, and the most important ones. But there's all kinds.

For example, in the fuel degradation model, it has a dozen parameters related to the rate of the fuel degradation, and the uncertainty ranges of those is based on the data that we have. But it doesn't affect the final results that much. If you were to do a regression on the actual releases from the fuel, you'd probably see those coming out as important. But by the time you roll it through so many steps out to dose, it doesn't matter that much any more.

BULLEN: Chris Whipple?

WHIPPLE: Mike, a couple of questions about the difficulty of doing the uncertainty analysis in a system that's this complex where you've got a mixture of different sub-models with varying degrees of conservatism versus realism, and it picks up on the previous question about why didn't cladding show up. I'm concerned that by how you define the width of the range you sample over for any one parameter determines your answer for a number of these factors.

I can give you another example that intuitively didn't make sense to me, which is how biosphere parameters could be more important than neptunium solubility. And it may well be that you had neptunium solubility sampled over a one or two order of magnitude range and some aspect of the biosphere sampled over a three order of magnitude range and that's why you got what you got. There may be other ways of looking at sensitivities. For example, just a simple derivative, how does the dose rate change with a 1 per cent change in the inputs? That is a more meaningful valuation of the sensitivities than using a Monte Carlo where you can hit the ends of the boxes on the parameters and show little sensitivity, and incorrectly conclude, I think is the case with cladding, that it's a comparatively insensitive performance parameter.

WILSON: Well, there's certainly truth to what you say. However, I guess I would argue that what one of the primary things we're after is where our uncertainty is greatest, and where that uncertainty affects the final results the most. I think maybe the argument would be whether we have included all artful uncertainty in the cladding model or not.

BULLEN: Steve Frishman?

FRISHMAN: You see these peak doses coming out at around 100,000, and you can see that it--well, from Andrews' thing, you can see that on neptunium and technetium, that it's actually just a spike. Some of yours show that it's just a spike. And that's only there because you've imposed this climate change at 90,000 and put a 5,000 year present in, and then you go back to another 90,000 long-term average. So all of your peak doses and all of this analysis, even with all the uncertainties you're finding, there's a great big uncertainty on top of that, and that's what are you imposing as a climate regime. And how do you get at the extent to which this sort of simplistic imposition of a climate regime is anywhere near reality, or what is the uncertainty of the regime that you're imposing on the system? Because you can come out with peak doses at 60,000. You can come out with peak doses wherever you want to put the climate changes.

WILSON: Okay. Well, I should let Jack talk more about that later on. But in fact, in terms of the sudden jumps in climate, I don't think that's unreasonable. As to whether we can rule out a 60,000 year climate change as opposed to a 90,000 year climate change, I don't know that that matters. But I don't think the time of when the peak dose is is that important to a regulation. I think it's important to understanding our results, and that's why I showed this like that.

FRISHMAN: Well, these peak doses are spikes and they show up as spikes. And from thinking through it, it looks to me like the spike is because of resuspension, and if that's the case, then if you were dealing with the repository releases only, you would see an entirely different structure. In fact, you see the releases go the opposite direction in those normal spike areas.

WILSON: Sometimes in our results, or a lot of times, when climate goes to a wetter climate, the doses actually go down, and that's because of an increase of dilution. You're not going to see that in the EBS part. In the EBS part, if it gets wetter, you're going to have higher releases.

FRISHMAN: Well, I guess what I'm looking at is you're getting your peaks primarily from these little spikes and, you know, there's a big peak imposed on top of the whole thing that is still a function of the climate. And I guess the question is, and I don't see you doing it all, how are you dealing with the uncertainty in the base case climate regime that you're imposing? And it sounds to me like you're not.

WILSON: The uncertainty in climate that we're including is only in the duration of the different climate types.

BULLEN: We've got Jack coming up to talk on climate right after this, so maybe you can repose the question at that time.

FRISHMAN: Okay. Well, I was just trying to say--

WILSON: If you're saying we should look at other functional forms instead of steps, then okay, but I'm telling you what we're doing is steps and varying the duration. That is the amount of uncertainty that we have included.

BAHR: I had questions along the same line, and maybe I'll just wait for the climate talk to ask those.

One comment just sort of related to transparency. You had correlations versus time and you were correlating the peak doses at a particular time with some parameter. Shouldn't you be looking--if any of those parameters have some sort of a temporal variability, shouldn't you be looking at a time that accounts for the transport time from the canister to your 20 kilometer distance? Because the timing of the peak dose is not going to be coincident with the timing of your release. Maybe that doesn't really matter in the--I think the one, the juvenile failures or the seep fraction might be the--you're really looking at seep fraction at a particular time, and then peak doses, or doses, at that same time.

WILSON: This is showing--

BAHR: The seep fraction is going to affect the rate of release at the repository. The dose 20 kilometers down gradient that's responding to that particular release is going to happen some thousands of years later.

WILSON: Well, see, these parameters here aren't time varying things. Those are fixed input parameters to the model, and we're looking at the correlation of the output value to the input parameter. Then the only time that comes into this is the fact that the dose changes over time. But those input parameters don't change over time.

BAHR: Okay. So none of those are things that have a temporal variability?

WILSON: This in fact is the seep fraction calculated for the LTA climate that this curve represents.

BAHR: Okay. So these were simulations without any climate change in them?

WILSON: No. No. They do have climate change in them. But this input parameter is a fixed input parameter, not something that varies over time. We could do the kind of correlation you're talking about with variables that change with time, but we have not done that so far. These are all fixed input parameters.

BAHR: Okay.

WILSON: They are input parameters that go into the model of something else that varies with time.

BULLEN: Any other questions from the Board? Staff? Leon Reiter?

REITER: Mike, maybe this is a question for Abe, I'm not quite sure. But when you present the base case, are you going to present the kind of deterministic results that Bob showed us or are you going to present some of these things? And the reason behind it is that often the mean is defined as the expected value. Are you going to--how are you going to show this as a base? What are you going to show in the base case?

WILSON: I'm not sure what you mean. In the document, we're going to show both. We're going to show it all.

REITER: Well, okay, but what's--the Congressional directive is to show the expected performance?

WILSON: You mean what is our definition of the base case?

REITER: Yeah. In other words, what's going to appear as the sensitivity test and what's going to appear as expected performance? Are you going to say, well, expected performance over 100,000 years is 5 millirem per year, or is it 30 millirem per year?

WILSON: Do one of you guys want to answer that?

VAN LUIK: Well, what we'll show--Mike is correct, we'll show both, because when we show the mean value case, we want to show the plots of the distribution that documents that that is the mean value case. We can't just make an assertion. And so it's our plan to show both.

WILSON: I think our working definition of a base case includes uncertainty in a lot of these parameters. So it's a probabilistic thing. But then we also single out this special set of parameters for extra attention, and show a lot of things related to that one.

REITER: Well, okay, it just depends on how you sell it.

BULLEN: Mike, thank you very much. We appreciate it.

Our next presentation will actually delve into the first set of models on climate, infiltration and unsaturated zone flow, and the presentation will be by Jack Gauthier.

Jack has worked on the repository performance assessment for the past 15 years, a significant contributor to both TSPA '91 and '93, and he is going to give us a presentation on climate, infiltration and unsaturated zone flow.

GAUTHIER: Well, first of all, I need to take this off. It's been bothering me all morning.

I'm going to talk about climate, infiltration and

UZ flow, and we've attempted--we tried to represent these with these icons up here. Basically, climate is an atmospheric effect. It's basically weather averaged over some period of time. Infiltration is a surficial effect. It's the water crossing the surface boundary here, and UZ flow is how that infiltrating water is distributed through the unsaturated zone, where the unsaturated zone is the region above the water table.

All right, climate first. Climate provides inputs to the infiltration model, the UZ flow component, the SZ flow component and the biosphere component. Through infiltration, it mainly affects UZ flow and SZ flow, but it also affects--we also put in provisions for water table rise, SZ flux, and then the biosphere, I don't have it up there, but we changed the irrigation rates.

We settled basically on three discrete climate states to look at. The reason for this is two-fold. First, climate is basically unpredictable. We didn't want to let on that we knew more than we really did. In this VA, we wanted to catch the broad brush strokes of climate.

The second reason is that we are using large, rather sophisticated models for other components, such as infiltration and UZ flow, and we couldn't run a large number of cases, we couldn't look at a large number of climate cases with these rather sophisticated models. That sort of limited us in the time allowed.

So the three climate states we looked at were what we call the dry climate, that's number one, and that's similar to the present climate there. The long-term average, to sort of give you some orientation, it's similar to Santa Fe, New Mexico, that climate that's there, and what we call the super pluvial, which is basically a worst case climate that we think we've defined there, and it's similar to the climate that's presently at Los Alamos.

One thing you're going to see, and it turned out to generate some comment with the NRC, is that we jump from one climate state to another through time, and we make an instantaneous jump there.

The other thing that I think is very important is that over 80 per cent of the time, we're dealing with long-term average climate, what we call long-term average climate. This climate is basically like a pluvial condition, for instance during the last glacial period, they had what's called pluvial conditions in the Great Basin, and that's what this climate is. So the results you've been seeing are pretty much dominated by the long-term average.

As for our basis in how we define these climates, the timing is based on the global paleoclimate record, and the magnitude is based on the local paleoclimate record.

The project has also done some computer modelling and global circulation models are in the news a lot nowadays, but we found that they had limited predictability.

So anyway, for timing, this is just one example. A lot of work has gone into these things. I'm just giving you a brief glimpse at them. But for instance this is a well known paleoclimate record. It's SECOR from around the globe, and presently we're down here in a rather warm climate. 20,000 years ago, there was an ice age, or a glacial climate. There is another interglacial, glacial, interglacial, glacial, and that proceeds sort of like this in this saw tooth fashion back through time, back through most of the Quaternary. It turns out, and I have them marked here, that these two pulses, these two glacial climates were particularly severe. It's not really apparent on this, but it's apparent in other records. So what we did was we just backed up here to about 400,000 years ago, and we started tracing out sort of an on again, off again glacial climate.

The interglacials are assumed to just be the troughs here, and they're relatively brief. So the first one should be over by now, but it seems to be dragging out. So we varied the time there between zero and 10,000 years. The average is 5,000. Then we jumped up to the long-term average, and Bob showed you that. We put in super pluvials to indicate that there are some conditions in the past record that appear to be very severe.

This gives you an idea of how we came up with the magnitudes for these climates. This is just a brief glimpse. Quite a bit of work went into this. They looked at--the USGS looked at packrat middens all around Southern Nevada and the neighboring states, came up with what sort of vegetation was growing there, correlated that vegetation with where it grows in the modern world, and attempted to back out what the climate was like to cause that vegetation.

And here, this is precipitation for over various periods. These are the glacial periods, and here's modern and here's the combination of the rest. And you see that the precipitation--well, it's not real obvious here, but it increased to about a factor of two during the last glacial maximum, and that's what we use for the long-term average.

So this is basically our climate model in this table, it's sort of a look up table. We have dry climate. The analog site is the site. These infiltration rates I'll talk about in a little bit. The duration is typically zero to 20,000 years, although as I say, for the first, the first duration is between zero and 10,000 years. Water table rise and unsaturated zone multiplier are as they are at present. Long-term average, double precipitation, we have an analog site here which turns out to be Rainier Mesa, right near Rainier Mesa. I'll talk about this.

Note that we only have uncertainty in these two areas here. We didn't have time to make these uncertain. Water table rise, 80 meters. This comes from evidence at the mouth of Crater Flat that springs were flowing there about 10,000 years ago, and also some strontium isotopes ratios in the Calico Hill seem to indicate that water table rise was between these values. And then we took the saturated zone multiplier from the regional scale model by putting in a glacial climate above it.

Super pluvial here, this analog is three times the precipitation, the present precipitation average. The analog site is a resort I think in the Sierra Nevadas, and I will talk about the rest of those.

So basically, what I want you to remember about climate is that we only use three states, and of those three, we mainly used LTA. So most of the results you see in the base case are based on a fairly wet climate. It was what was there during most of Quaternary., and then also that we have limited uncertainty.

Now, I'm going to tell you about some sensitivity studies we did. These are these one off sensitivity studies that Mike mentioned where we just compare one possibility with another possibility. I have to give you some disclaimers here. First of all, I've only had these results for about ten days, maybe even less. And in that time, I had to learn Power Point. And the other thing is I don't know everything. That should be obvious.

And then the final thing is that some of these results are going to be contradicted by results I show you for the infiltration sensitivity studies. However, be that as it may, there's some evidence that we--that the climate durations that we used in the base case might be wrong. This evidence mainly comes from lake levels in the area. The lakes appeared to last only about 50,000 years, not the 90,000 years that we would have predicted with our long-term average.

So what we did was we took all these climates and we made each one 50,000 years in duration so that the--except where we started in 5,000 years again for the LTA. But then the LTA starts in 5,000 years, goes for 50,000. Then there's another 50,000 dry, et cetera. And as you can see, what these results are telling us is that the duration of the climate is not really significant. This is the super pluvial here, these two are right near a super pluvial.

NELSON: Did you say you were using 50,000 year dry intervals?

GAUTHIER: Yes. Yes, except for the first one.

BAHR: Is your super pluvial 50,000 years also?

GAUTHIER: Yes. And I guess a corollary to what I'm saying here, is that what this plot is telling you is that magnitude is not important. In fact, maybe it's variability itself that's important. That would be one interpretation. And here is even more stronger evidence to support that interpretation. What we did was we ran the whole million years, but with only a constant climate, and we did this three times. We ran it with the present day climate for the whole time, the dry climate, the long-term average, and the super pluvial. And you see that there is some variability here at the beginning. However, it's a little counter-intuitive. The super pluvial actually peaks lower than the long-term average, and in fact, none of the peaks are much different than the present day dry. What's different is when they occur. Again, sort of an indication that the magnitude of climate is not important. Perhaps the variability, what gives you those little wiggles, is important.

Okay, let's go on to infiltration. Infiltration affects the UZ flow and the thermal hydrology components. It's the boundary condition for both of these. The model, I left out a bullet here, but the model is that basically a collection of 1-D models that look at--on 50 meter grids basically over the whole site. These 1-D models calculate water balance in the soil profile based on precipitation, evapotranspiration, permeability and storativity of this soil. Basically, it's a tug of war here. The water, once it reaches the surface from the climate model, evapotranspiration tries to pull it out. Permeability and storativity hold it back. Gravity and capillary try to pull it down.

Net infiltration, I think this is important how we defined it, it's the water percolation at bedrock, which is very near the surface, for instance along ridges, or a depth of 6 meters in deep alluvium.

These are the infiltration model parameters; precipitation, temperature, cloudiness, vegetation, slope. The important thing to note here is that for VA, this effort was rather premature, and the models were really developed to look at only the present day site. So a lot of these parameters are set, hard wired into the model for the present day site so that temperature is present day, cloudiness, vegetation. If we were to go to a long-term average or super pluvial, the temperature would go down and cause more infiltration. The cloudiness would increase and cause more infiltration. Vegetation would increase, probably cause less infiltration. So a lot of these things are counteracting.

Precipitation, we did try to find more suitable ways of doing that for the other climates.

All right, this map is what we used as the boundary condition for the UZ flow model, and let me show you here, this is the gridding for the UZ flow model. Here, this finer gridding area, this is the exploratory studies facility tunnel. The other finer gridding here is where the potential repository will go, and the repository fills up this lower level, and as what you can see from this map, is that the highest infiltrations are along the ridge crests. These lowests are where you have deep alluvium, which is, for instance, in the washes.

The model is being somewhat corrected as we speak to redistribute that water somewhat. But this is what was used in the base case, TSPA-VA.

Now, for the other climates, we needed to find a precipitation record, because it's not just the mean annual precipitation which affects infiltration. A greater effect is the variability. If you get one or two wet years in a row, you would expect a lot more infiltration. And how that's distributed in time is important, so we needed some precipitation records from sites that had roughly the same mean annual precipitation as our predicted climates, and what we found were these--well, basically three sites. Here's the present site which we're using for the dry climate. The other sites, these two possibilities were for the long-term average. We selected Rainier Mesa here, and it's part of Piute Mesa. And here in the Sierra Nevadas is where we picked the record from South Lake. It's at an elevation of 9,000 feet, pine forests.

Okay, so now with that background, here is the map for the long-term average, and this is, as I keep stressing, this is what's important to TSPA-VA base case. Do not compare this directly with the present day because we had to change the scale in order to see any variability. So this scale is different for this one.

Now, over this whole area, we got 32.5 millimeters a year. This is the USGS model from last year. Over the repository, it's more like 40 millimeters a year infiltration. Again, you can see the crest here of Yucca Mountain and the distribution, in general, everything a lot wetter. During the long-term average, it's predicted that there will be open juniper forests on Yucca Mountain.

Okay, the super pluvial is wetter still again. The average over this whole area is 118 millimeters a year. Along the ridge crest again, somewhat higher, this is probably about 300 millimeters a year along the ridge crest. What you start to see here, though, is that you actually start to see streams in some of these washes.

Well, we do have uncertainty in our infiltration model. Well, let me put it this way. PA placed uncertainty on the infiltration model. So what we did was we said that the--we took a divide by three, where we took every single cell on this model and divided it by three, and then we took the map itself, and then we took a map where every single cell was multiplied by three, and we used those to look at the uncertainty in the base case calculations. And this is an example of saying, well, is this appropriate or not. So what we did was we looked at the highest rainfall that we thought was appropriate for the area, which is 675 millimeters a year, and ran this absolute worst case sort of thing to compare with what we were using for our times three.

Well, over this entire map, infiltration is 284 millimeters a year, and in our worst case, absolute, our times three case for super pluvial, it's 360. So we're over estimating somewhat.

All right, sensitivity analyses for infiltration. This is the one that I believe tends to contradict our climate cases. What we did is here are those three uncertain infiltration rates, the times three, the base case map, and the divided by three map, and at 100,000 years, you see a significant difference here, probably an order of magnitude difference in dose. This curve is pretty much the same as in the climate one. This one should be pretty much the same as the LTA, but they don't match. They don't match very well.

I think part of the explanation is because we have the coupling between our waste package model and climate is incomplete. In this case, they actually did couple that, and they found out some interesting things, like for instance the amount of water going through the repository was so high here it kept the temperatures way down, and you basically got no failures between this initial juvenile failure spike and

20,000 years.

Okay, on to UZ flow. UZ flow has inputs to the seepage component, UZ transport components, and also the properties that were developed for the UZ flow model were used by the thermal hydrology component. The model is three dimensional, steady state, dual permeability, which means we have separate calculations for the matrix and fractures, and it was developed by LBL. The way it's used is that it was calibrated to the matrix saturation, the matrix potential, pneumatic data, and perched water data that we have from the site, using--and the calibration was done using infiltration maps and site hydrologic property data.

The way we used it is we used it directly. We had them run a bunch of cases. If you want to look ahead, it's this viewgraph here. We had them run all these cases on the bottom, and then we sampled from them.

Let me go back to where I was. This is the plan view of the top, the surface gridding. This should be the same as those infiltration maps. You should be able to basically overlay them, and it shows the repository. It shows how they put faults, major faults in here.

This is an example of the vertical stratigraphy that was put in the model, first the surface up here, the water table down here. For the different climates, we raised the water table like 80 meters for the long-term average, 120 for the super pluvial. There's quite a bit of structure in there. And then I wanted to show these 15 basic cases that we developed for sampling in the TSPA calculations.

Here, this is an example of the results that we get when we run the UZ model. These are the streamlines. I'm pretty sure this is for the long-term average climate, but it really doesn't matter. The point to be made here is that, well, this is just showing where the repository is. It's down here in the mountain actually, and these are wells at which we got data to calibrate the model from.

The streamlines basically go--are basically vertical from the surface down to the top of the Calico Hills, and interior to the Calico Hills down here. Some places they're vertical all the way through the section. In other places, they come down, hit, for instance here they're hitting the perched water, running off laterally, and going into the saturated zone. This explains why area 5, which is right along here collects so much of these radionuclides, is because they're taken from other places in the repository and moved over to area 5. And I hope you remember what area 5 was.

This is another way of presenting it. This is the infiltration from the infiltration map at the surface. Here it is at the repository horizon, and you see it hasn't changed much at all. And here it is at the water table, and you see it's changed a lot. In some places, the water has pretty much gone straight through. But in others, this is all dry and here we have a section where it took all this water and basically funnelled it down into this area, or this area.

And this plot gives you some ideas of the travel times that we're calculating with this model. For a present infiltration, about 7 millimeters a year. We're getting a median travel time of several thousand years through the unsaturated zone. For the long-term average, this has been moved down to about several hundred years for the super pluvial, maybe 100 years travel time through the unsaturated zone.

Kind of interesting here is this bimodal shape of these curves. These are showing pretty much the component that's really fast through the fractures directly from the repository down to the water table, and these are travel times of about one month.

Sensitivity analyses. We have an alternative conceptual model where basically all the water flows almost entirely through the fractures down to the water table, very little interaction with the matrix, even in the non-welded units, although there is a little bit. And here we have the LTA climate. Here is what I just showed you before, our normal base case model here for the LTA climate. Here is it we call this the DKM WEEPS model. It's a variant of the real WEEPS model, and what it shows is that you have a significantly greater volume of water goes faster directly down to the water table. In fact, over half of the water, the median travel time here for this WEEPS model is just two months.

Now, though, does that make any difference to safety, and as you can see, it really doesn't.

SAGÜÉS: What is the meaning of the legend again?

GAUTHIER: Well, I'm sort of sorry about this. I probably should have taken more time. But this is the base case model. This is what--what we did was we ran the base case over again, and all we changed was the UZ flow model that we were looking at. So this is the comparison one. This is the one Bob showed you for 100,000 years, the dose curve, and what we did was we put in that much faster travel time through the fractures, and that's this what we call the DKM WEEPS model, and that's all we changed. We didn't change the seepage model. We didn't change the matrix diffusion model. All we changed was this basically the travel time model.

BULLEN: Bullen, Board. Jack, did you change the waste package degradation model in this?

GAUTHIER: No.

BULLEN: Or is the waste package affected by this faster flow?

GAUTHIER: I would think so. I would think that in fact you'd have fewer but quicker failures with this sort of model, but we didn't carry it through there. All we looked at was travel time in this case.

BULLEN: Cliff, would you come to the microphone, please?

HO: For both the base case model and this alternative WEEPS model, what you're asking, Dan, is at the repository, and for both these models, the partitioning in the fracture is nearly the same. Where the real difference comes in is when the flow flows below the repository, you've got more of the particles staying in the fractures below the repository. So as far as what's happening at the repository, the two models show very similar results.

BULLEN: I guess a followup question before you leave, Cliff, would be does the water from the surface get to the repository horizon faster also? Is that the indication?

HO: Yeah, for the WEEPS model, we haven't actually done a lot of sensitivities on it, but I presume that because there is more fracture flow, even in the non-welded bedded tuffs, say the PtN that's above the repository, that the travel times would also be faster.

BULLEN: And then the follow-on is did you change the fraction of waste packages that saw seeps, since the seeps are the key factor on when the waste packages fail?

HO: We have to remember these are steady state runs in the UZ, so what's really important is the partitioning, how much water is in the fractures versus how much water is in the matrix. And, again, for these two models, the amount of water in the fractures is similar at the repository horizon. You're talking more of a travel time, transient issue, up above the repository. Maybe Mike can add more.

BULLEN: Mike Wilson, go ahead.

WILSON: We used the same seepage model, and in the model, it's a function of the percolation flux, which as Cliff was saying, is basically the same in the two models.

BULLEN: That's not intuitively obvious to me. I guess that if you have more water flowing through fractures, that you'd have the same number of drips; is that what I'm supposed to be--

WILSON: The big difference is in the non-welder layers. In the welded layers, the two models look almost the same, and the repository is in the welded. So they look basically the same at the repository.

BULLEN: Other questions from the Board? Richard Parizek?

PARIZEK: Yes, Parizek, Board.

Our clarification on Page 24, your spaghetti hanging off the plan surface had colors. What were those colors? You didn't explain this. Is that part of recharge amounts or flux rates, or what was it?

GAUTHIER: Number 24, oh, that's the flow field, flow streams. Do you know, Cliff?

HO: Cliff Ho, Sandia Labs, M&O.

These colors are just simply for visualization to identify the different particles that were released to identify the stream lines.

PARIZEK: So they have no flux, significance?

HO: No.

PARIZEK: Now, a question about--you show that as being high infiltration, based on Flint's work. Some of that ridge top is narrow, and why wouldn't there be more run-off, storm water flow off that crest, and as a result, maybe refocus the infiltration part-way down the hill rather than right up on the ridge? I know it's obviously cooler up there. Maybe the rainfall is higher. On the other hand, is it reasonable that that's the high infiltration part of the landscape?

GAUTHIER: I don't think it's reasonable. We've been reworking this problem. Well, I want to say that I'm just one opinion in this matter. We've been reworking it. Al Yang at the USGS has just started to analyze chloride concentrations in the PtN below the ridge crest at SD-6, and he's getting rather high concentrations. They indicate an infiltration to first order, an approximation, of about 2 to

3 millimeters a year.

We get the same sort of--we know that the perched waters are very dilute, and they came in much--at a much higher infiltration rate, although perhaps episodically over time. The first order, they look like 10 or 12 millimeters per year. I think, and I believe that there's some basis in this at Apache Leap and at an analog site that the NRC has in Arizona, and Rainier Mesa, which is north of Yucca Mountain at the NTS, that part of the infiltration comes through the washes, and Alan Flint and Joe Hevesi, who are responsible for this model, are aware of that and they've made some great strides in the past several months to look at run-off in a more rigorous manner, and now they are getting quite a bit of infiltration in washes.

PARIZEK: It would be logical because you have a focused flow there, and often the alluvium is very thin up at the headwaters of those. So that really ought to be a high infiltration area. And what the outcome of that is, you don't know yet because you've just put the water in somewhere else, and maybe that's good, or it may be bad, or it doesn't make any difference to the dose eventually.

GAUTHIER: I think it would--well, we've looked at this same question, but in other TSPAs basically, and what we find is that the more concentrated the influx in certain areas, the less the releases, because you have fewer containers interacting with these high recharge zones.

PARIZEK: One other question about the Rainier Mesa analog. There, that's got vegetation distribution, I guess known precipitation, so that is useful in trying to say what the rainfall does to the vegetation. But do you know the infiltration rate there by other ground experiments within, say, access tunnels that are in Rainier Mesa?

GAUTHIER: We have a good idea. In the center of the mesa underneath a wash, it's about 24 millimeters per year.

PARIZEK: Measured? It's measured?

GAUTHIER: That's probably a minimum. But what they did is they would pump water out of the tunnel. But I think it's a good minimum because what they did was they drove the tunnels right through this perched water zone, and all the infiltration seemed to be doing was raising the perched water so that it entered the tunnel, and when they would pump it out. So we know the volume coming out. We know the area above. That center tunnel is pretty well defined. It's a minimum of 24 millimeters per year. It's like our LTA. Other parts of Rainier Mesa show much less infiltration. Down south, a tunnel south of there is probably between 7 and 10 millimeters per year, and a tunnel south of there is G-Tunnel, shows virtually no infiltration.

PARIZEK: And that's based on some rock types that are similar enough to what we have near the crest of Yucca

Mountain?

GAUTHIER: Right. Rainier Mesa has quite similar geology.

BULLEN: Other questions from the Board? By the Panel? Chris Whipple first.

WHIPPLE: Yeah, if you could find your Slide 10 again? It's the climate sensitivity million year.

GAUTHIER: Right.

WHIPPLE: Yeah, I'm pleased to see this curve, because it helps separate the performance of the system for different climate cases, and I think the million year scale may make the effects in the first period difficult to sort out. But my question is, as I understood you to say, that the reason that the early failure rates for the present day dry climate appear to be as great as for the other cases, was that what I saw as a trade-off, that in the WEEPS model, you have far fewer cans to get wet, but conversely, you have a much hotter repository, and that the two sort of offset, and the number of can failures is roughly the same in all three cases. Was that what you said?

GAUTHIER: Well, that's not what I said. But what you say is probably true. We're kind of mixing several different factors here. This one is just infiltration. This one is just different infiltration rates basically.

WHIPPLE: But you do have the WEEPS model running so that the number of cans that see liquid water goes up with infiltration?

GAUTHIER: Yes. The WEEPS model that we used in TSPA-93 shows that.

WHIPPLE: My question, and this is based on having seen earlier versions of this, is I saw a much greater sensitivity of early can failures to infiltration than this shows. Have you adjusted something in your corrosion model to make it more temperature sensitive since the earlier runs were done?

GAUTHIER: I'm going to let Jerry discuss that. He's shaking his head.

WHIPPLE: Why the can failure rates are no higher as the influx doubles and triples and so forth? With the WEEPS model being non-linear on cans seeing drips, I can't tell why that would happen here.

GAUTHIER: I think you may be want to consider this one. This is just the change in infiltration. The point I was trying to make is that these two plots are contradictory, and I don't understand yet why that's the case. But here we see for a higher infiltration, this black line, we have this dearth of releases below 20,000 years, and that's probably because the cans do not get hot.

WHIPPLE: It may be that 100 runs isn't enough.

GAUTHIER: Well, actually this is just one. So you're right, we probably need to make--we need to investigate these in detail, I think.

WHIPPLE: Again, I'm not trying to dominate this, the WEEPS model and the curve Bob showed earlier this morning shows that for present day climate, you've got on the order of 1 or 2 per cent of the cans seeing liquid drips. And for the long-term average, you're seeing on the order of 30 per cent of the cans. And that should translate into 15 or 30 times more early corrosion failures for the wetter case, and it doesn't show up on the dose curves.

GAUTHIER: My disclaimer was that we really haven't had time to study these, and I'm just presenting them to show you really what we're doing with our sensitivity studies. If I had them to do over again, I would have been a little bit more particular and made sure the correlations were better aligned through all the parameters. So I'm just saying this is what we've done so far, and unfortunately I can't give you a better answer than that.

BULLEN: Jean Bahr?

BAHR: I was glad to see that you changed the duration of your climate change periods. I guess some other issues associated with that, and I don't know if you're planning to look at those, would be the pattern of those, because you always have a dry period directly following a super pluvial, and what's the effect of that. And then also your effect of the very rapid change, which isn't necessarily geologically unrealistic, but it would be worth looking at what that is, because even though you're only seeing small changes in the peaks, depending on what regulatory standards you're ultimately meeting, the timing of those may be important, and whether they happen early or if you're seeing the peak being this long-term release at a million years may eventually be important.

GAUTHIER: It might be important. You're right. In fact, in the 1980s, the precipitation at the site averaged about 150 millimeters a year. In the 1990s, so far it's been about 250 millimeters a year. So we might actually be at what we considered the long-term average within reason right now. And it all started when we got that series of El Ninos starting in 1991.

BAHR: Are you planning to look at different patterns of those climate changes, in particular going from a super pluvial back to the long-term average rather than going to a dry period right after that?

GAUTHIER: Well, we weren't, although we were going to try to define the super pluvial better, because at this point, we just know that there's something very extreme in the record, and we don't know all that well its characteristics. And from the record, it looks like it's always followed abruptly by a dry period.

BULLEN: Any other questions? Board Staff, questions?

Leon Reiter?

REITER: Jack, you said that you assumed the pluvial started at 5,000 years, but you varied it over 1,000 and 10,000. What's the different sensitivity, say we assumed the average started say ten years from now versus say 5,000 years from now?

GAUTHIER: We didn't see any sensitivity in the sensitivity analysis to that parameter. I do have to caveat that a bit because our waste form degradation model is incompletely coupled to climate--I'm sorry--waste package. What did I say? Waste form?

BULLEN: Any other questions? If not, thank you very much, Jack, and we will reconvene at 1 o'clock.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

BULLEN: A couple of administrative announcements before we get started with public comment. For those of you that may notice, we did turn up the heat this morning. The air conditioning is broken. We have been assured that it will be fixed within the next two to three hours. So we might see a few overcoats shed, and we have a couple of fans running at the front of the room, or around the room, but we can only run them at this speed, because it will drowned out the microphone sound. So bear with us.

As always at the Board meetings, we like to encourage comment from the public, and you'll notice that today's comment is not at the end of the day. We understand that some people, when our meetings drag on, can't stay for the duration because of scheduled plane flights or other commitments, and so we've tried something new with these meetings. We're having our public comment period at noon today, and we're going to have our public comment period first thing tomorrow morning.

So we had two people who signed up to make public comment, and so I will invite them to come to the microphone first. And if there's anyone else who would like to make comment, please come to the microphone after that, and identify yourself and make your comment.

Our first speaker is Mr. Sheldon, is it Teetlebaum--from SAIC, M&O? Oh, he signed the wrong list. You signed the wrong list? Would you like to make a public comment. Glad to be here? Okay. Then we only have--we have one other person who signed up to make a comment, and that's Ms. Judy Treichel from State of Nevada.

TREICHEL: Judy Treichel. I am from the State of Nevada, but I'm from the Nevada Nuclear Waste Task Force.

I'm not sure if I'm making a comment or asking a question. I most times plan to ask a question and then there's dead silence, so it winds up having been a comment.

I'd like to go back to Abe's presentation. This should come as no surprise to you, Abe. On one of your slides, Number 3, you're saying that there's a question, and the way I understand it, this question came at the request of the Board or as a recommendation of the Board, and the question is, "Does the TSPA demonstrate the safety of the repository?" And it's never been my understanding that that was the kind of thing that the Board thought that TSPA should do. And I know you're quoting a source in here, but I don't have that with me, and I wasn't able to find one here. It's the report before that one.

But in this report, which I agree with, and I can't imagine that they've changed so much, it says, "In the report, the Panel down plays the TSPA as being a rigorous predictive tool, preferring to think of it as a means of demonstrating a reasonable expectation of compliance with safety requirements." Do you think it could comply? Do you think it could not? But that has nothing to do with it being a safe repository. That's a tremendous leap from an estimation of how you would do--you know, how you think you would come up against the requirements.

And then it goes on after that, you're talking about "enhancing the likelihood of obtaining positive answers to that question." Well, the only positive answer I can guess at would be that yes, it's a safe repository. So that would then wind up being a suitability decision on the basis of TSPA, which has been something that's made me break out in a rash for a long time. And I don't like it. I don't know if you're going to respond or not.

And then the last thing is under one of these things that would lead to this positive answer, and of course it's at the very end, which you alluded to before, is public acceptance. And this is just curiosity, but it talks about increased public involvement, which would certainly be a new thing for this program. And you said that something was in the works on that. Are you able to talk about that? Is there anything that is happening for public involvement?

BULLEN: Abe, if you don't mind, Abe, would you mind at least addressing the issues that Judy brings up?

VAN LUIK: Yes. I think they're good issues, but they convolute a couple of things. I don't think there's a real difference between what we paraphrased from the '96 report and the one that you're reading.

I also agree that PA is not a quantitative predictive tool, but I think it is a stylized indicator of performance that the regulators have taken that into account. You know, if you read the regulations, they say that proof is not to be had in the ordinary sense of the word. There is uncertainties, a lot of irreducible uncertainties. So they recognize that, but yet they are setting standards that they feel are protective of public safety, and they say that the way to meet them is to take these calculations that we call performance assessments and show that, you know, given all the uncertainties in those calculations, show that there's a reasonable certainty of meeting these requirements.

So I think there's no contradiction, but there is a couple of loops in between there of logic that in my viewgraphs, I didn't explain. As far as the VA not making a safety case, being a decision point for the Department, that's correct. The VA is not to make a decision for the Department, but we fully recognize that if we come in with a VA result that is totally unable to pass any technical, public or political ho ho test, that we're in deep doo doo in this project.

But the VA is not a decision point for us. It's a declaration of what it is, what it costs, and how we think it will perform, and what is necessary to move us from here to licensing. And it's supposed to inform the decision makers that we've alluded to a couple of times in my talk, the political decision makers, as to whether or not they want to move forward with this program.

As far as public acceptance and public participation, these are difficult things. The idea that we can go out and sell this whole thing to the public I think is a bit ludicrous, because they have nothing to gain from it except perhaps people that live next to a current waste storage area. But what we have is a plan that is coming together to take the VA and its results basically to the public and explain it to them, and what I alluded to this morning is that we have a couple of people and organizations, one of which is in contract negotiation right now so I can't reveal who they are, that have experience with basically explaining complicated controversial doings to the public. And as one of these people explained to me, we see it as a success if the people understand it. If they understand it and reject it, we still see it as a success, but the idea is that we go and explain to the people what we're proposing to do, and what the outcome of it will be in the long term.

So to answer your questions, yes, we are going to have more public involvement after the VA in terms of trying to explain to the public and getting reaction from the public. And I see no real difference between seeing that TSPA makes a safety case versus saying that it addresses regulatory requirements, because the societal view is that the regulations that we address do exist only to protect public health and safety.

TREICHEL: Well, I don't know if we've discussed this or argued about it. I keep forgetting which it was. But I think we have had a discussion about how VA primarily should be a tool to tell you mostly what you don't know yet rather than turning it around and saying you do know that in this case, you've got a safe repository. And that's just lousy wording.

As far as the enhanced public participation thing, I'll be really excited to see that, and I'm sure it will all be top security until it happens.

BULLEN: Thank you, Judy. Now I've run to the end of my list of people who had signed up for public comment, but as I scan the audience, I find people who have been long involved in PA either in the Yucca Mountain project or at the waste isolation pilot plant project, or as part of the TSPA peer review panel. So if you have any questions or comments and don't want to say them today, it might be strongly encouraged that tomorrow morning first thing, you can come and pose your question or ask your--or make your comment.

Is anyone else interested in making a public comment at this time? Dr. Long, please identify yourself.

LONG: I'm Austin Long at the University of Arizona, and I've been working with EPRI on some climate modelling, so I have some questions about the climate models that maybe Jack to respond to.

I notice that there was a conspicuous absence of any greenhouse model in the climate, and there's a consensus of climatologists that suggests that this might be an important omission.

I also was wondering if it might be more valid to consider only the winter precipitation, as many people think that this is much more important in terms of net infiltration than the summer precipitation, and it might, if you just consider the winter portion, that might cut down the total net infiltration. I don't know the details of your model.

Also, just looking at your--what you consider the average long-term precipitation, it looks like an exaggeration, it looks like what you've taken is a value that's much more close to the glacial maximum than sort of the glacial modal value. I'm wondering if I'm misinterpreting what you've shown.

GAUTHIER: Well, this is Jack Gauthier. Well, Austin, let's see, for number one, the greenhouse, when we first defined the climate scenarios, we did have a greenhouse climate in there, and we thought that perhaps there's some reason to believe that perhaps a warmer earth would cause more El Ninos. El Ninos approximately double precipitation in that area. So we picked a value of two times precip, basically the same as the long-term average.

It turned out that all of the--for pragmatic reasons, we didn't necessarily want to go into that sort of detail looking at maybe a 200 to a 2,000 year stretch there separately, so what we decided was by making that initial climate change time, sampled uniformly between zero and 10,000 years, we would at least be able to see if that parameter, that start time was important. It's turned out so far that we haven't seen it as an important parameter, so I'm not sure that--I mean, we'll probably reexamine this, but I'm not sure that we found that it is important to have a greenhouse climate, other than for perhaps a scientific community confidence issue. It doesn't seem to be important to the final results.

The winter precip issue, it turns out that when you do have a climate change there, from our reconstructions, it looks like most of the precipitation change is in the winter, and in fact when we did the biosphere calculations, there was so little summer precipitation change in future climates that it didn't really make any difference to the results, because that's when you do the irrigation and what not. So in a way, we're implicitly taking that into account.

We have asked--PA has asked the people doing the infiltration modelling to add a snow pack factor that either delays or somehow takes into account the fact that you're getting most of your infiltration and run-off, you know, in the spring and is saved up over a number of months. So we are hoping to get that taken into account before LA.

What was your last comment?

LONG: Something about the long-term average, it looks like you've taken a high value.

GAUTHIER: That's true, and the climatologists--well, I don't want to include you in this, Austin, but they tend to like extremes and what not. That's where all the action is.

LONG: That's the exciting part.

GAUTHIER: And so I had gotten a lot of pressure from our USGS climatologists to make that even larger. They wanted 2.3, for instance, which is what the average is was that they came up with, because that would be the actual stage to maximum, would be about 2.3, and I said well, look, we're trying to capture behavior over a long period of time here, and so we need the average between that maximum and something more, something not quite so extreme that would still happen during one of these glacial climates. So we came up with two, and we didn't add a decimal place because we didn't think we needed it, or from a PA perspective, we didn't think the competence was there also.

LONG: If I could add a fourth point? This is more sort of making a grand stand play for a probabilistic approach to climate, because the future climate is far from agreed upon by climatologists, and there's a, for example, a non-zero probability that we'll have a runaway greenhouse and those fluctuations will go away, a small, but non-zero.

GAUTHIER: Right. I wish we could do something more sophisticated with our climate modelling, and perhaps we will be able to. So your point is well taken.

LONG: Okay, thank you.

BULLEN: Thank you, Dr. Long. Any other questions or comments from the audience?

(No response.)

BULLEN: Seeing none, I will remind you that we have again a comment period tomorrow morning first thing at 8 o'clock, and we will now continue with our technical presentations on the TSPA-VA. I'd like to re-introduce Mike Wilson, who's going to speak to us about seepage and thermal hydrology.

WILSON: Okay, seepage and thermal hydrology. And I really need a list of acknowledgements for this one because a whole lot of people are doing a whole lot of work on this, and most of them not me.

I thought I would cover seepage first. All right, first definition, what we mean by seepage is the liquid water getting into the emplacement drifts, and in particular, we're interested in that that actually drips on waste containers, of course. We know that it's important. It enhances waste package corrosion and mobilization of radionuclides and the local transport of radionuclides. And it's been observed in the results I showed earlier and in previous results also that that's one of the key factors in influencing the repository performance.

We have in our model, parameterized seepage with two quantities, the seepage fraction, or fraction of waste packages contacted by seeps, and the seep flow rate, which is the actual rate of water flow onto the containers that do have seepage. And as has been noted, the spatial discretization we're getting in our model for the EBS is divided up into the six repository areas that were shown before. Ideally, I guess we would be modelling 12,000 containers or 10,000, however many there are, but we figured six was as many as we could handle.

The conceptual model of seepage is that an opening in an unsaturated medium can act as a capillary barrier and water flows around it. Seepage will occur at a point on the drift wall if it becomes locally saturated there. And that local saturation can be because of the fact that you have an opening there blocking the flow, or because of heterogeneities in the permeability field creating low blockage to the flow.

The fact that this capillary barrier effect exists has been confirmed in other contexts, but in particular, a recent ESF niche study in which they released some water above an opening off of the site of the ESF and measured how much water came through into the opening below. It seemed to fit our conceptual model pretty well.

The basis for--

BULLEN: Bullen, Board. Just a quick question about that. In the niche seepage test, when they did the introduction of the water, did you see the seeps form at the same place, or did they show up at different places after you did it in subsequent introductions? I guess the key here is, you know, will it drip in the same places?

WILSON: I don't really know the answer to that right off hand. I don't think--the results that I have seen are pretty early on, and they may have answers to that by now, but in what I have seen, they hadn't done that yet.

BULLEN: Okay, thank you.

WILSON: In other contexts, it has been observed that flow tends to follow the same paths. If a path is wet, it will flow preferentially there.

Okay, we're basing our seepage for the TSPA on a series of process model calculations that have been made at Lawrence Berkeley National Lab. using a three dimensional steady state isothermal fracture continuum model with heterogeneous fracture permeability. And this is an illustration of it showing three slices vertically and three slices horizontally. You can see the heterogeneity of the flow. You can see this is the opening. You can see the water is going around the opening. And here's a place where the saturation is getting high right at the crown of the drift, so that water might be about ready to start seeping in. This is an illustration of the process model that's being used.

This last point relates to comments that have already been made. I didn't specifically mention the collapse of the drift here, but in the modelling we're doing, when--things like collapse of the drift, thermal mechanical, thermal chemical alterations of the permeability around the drift, those might be important, but we have not included them, other than to a very small extent in some sensitivity analyses. We're assuming that for this base case, we're assuming that those effects are insignificant or offsetting. And what I mean by that is it could be that thermal mechanical effects tend to open the apertures. Thermal chemical effects tend to close the apertures, and it sort of balances out. That's only speculation.

SAGÜÉS: What's the meaning of the colors?

WILSON: The colors? It's the fracture saturation. This is low saturation. The purple is the highest saturation.

This is a way I tried to visualize for you the kind of things we're calculating. This is a segment of a drift that's been opened up and laid out flat. So this is the top of the drift. This is the bottom of the drift. And this is the bottom of the drift. They should close together like that. And it's illustrating the model the men were using. It's about 45 meters long and 5 meters in diameter, so that after you lay it out, it's about 16 meters in circumference. And what we're doing is dividing this up into segments of 5 meters length because the waste packages are about 5 meters in length. So you have nine of these here, and then you run the simulation and look at where water seeps in in the model. For this illustration, these represent places where water is seeping in in the model.

And then for purposes of carrying forward into the seepage model for the TSPA, we look, number one, at how many of these have seeps. So for this example, there's two of these nine segments that have seeps. So the seepage fraction would be two over nine for this particular calculation. And then for the ones that do have seeps, you would look at the total flow rate in that. For example, you'd add these two together to get the seep flow rate. This one only has one so that's what you would use. And we're only counting seeps that occur in the top half of the drift.

This one, for example, is on the bottom half of the drift, and the assumption is that you have water that's kind of seeping in the side there, and it's not going to go horizontally enough so that it would contact the waste package. It needs to be above the waste package. And another thing to notice, the waste package in fact is only something like one and a half meters wide. So including the entire 5 meter drift width, we're including some conservatism.

This is kind of a complicated viewgraph that shows how we're taking the output of the process model and converting it into a probability distribution for use in the TSPA. This here is an example of the output of the process model for a given percolation rate, 73 millimeters per year at the top boundary. There's a matrix of cases that are run varying the fracture--the mean fracture permeability. The fracture permeability field is heterogeneous, calculated from a statistical model, and this is the mean, ranging from 10 to the minus 12 to 10 to the minus 14 meters squared. And then the fracture alpha parameter, which basically represents the fracture aperture size, is also varying over one order of magnitude.

For each value of percolation, we run a matrix of these nine cases for that 45 meter long domain, and this shows what the results were for this particular run. For this run, these three had seepage fractions of one. So in those three combinations of fracture parameters, all of the nine segments had some water dripping into them. This one had water dripping in about half of the nine segments, and this one had water dripping in a couple of the segments, and these didn't have any at all. So this is encapsulating the results of a bunch of 3-D model runs, and you can see the behavior of this model is that you tend to get seepage more at the high fracture apertures and at the low fracture permeabilities. Basically, with the process model we're using, if the fracture permeability is high, it has a lot of lateral mobility and it can go around the drift very easily. And so when the fracture permeability is lower, it has more of a chance of getting trapped and actually funnelled into the drift itself.

To go to the probability distribution that we want for sampling in a TSPA, we have to assign some kind of weighting to these nine cases, and this is the weighting that we chose for our base case. Basically a 25 per cent, 50 per cent, 25 per cent for the three fracture alpha cases, 25 per cent, 50 per cent, 25 per cent for the three fracture permeability cases, and that's reflecting what we know about the fracture properties in the Topopah Spring, and primarily from some bore hole measurements and a lot of ESF measurements.

This case here is what we thought was the most probable case based on air permeability measurements and fracture mapping and all that kind of stuff, and we're allowing for a spread around it in both directions. Now, if you take these results and weight them with these, you can get a histogram of the seepage fraction like this. So that is for this bar here, you'd take these three that all had seepage fractions of one, and you'd multiply them by the three weights here, and it would give you a bar this high. And these four zeros would get multiplied by this weight, the sum of these weights, and that would give you this bar here. And then these two gives you these two other bars.

Now, this is a discrete probability distribution for seepage into the drift for this particular percolation flux. What we did to carry it forward to the TSPA is to take this discrete permeability distribution and make it into a continuous permeability distribution represented by the mean and standard deviation. And that is illustrated here. For the range of percolation fluxes that were calculated, this shows the mean and the standard deviation that we got by that method. This is the 73 millimeter per year one here, so it has a mean that's around .4 for the seepage fraction and a standard deviation that's a little bit higher.

And this part of the thing right here reflects on a question that came up this morning. In the process model, there is actually a threshold around 7 millimeters per year. Below 7 millimeters per year, with those combinations of fracture parameters, we get no seepage into the drift. And above 7 millimeters a year, we do. But the bottom part of this curve is adjusted to take into account the fact that you have a distribution of percolation fluxes around the repository, and any one of the six areas that has a mean of, say, 5 millimeters per year will have some fraction of its fluxes above the 7 millimeter threshold. So the bottom part of this has been adjusted downward in order to take into account the distribution of percolation fluxes within the various regions.

This shows the same thing in a little different way. This is the same mean curve, but now instead of standard deviation, I'm showing the 95 percentile and 5 percentile curves, and this shows that there's a very large uncertainty in what the sampled seepage fraction is within this method. And we go through the same exact procedure for the seep flow rate and come up with this sort of a thing where this is the mean, and then this is the 95th percentile and the 5th percentile. This gives a range of seep flow rates that are sampled from within the EBS model.

I have only one other thing to show, and that is the effect of changing the distribution that's used for those input parameters, this is going back to this here, these weighting factors, the slide that I put up and then took back down shows the effects of changing these weighting factors in two ways. One is shifting the permeability to higher permeabilities in the fracture alpha to higher alpha, that's this way, and the other is shifting the permeability to lower permeabilities and the fracture alpha to lower fracture alphas.

So basically this is representing fracture apertures opening up around the drift by about a factor of three. This is representing the fracture apertures decreasing around the drift by a factor of three. And how does that affect the result? I don't have a picture showing how it changes the seepage distributions, but this shows how it affects the computed CCDF for the peak doses over 100,000 years, and you can see that there's some effect, but it's not a huge effect.

The case where you decrease the fracture apertures, that's going to the lower permeability and as I said before, that tends to increase the amount of seepage in the model. So that you see that there's fewer of those zero dose cases. There's seeps in more places, and so more of the containers fail, and it makes the releases a little bit higher, but not a lot, and the other case where the fracture aperture is open, it moves things over by I guess maybe about a factor of three.

To summarize, the seepage for TSPA-VA is based on a 3-D heterogeneous drift scale flow model. The comparisons that we have so far with initial ESF niche tests are actually pretty good. We used nine sets of fracture hydraulic properties weighted in the way I showed to arrive at the final abstracted model, and then some kind of rule of thumb numbers to remember for--within the model for the present climate, the seepage fraction is about 3 per cent on average with a huge range of uncertainty. For LTA climate, that goes up to about 25 per cent and still a very large range around it. Super pluvial, up to about 40 per cent.

By comparison, the UZ flow model expert elicitation asked the experts assembled in that to estimate what they thought the fraction of the area within drifts that would have seeps would be, and in fact only I think maybe three of them ventured to answer that question, and the ones who did came up with answers of very small, like a tenth of a per cent, up to as high as 10 per cent for one of them, which kind of agrees more or less with the range we have here, for what it's worth.

The seep flow rates range from about 20 liters a year for the present climate, that's the average again. Just to calibrate it, that is something like a drip every couple of minutes. Up to for the super pluvial, as high as 700 liters a year, which is still only about a drip ever few seconds. So we don't have any flow rates that are high enough that they would be a continuous flow in what we're using.

Going on to thermal hydrology.

FRISHMAN: Mike, let me ask one thing, just as sort of a bridge. Have you thought about looking at a very sort of detailed sampling of Chlorine 36 along the ESF? Because every one of those represents a seep. Have you looked at that as sort of a reality check under whatever kind of episodic condition probably created those, and see how that compares with your seepage fraction?

WILSON: We thought about doing things like that, and looking at things like fracture densities and fault densities and things, and we haven't done a lot with it, but I believe from what we have done, that something like that gives an answer not that different from what we have, although you don't know what the percolation flux was like when that Chlorine 36 came down, so you don't have a perfect comparison. But using the current climate, you would get, I can't remember any numbers, but pretty small numbers. The fraction of the Chlorine 36 samples that have Chlorine 36 elevated is relatively small. So it fits in with this qualitatively.

NELSON: Nelson, Board. Have any of the observations about drips in other tunnels, like over at Rainier Mesa, be of use to verify this?

WILSON: I think they would, and Jack for on is pushing very hard for that, but it has not been done so far. I think that would be a very good thing to do to add some confidence to the model.

CRAIG: Craig, Board. Could you explain how you go from this continuous flow to the actual drips that will hit the package, and in particular, how do you handle the concentration of minerals and the possibility that you may get mineral build-up if you have a wetting and drying out process on the surface of the canister?

WILSON: That's something we're not really doing. The near field geochemical environment task has done a few kind of back of the envelope type of calculations to try to estimate things like that. But it is not included in our base case at all.

BULLEN: This is Bullen, Board. It looks like every time you put up a summary slide, we're trained to ask questions. And so we'll just take a break in the middle and go ahead and Alberto can ask the next one.

SAGÜÉS: Very good. The effects of high temperatures after thermal holes on the question of water returning, is that taken care of by this model?

WILSON: No, that's another thing that we're neglecting right now, and I didn't put a bullet on here, but there's a bullet at the end of the thermal hydrology section that says that at the moment, we're neglecting it, and we actually have some basis for that, and that's that the waste containers are for the most part not failing until many thousands of years out.

The effect of increased seepage that you might expect from condensate drainage is probably only going to happen in the first thousand years or so. So we don't feel too bad about neglecting it because of that. We did want to do a sensitivity study, and I hope we still will, in which we try to take that into account. But it hasn't really been done yet.

BULLEN: Priscilla Nelson?

NELSON: Nelson, Board. Even as you're concerned about seepage coming into the tunnel or the emplacement drifts, how--are you also modelling how water is leaving the drift?

WILSON: Not really. We assume it does. We assume some fraction of it goes into the container and contacts waste, and then exits out the bottom, but we're not discriminating it at all.

BULLEN: Questions from the Panel?

WHIPPLE: Just a quick one, Mike. Same question I asked Bob this morning. Have you all considered trying to do this flow on an episodic rather than a steady case?

WILSON: There were a few calculations done with the process model with that, just kind of what if sorts of things, but we haven't really gone anywhere with them because we really don't know how to tie that to some--to reality. We have no idea what episodicity might be, and there's a very strong feeling within the project that the Paintbrush unit above the repository probably damps out most of those episodic behavior. There is a minority opinion that that's not the case, but given that, we don't know what the episodicity would be. I think it's work looking at, but we're not sure quite what to do with it.

BULLEN: Jean Bahr, and then Steve.

BAHR: Jean Bahr following up on Priscilla's question. Are you assuming then that the water that infiltrates exits almost instantaneously, that there's no build-up at the base of the drift?

WILSON: Yes.

BULLEN: Steve?

FRISHMAN: Alberto's question was one that I talked with someone about at the break today. We're not just talking, when we're talking thermal effects on the fractures, we're not just talking about reflux from the thermal pulse. What we're talking about is changes in the fracture system, in the fracture fills, that are caused by the thermal pulse, but then in a sense change the character of the fracture system from then on until something else happens.

WILSON: Yes. That's right.

FRISHMAN: And that's what I was talking about this morning.

WILSON: For the base case, we're neglecting it, and the only thing I have to offer on that subject is this one little curve. This is probably as much as we're going to get in the VA as trying to estimate that effect. That's pretty much, other than this, that's pretty much beyond what we're doing.

BULLEN: Paul Craig?

CRAIG: Craig, Board. Could you also talk a little bit about how to think about the fraction of the packages that get wet and how the number of cracks changes in the different kind of flow regimes? Is the number of packages that will never get wet, is that fixed as a parameter, or is that computed by the model?

WILSON: Well, it's computed from--I guess that's like one minus the seep fraction. You can--

CRAIG: What I'm thinking about is that we were told this morning that some of these packages don't get any drips at all. Is that--

WILSON: These numbers here reflect on that. For super pluvial climate, on average, 40 per cent of the packages are contacted by seeps. That means that 60 per cent of them are not. That 60 per cent would be dry always.

CRAIG: Okay. And that's--

WILSON: However, that is sampled and it's actual sampled almost between zero and one. There's a very wide range about it.

CRAIG: So that 60 per cent is the number that comes from averaging the runs; is that correct?

WILSON: That's right. This would be like an average over the hundred runs.

CRAIG: Thank you.

WILSON: So on average, over all the runs, 60 per cent of the containers would not get wet.

BULLEN: For completeness, any questions from the Staff at this time? Carl Di Bella.

DI BELLA: Well, this is Carl Di Bella, and you wanted to be complete, so I'm going to ask a question about the TSPA-VA documentation, and maybe some of the documentation that backs it up, too.

Some of the things that are not going to be in TSPA-VA, for example, Steve mentioned refluxing apparently is not going to be in there, and this assumption that Mike mentioned that the effects of seismic and thermal hydrological effects on the drift stability are not going to be in there either, are these assumptions going to be discussed and the basis behind these discussions discussed?

WILSON: Yeah, that's our plan. Some of these are in the category of future work, and we do want to discuss them and try to estimate how important we think they are and what priority they should be given. And in some cases, there will be a lot more discussion in the technical basis report than in the actual VA report itself.

BULLEN: Thank you, Mike. Now you may continue.

WILSON: Well, I'm not watching the time. I hope you are. But you're allowing a lot of time for questions, so I think we're probably okay.

BULLEN: That's actually how the meeting was laid out.

WILSON: Okay. So thermal hydrology. Basically, we're doing two kinds of thermal hydrology and calculations that we called drift scale and mountain scale. The drift scale thermal hydrology calculations are used to determine the thermal dynamic conditions in the drift, that is, how hot and how humid it is around the waste packages.

The mountain scale thermal hydrologic calculations are used to look at larger scale movement of fluids, especially with gas, but to some extent, the liquid. It's important to note, though, that the drift scale calculations have to be coupled to mountain scale calculations because there's important effects, such as the containers near the edge of the repository cool off faster than containers in the center of the repository, and so you need to have the two scales coupled.

In fact, we do have two separate models. We have one model that's strictly a mountain scale model, and then the model that we called this scale is in fact a sequence of models that go all the way from drift scale up to mountain scale to try to approximate the coupling.

This is just an illustration of the quantities that thermal hydrology is calculating that are getting passed on to the other components of the TSPA. The waste package temperature and relative humidity is used for waste package degradation. The drift wall temperature is used for the near field geochemical environment calculations, and the invert liquid saturation is used for EBS transport, for example, to calculate what the diffusion coefficient should be in the invert, because that's saturation dependent.

The mountain scale model looks something like this, and we're getting from it air flow parameters, the air flow flux and the air mass fraction at a center repository location and an edge repository location. And this model uses a smeared heat source and so it doesn't have actual discrete containers in it.

The modelling approach for the drift scale model is, number one, we don't model a single waste container. We model a drift segment that has eight waste packages in it, because there's a large variability of the heat output from different waste containers, and that's one of the effects that we thought was very important to capture in what we're doing. The hottest waste packages, what they call Design Basis Waste Packages, are clear up at 18 kilowatts, and some of the waste packages that just have DOE spent fuel in them will be less than 1 kilowatt. So there can be a big difference in how hot they are, and that's something we thought we needed to capture. The typical commercial spent fuel package is like 8 or 9 kilowatts.

We're using a dual permeability flow model which allows much greater mobility of the fracture water than the equivalent continuum models that were used mostly in the past.

For open drifts, we have a radiative heat transfer model, and like the seepage, we are modelling the drifts as being open, even though in fact we expect that the drifts will collapse after only a few hundred years. That's something that we do not have in our models at this time.

We are doing some sensitivity calculations on what the effect would be.

I already mentioned this; we have a series of linked models from the mountain scale down to the drift scale to approximate the 3-D behavior. We know at the scale of the waste package, it's really important to get three dimensional effects in there because a three dimensional discrete waste package has a different temperature than an infinite line source would. And at the mountain scale, as I said, how close you are to the repository edge and where you are with respect to the topography and the geologic units, that sort of thing is important. So we were capturing all those effects by this.

It is a fairly complicated process, which I am not going to attempt to try to explain here. I think it's worth noting that we're, because of that complication, we thought it was worthwhile to actually do a lot of the calculations two different ways. And the second way gives pretty much the same answer, so we feel much more comfortable about that.

Conceptually, the mountain scale model is simpler. It's a two dimensional cross-section that I showed a few minutes ago. We were not able to get the dual permeability model to run in reasonable run times for that model, so we're using an equivalent continuum flow model, but with a reduced matrix satiation, which means that the equivalent continuum model is modified so that the water can start flowing before the matrix is up to full saturation, and the value that that satiation level is set at is obtained from dual permeability models. So, in fact, we think that these equivalent continuum calculations and these dual permeability calculations correspond pretty well.

We're tying everything completely with the UZ flow section using the same layering and hydrologic property sets and infiltrations that are being used for the UZ flow.

And as was already mentioned before, if we're going the next step into the abstraction down to the waste package and EBS level, we divide the repository into six regions. This dashed line here shows the actual outline of the repository in the referenced design. For simplicity, we idealized that to a rectangle, which is very close to the actual shape, and then as Bob talked about this morning, these six areas were chosen primarily on the basis of trying to follow the infiltration contours, but also to some extent to follow the kind of--the effects having to do with geologic units and with the edge effects, like this unit here, this long thin one is along the edge, all along its length so that we see that it cools off faster than most of the other ones. This one is the only one that's right in the middle, and because of that, it stays hotter a little bit longer than the others. Why six? As I said, it's because that's kind of how many we thought we could handle.

This shows the average temperature curves for commercial spent fuel for those six regions. In each of the calculations, there's a grid of values that are calculated around those regions, and these curves here represent the average over all of those. And as I was just saying, you can see that the CC region stays a little bit hotter than the other ones. The SW region, that's that long thin one, takes--it cools off a little bit quicker than the others. But there's not a huge difference in these curves, and it's important to note that by 10,000 years, they're all very close together, and with the waste package model we have now, almost all the waste package failures are after 10,000 years when these temperatures are almost all the same.

BULLEN: Mike, before you leave that one, if you'd take a look at this curve--this is Bullen, Board--if you take a look at the curve for Southwest, that means that the average temperature of the waste package in as short as, say, a couple hundred years is below boiling?

WILSON: Yes, for average. There's going to be some that are quite a bit hotter, and some that are quite a bit cooler.

BULLEN: So this is the long-term average climate?

WILSON: That's the other thing I meant to mention. This illustration is for the long-term average climate, and that's because we're really putting most of our attention on that one, since that obtains most of the conditions.

The present climate is a little bit warmer, but not by very much. I probably should have shown that, too, but I was trying to keep the number of slides down. It's a pretty small effect.

BULLEN: But this is a representation of more water means that it's a little bit cooler?

WILSON: That's right. Yeah, the temperatures go down a little bit with the wetter climates or with the higher sampled percolation fluxes. But it's not a big effect.

This shows the variability of waste package temperature for the spent fuel within one of the regions, and the particular region this is is the northeast region. This shows the range of values. So as Bob said this morning, these would be--there's actually two curves there though you can't tell it. Those would be the design basis waste package curves, and these would represent some of the cooler spent fuel packages, which are only a few kilowatts. And you can see that the spread in the temperatures calculated is greatest at early times, and as time goes on, it becomes less and less. Once again, by 10,000 years, there's not a very big spread in the temperatures.

I didn't bother to bring curves of relative humidity because the relative humidity and temperature are very closely tied. Usually if you look at the temperature and you know the way to think about it, you can figure out pretty much what the relative humidity is doing.

This shows for the five base case flow fields, Jack had that little tree chart that he showed this morning of the five base case flow fields, this shows the temperature results for those averaged over region NE, and the biggest effect is simply in flux. We have three fluxes represented. This red curve is the nominal infiltration value that was calculated by Flint and Hevesi, and then we used a range of three times higher and three times lower, and you can see how that changes the temperature.

Once again, this is for LTA climate. You can see that the lower flux makes it just a little bit warmer, and the higher flux makes it somewhat cooler. And there's a huge range in the fluxes here. This I times three is 90 millimeters per year, so that's really getting up there.

And secondarily, the other factor that's varied in these is the fracture alpha, because that was determined to be one of the most important hydrologic parameters, not only for the drift scale as in the seepage model, but even at the larger scale, but you can see that there's very little effect on that one, not nearly as much effect as the flux variation.

Then I thought I should show something representing the mountain scale thermal hydrology. This shows the air mass fraction in the repository at the center location and at the edge location. This is for present climate instead of long-term average climate, and that's because almost all the actions in the first thousand years here, and that first thousand years is in the present climate, so it wouldn't make much sense to show the other.

You can see that there's a big difference between how much the air mass fraction gets reduced, and probably somebody--the air mass fraction means the fraction of the gas that is air, i.e. oxygen and nitrogen and that kind of stuff. The rest of it, the one minus this is all water vapor. Basically, when things start heating up, you get huge amounts of water boiling off and it drives the air away from the repository for a period of time, and that's why the air mass fraction goes down. This is something that's potentially important for the waste package corrosion, because if there's not oxygen, that's going to lower the corrosion rate. However, it turns out that this first thousand years makes no different whatsoever to the waste package corrosion model.

It also is important to the geochemistry, however, like in the estimates of how long it takes the concrete to carbonate, the reduction for a period of time in the air mass fraction makes a difference.

BULLEN: Mike before you leave that one, based on the data that you're going to get, or you are getting from the drift scale test, and the fluctuations in relative humidity that you have observed with essentially barometric pumping, would you expect to see changes in the air mass fraction as a function of time, or would you expect to see variability along this? I know that these are just average air mass fractions for certain precipitation and climate, but would you expect to see changes based on the fact that, you know, where the data that you're getting from the drift scale test, it may not be that you have such a homogeneous mountain that allows you to drive all the air out.

WILSON: Well, maybe you know something about results from the drift scale test. I haven't seen anything from it personally, so I can't really answer your question.

BULLEN: Well, the knowledge that--I've seen some information that basically tells me that there's changes in relative humidity that are measured that fluctuate between, say, 25 and 45 per cent, and they close--the barometric pressure changes. Now, one of the explanations was that there's a leaky bulk head. Well, I would argue you're going to have a leaky mountain. And so--

WILSON; Well, in fact, we do have quite high permeabilities in our calculations.d

BULLEN: I know. But do those permeabilities respond to barometric changes? Does your mountain respond to barometric changes as does the drift scale test, is my question?

WILSON: I'm not quite sure how to answer that. The permeabilities that we're using come from a series of calibrations to pneumatic data that LBL did, in which they looked at the pneumatic pressure variations in the ESF primarily, and in bore holes, and matched those to the absorbed ones. So I believe that the air permeabilities we're using match that well. As to specifically how it relates to the drift scale tests, I can't tell you.

BULLEN: Okay. I would just suggest that when the drift scale test data become more formalized, that you might want to revisit how much of the air you can actually drive out of the drifts for long periods of time.

WILSON: Oh, sure. I think we're eagerly waiting the information from it, and we've already gotten some good information from the single year test, and we expect a lot more from the drift scale test, but unfortunately, almost all of it after the VA.

BULLEN: Right.

WILSON: Okay, to sum up, the drift scale thermal hydrologic quantities, that is temperatures and relative humidities and liquid saturations inside the drift, are obtained from a multi-scale method that accounts for mountain scale and drift scale processes, and it's been tested against a 3-D thermal hydrologic model for center and edge locations and agrees pretty well.

The gas quantities are obtained from a 2-D mountain scale thermal hydrologic model. We're using--I think we have a pretty good advance over the thermal hydrologic models that have been used in the past, in that we are using models that allow much more mobility to the fracture water so that it can drain more realistically.

We are maintaining consistency between the UZ flow and transport and the thermal hydrology. And the last bullet is a little bit a non sequitur, I guess, but the drift seepage probably should have been on the other summary slide, it's being calculated with isothermal flow models. So the thermal effects on seepage are neglected, as I say, as kind of a side calculation, we intend to look at that, but it's not part of our base case models.

BULLEN: Thank you. Questions from the Board?

SAGÜÉS: Yes. Coming back to the figure that you presented on the air mass fraction, first of all, what is year one; that is when the repository is closed or--

WILSON: Oh, the time zero is when the waste is emplaced. The repository closure has no effect on these models. We are not modelling anything special as happening at the time of repository closure. So waste emplacement is the only time that enters into this.

SAGÜÉS: Because of course then in the first so many years, the repository is going to be ventilated and all that, and that is not taken into account on that; right?

WILSON: The current plan is for the drifts not to be ventilated after the waste is emplaced. I mean, they may change that, but in the current design, the drifts are not ventilated after emplacement. They're not really sealed either, however. The doors at the ends of the drifts are open to air flow, but it's not ventilated.

SAGÜÉS: But that of course will represent a significant amount of air exchange with the--

WILSON: There could potentially be a lot of exchange between these because of the drifts not being sealed, and I do not know what that effect would be.

SAGÜÉS: And that assumes that all of the drifts are populated with cans of the same type and all that; is that correct?

WILSON: That's right.

SAGÜÉS: I see. What causes that sudden decrease in the air mass fracture about the 150 years or so?

WILSON: This here?

SAGÜÉS: Yeah.

WILSON: That's just when the repository is getting really hot and it's generating so much steam that it drives all the air away. The one that is kind of mysterious is this early part here, and our best guess as to what's going on there is that it's caused by an early influx of condensate drainage that brings a bunch of water into the drift that then gets vaporized and so you get excess steam generation in this period that dries the air out for a period, and then that condensate drainage pulse goes away, and so the air mass fraction goes up for a little while before it finally goes down again.

BULLEN: Other questions from the Board? Parizek?

PARIZEK: Parizek, Board. Do we know what the niche test injection rates are equivalent to in terms of percolation flux to where you're actually putting water in the invert in order to reduce leakage into that--

WILSON: I can't remember the number for the injection rate, but it corresponds to an extremely high percolation rate. I was trying to estimate that, and it's at least 100,000 millimeters per year for local percolation.

PARIZEK: Yeah, even the super pluvial would be less than that, so that's really--

WILSON: Yeah, that's right. They have to be able to see something within their lifetime.

PARIZEK: It's also very close to the--

WILSON: Yeah, it's only, oh, three feet up from the top.

PARIZEK: I like Figure 6 in color, the spider web effect around those emplacement drifts where you have fracture permeability enhancement, because the niche test permeability data taken before the niche was dug in and after it was dug in show, what, a factor of 6 to 100--

WILSON: I don't remember the number, but it was quite a lot.

PARIZEK: Yeah. So that's again consistent with this spider web thing you have around the side, which could shunt flows in a beneficial way or a harmful way. I hope this is under investigation.

WILSON: Yeah. Well, that's another thing pretty much in the future work category.

BULLEN: Other questions from the Board? Panel?

BAHR: You said something in passing about the fact that you expected that after about a few hundred years, the drifts themselves would collapse, and that raises all sorts of questions about your model of the drift seepage, because as Dick Parizek mentioned, you've got this fracture permeability just due to the creation of the drift itself. When this drift collapses, what else is that going to do to the permeability distribution and could that enhance the dripping or could it cause more diversions around the edge? I don't know how serious that comment was, and it was sort of made in passing, but it made me start questioning your whole seepage model.

WILSON: Well, I believe that after not that long a time, the drifts will be filled with rubble. So we aren't quite modelling the right problem. My feeling is it's not going to make that big of a difference, but I don't have any calculations to point to you that support that.

BAHR: Would there be a way to--your drift seepage models are based on a particular sort of geometry. Should you maybe be modifying that geometry to make a very jagged boundary perhaps as a sensitivity study?

WILSON: Yeah, we certainly need to do some models of that situation, yes. However, I actually think the bigger problem than that is just the whole question of what is the effects of discrete fractures. We're modelling this with the fracture continuum model, and I don't know how that would relate to a model that has actual discrete fractures in it.

BAHR: Your drift scale, the location of those seeps is not constrained by some sort of random fracture distribution around the edge? What causes the localization of the drips?

WILSON: Well, that comes from the heterogeneity in the fracture field. But it is a continuum fracture field. It's actually a real fracture field. It's taken directly from the air permeability measurements that were taken around the drift scale test. So that 45 years that we're modelling for the seepage is meant--it looks very much like the drift scale test region, but as I say, there's a big question in my mind of how the continuum model relates to the discrete model.

BULLEN: Other questions from Panel? Questions from the Board Staff?

(No response.)

BULLEN: Seeing none, thank you very much, Mike. And we'll stay ahead of schedule. Our next--

NELSON: Just one comment. This is Nelson, Board.

I do not believe that the drifts are going to collapse at the rate at which you've indicated. So just so that there can be a contrary opinion someone in the room expressed.

WILSON: Okay. Well, thank you.

BULLEN: Our next presentation is on waste package degradation in the TSPA-VA, and it will be made by Dr. Jerry McNeish, who is EBS Department Manager for the M&O.

MC NEISH: I did a quick scan of the audience to see how many people were asleep, and it was about 10 per cent, I think, so that's right in line with some of our numbers, too.

Okay, I'm going to talk about waste package degradation, and I just want to locate us again on this Hollywood graphic that Bob has put together. Basically, we're talking about the engineered barrier system and somewhere on here is the waste package degradation icon.

I'm going to quickly run through the reference design. You've seen that many times already today. Then talk for a while about the conceptual model and the bases that we're using for our waste package degradation, and then show some base case results and sensitivity analyses to that waste package model.

You've already seen this, but this is our reference design for the waste package, with a two barrier system, the outer barrier 10 centimeters of mild steel and 2 centimeters of the alloy C-22, and in our modelling, we're using only this waste package size, so we don't have a different size for like the glass wastes.

And if we look at some of the key inputs and outputs to this model, on this side are some of the inputs, the design, both for the waste package itself and also for the repository, how the packages are put in there, the areal mass loading. Another input to the degradation model is the temperature, which comes from the thermal hydrology that Mike just described, also the relative humidity comes from that modelling. And then whether or not we're in a portion of the repository that's dripping or not, that has a big impact on the waste package degradation. Another key input is what are the thresholds that we're using for initiating the corrosion.

Then just some of the basics of the model, we've included juvenile failure in a cursory fashion at this point. We also have degradation of the outer barrier, both due to general and localized corrosion, and also degradation of the inner barrier, again, due to general and localized corrosion.

And then the output from the model is a number of things which are fed into our total system performance model. What is the timing of the first pit or first patch that penetrates the waste package, and then also through time, what is the overall exposure of the waste package according to the degradation?

Our model discretizes the waste package into patches, roughly a thousand patches per waste package. And we have some bases for this waste package degradation model which includes both short and long term corrosion testing. We've also called on the waste package expert elicitation panel to help shore up some of the areas where we don't have explicit data, and then we've used some data from other sources in developing this model.

I'll talk briefly about the juvenile failure. Bob has already mentioned this, but I just want to cover it again. The early failure is assumed to be due to manufacturing defects, handling problems which weaken the package so that it fails in an early time frame.

Within the M&O, there's been some analysis already of weld failures which came up with the probability of 10 to the minus 6, 10 to the minus 5 for failure of the double walled container. So in our case, that's not going to have an impact, because we have roughly 10,000 packages.

There's also been some Canadian analyses of this problem, and they've indicated on the order of 10 to the minus 3 probability, so one out of a thousand packages might experience early failure.

So we developed a distribution that runs from 10 to the minus 5 to 10 to the minus 3 loguniform, indicating we don't have a good idea basically on that range, you know, where things fall.

And then our deterministic case, you've seen results already, has that single failure with the single patch at a thousand years. And if you look at these numbers, that could show at the worst case we could have approximately ten packages failing for a single realization. And the juvenile failure packages are only in dripping zones.

WHIPPLE: Is that bad luck?

MC NEISH: Yes. In the non-dripping zones, we just said that there wasn't going to be much release from those packages anyway if they failed early, so we just isolated them to the dripping zone.

This is just another look at the conceptual model, some of the key inputs from the drift scale, thermal hydrology in the seepage model. We're also getting pH of the dripping water from the near field geochemical environment abstraction. And I just wanted to note that the size of the patches in our model is roughly 310 centimeters squared.

BULLEN: Jerry, before you leave that, Bullen, Board. We learned a little bit this morning that when there was more infiltration and more seepage, that the temperature was cooler, and that maybe since the temperature was cooler, the waste packages failed more slowly. Is that encompassed in your temperature dependence of failure? Do you have that kind of temperature dependence that's affected by the temperature changes associated with the higher infiltration rate, or is it not fine tuned to that level of detail yet?

MC NEISH: You would just have a different temperature and relative humidity curve that would feed into WAPDEG.

BULLEN: But those are currently in the model and that's just one of those things you just turn the crank and it goes?

MC NEISH: Yeah.

BULLEN: Okay.

MC NEISH: You know, we'll run the thermal hydrology with that higher infiltration and that will give us a slightly lower temperature, which then would have a, you know, slower package failure once you feed it into WAPDEG.

This diagram tries to walk through the basic logic in the waste package degradation model. The code itself is called WAPDEG, W-A-P-D-E-G. Basically, there are a lot of switches throughout the model where we're checking, okay, do we have the right temperature for corrosion, do we have the right relative humidity, are we in a dripping zone, do we have the right chemistry to initiate certain types of corrosion, things like that. So that as you step through this logic, you come upon these switches and you decide either you're heading down a faster corrosion path or things are slowing down.

Again, the inputs are shown here, temperature and RH and the dripping, and also some information from the near field geochemical environment model.

Now, in the base case, which has been presented so far, the only parameter coming out of here is pH that's feeding this model. We're also developing this other information, but it's not currently hooked into the base case evaluations.

The big switch is whether or not we're in the dripping zone, and if so, then we start heading down the quicker failure path. These asterisks show the fastest failure path. But to stay on that path, you have to have the high pH water, which as has been mentioned before, we assume that the liner fails fairly rapidly, so we don't have this kind of a high pH incoming water to really throw us into this high aspect pitting corrosion.

So primarily if you're in a dripping zone, you're coming through this pathway to degradation of the outer barrier through aqueous general corrosion, on down to checking again the temperature primarily to determine whether you're going into just general corrosion of the corrosion resistant material, or whether you might also have some localized corrosion.

Now, in the areas of the repository where we have no dripping and we come through this, this portion of the model where we're evaluating testing for relative humidity, and if we have basically relative humidity from 70 to 80 per cent, then we have just humid air, general corrosion of the outer barrier. Otherwise, if we're higher than that, we can have aqueous general corrosion of the outer barrier. And then on down through the CRM general corrosion.

Again, the output will be either time history of the patches as they fail, or if we've had some localized corrosion of the CRM, then we might also have some pit penetration as well as the patch perforations.

These next couple slides are basically a reiteration of what was on that logic diagram. In October of 1997, Joon Lee provided the details of the outer barrier degradation model to the TRB. Basically, we look at humid air, general corrosion and also aqueous general corrosion, and look at it as a function of time, temperature and relative humidity. For the humid air--well, for both of the localized corrosion models, we used a pitting factor on top of that general corrosion model.

For the CRM corrosion, again, we have general corrosion either in a non-dripping or a dripping condition, and most of this information that we're using in WAPDEG was developed as a result of the waste package degradation expert elicitation. They in turn used some--or looked at some data that came out of our long-term corrosion test facility, as well as some short-term data and whatever literature data is available.

The next figure is results from the expert elicitation and it's a compilation of the elicitation from each of the experts that responded to the particular question for a particular environment, so in this case the red line indicates the moderately acidic and the moderately oxidizing environment. At a temperature of 100 degrees C., what was their expectation of what the corrosion rate should be? And they gave it in terms of the probability and the corrosion rate, so a CDF, and basically the group that was conducting the expert elicitation has compiled the multiple answers from the experts and put them into a single corrosion rate curve for each of these environments. And in our base case, we've taken the majority of the cases to be in this moderately acidic and moderately oxidizing environment, and then lesser amounts in these other two curves, which are much more aggressive kinds of environments.

Now, that was for 100 degrees. They were also elicited at 25 degrees and 50 degrees C., and this plot puts together the corrosion rate versus one over temperature to show, and then puts on the existing data to show where the expert elicitation curves come in relative to the existing data that they reviewed for the expert elicitation.

So basically up here is the 100 percentile of the expert elicitation and here's the zero percentile for the expert elicitation, and you can see it captures the range of the existing data, which is quite a broad range. And this uncertainty is a big driver in some of the things that you've previously seen, like the horse tail plot that Mike showed where he had doses over several orders of magnitude. This range in the uncertainty in the CRM has a big impact on that.

I just wanted to remind you of the base case results for waste package failure. Bob showed these figures, but on this axis, we have the fraction of the packages failed versus time. This early failure is just solely--that's the outer barrier failure, and then starting in here, we have the first pit or first--I'm sorry--the first breach curve, and early on in here, there are a couple packages that failed by pitting, but primarily the packages are failing by patches, by general corrosion of the CRM. This curve here shows the packages which are failing by just pitting.

And you can see that at a million years, the number of patches that have failed per waste package is still--it's not that large. It's only about 40 per cent of the packages that have 10 per cent of their patches fail, because there's roughly 1000 patches per package. So there's some packages which have a significant portion of the waste package failed, but many of them have only a few patches that have failed.

Now, I want to move into the sensitivity analyses. I want to look at just a few sensitivity analyses. There have been some questions about the wetting of the waste packages. In our base case, we assume that the whole waste package is getting wet, and so we want to take a look at what happens if you're only getting, you know, 1 per cent or 10 per cent wet. Also, we want to look at this uncertainty and variability in the CRM corrosion rates, and then look at the juvenile failure effect.

There are additional cases that we're looking at now, but I'm not going to show any results on it. These will include things like a ceramic coating on the waste package, drip shields, two types of drip shields actually, and then some combinations of those things. All of them will have a backfill. Those results should be presented in our May 15th document.

So this figure shows the sensitivity of the waste package failure to the fraction of the waste package that is wetted, and this, as I mentioned, is our base case. 100 per cent of the package in those dripped on areas gets wet, and then this shows sensitivity if we only have 10 per cent of the package getting wet or only 1 per cent of the package getting wet. And there's a tremendous difference in terms of number of packages that have failed.

For the case where you only have 1 per cent, only roughly 60 per cent of the packages ever fail in the drip zone, but if we take our base case assumption, we've got 100 per cent of them failing.

SAGÜÉS: Excuse me. Just to make it clear, fraction of packages fail, and that would be a waste package surface fraction wetted. That's not the number of packages in drifts; right?

MC NEISH: No.

SAGÜÉS: Of those who are being dripped on, you're saying--let's take the case that the entire package is being wetted by the drip; is that right?

MC NEISH: Right.

SAGÜÉS: That would be the blue line?

MC NEISH: Right.

SAGÜÉS: And the other ones, it's only 1 per cent of the package surface gets wetted by the drips. Thank you.

REITER: Leon Reiter, Staff. What's the definition of failure?

MC NEISH: One patch or pit, but in these, it's primarily patch failure. So a general corrosion patch failure. And in our model, that means that the waste form is exposed to the drip conditions and waste form degradation can start.

SAGÜÉS: You know, this doesn't seem to agree. You have a figure, a number 12, I believe, shows the base case expected performance, analysis of results. You know, if I look at the--maybe you ought to pull it up--yeah, if you look there, the green line for the first pit, that looks to me like even if you have like a 1 per cent, 5 per cent by something like 1,000 years, they would have pits already; right? Because that green line, although it's very close to the--yeah, it already would have a few. But you said failure by other pitting or by--see, why doesn't the blue line start a little earlier?

MC NEISH: That's a good point. This one may just be from patch failure. Yeah, that's a good point.

On this one, when we had the early pit failure, I think that was only in a couple packages. But you're right, it doesn't show up on this one.

Primarily for the base case anyway, the package, the first breach is due to the patches, but there are a couple early pit failures that show up in here.

Just to continue on with the percentage of the waste package that gets wet and how that impacts our degradation, at a million years for this model, our base case, which assumes 100 per cent of the waste package surface gets wet, shows this kind of a profile where we have again, you know, 40 per cent of the packages have about 10 per cent of their surface area degraded. Then if you look back at the other percentages, for the 1 per cent case, we have very few patch penetration even in those packages which have a penetration. And that has an impact in terms of our seepage into the package because that's linked to how many patches or what surface area we have degrading.

Another important aspect of the waste package degradation model is the variability and uncertainty in the system. So we have variability from conditions in the drift environment and the waste packages themselves, which can contribute to a broad range of degradation. Also, there's significant uncertainty in the corrosion rates, which is going to contribute to a broad range of degradation. And we have evaluated this using a split of the total variance for variability and uncertainty to try to capture this possible range.

And the model indicates that our most rapid failures are due to the high variability and high percentile of uncertainty cases, and our best performance comes from the low variability case where we're sampling from the low percentile of uncertainty, the low range of the corrosion rates for the CRM.

And this just shows the impact of choosing from either the low end or the high end of the CRM rates, and we get a dramatic difference in the failure rates. If we're selecting from the high end of the CRM corrosion rate, we have significant failure between 10,000 and 100,000 years, and in the other cases, we're well beyond 100,000 years before we have many failures at all.

So trying to narrow or better define that CRM rate seems like it should be a high priority in terms of trying to reduce this type of uncertainty as well.

This is showing our dose and how sensitive it is to the CRM corrosion rates, and we're getting, you know, basically about four orders of magnitude difference in the doses based on sampling from either the 5th or the 95th of the CRM corrosion rates.

Here's our expected value case in here. For the low end, you see this peak from the juvenile failures is the main thing on there. But it points out the extreme importance of trying to understand that CRM corrosion rate.

And then the final sensitivity plot just shows the base case, which is shown here in black, versus the case which has no juvenile failures. For the base case, we've got this early peak. With no juvenile failures, our doses don't start until closer to 10,000 years. And this is just for the single package failing at early time. If we were to have a worst case where we had up to our maximum of ten, you might imagine that you would multiply this by, you know, ten. So it's still below our peak, but it becomes a little bit more significant if you have that many early failures.

BAHR: Can I ask a question about that last figure? This is Jean Bahr.

What's the source of the doses at 7,000 to 10,000 years? Because your previous figures only showed failure starting around 10,000 years or so, 20,000 years for the worst case where everything was wetted. So there must be some other failure source?

MC NEISH: No, there's a--the Figure 12, I think--

BAHR: 14, Page 14, for example, your 100 per cent case shows nothing until about 20,000 years.

MC NEISH: Yeah, but this is the one that--I mean, it's what Alberto pointed out. We have some failures here at a couple thousand years.

BAHR: So pits are--you're considering releases from pits, single pits?

MC NEISH: right.

BAHR: Okay. And that small amount there is responsible for what's coming through in that first 10,000 years?

MC NEISH: Right.

BAHR: Okay.

MC NEISH: So this--I don't know how many packages it is, but there's a couple packages that failed before 10,000 years, and those are contributing to that early release.

Just to summarize, the waste package degradation model includes juvenile failure model and degradation of both the outer barrier and the inner barrier, and it's supported by a significant amount of lab and field data, as well as expert elicitation. In particular, the CAM is supported by a lot of data, and the CRM is supported by our expert elicitation.

The primary factor affecting our long-term waste package performance is dripping or no dripping. I didn't show a figure, but basically if you're not in the dripping zone, then your packages don't start failing until 700,000, 800,000 years.

Factors which are not considered in this model include such things as microbially induced corrosion, stress crack, corrosion cracking and also structural failure of the waste package at a late time to where we wouldn't have any isolation capabilities from the waste package. And these items potentially could have negative performance implications.

I think in the VA document, there's a possibility we'll have some MIC evaluation and perhaps some structural failure evaluations, but we probably won't have anything on stress corrosion cracking except just a discussion of it.

And then finally, some of the key data requirements are, you know, which we've already mentioned before, are evaluating the dripping and trying to get a handle on that, how many packages are actually going to be wet, and then also in some way trying to substantiate the waste package degradation and expert elicitation results, especially the CRM corrosion rates for the conditions that we expect in the repository.

BULLEN: Thank you, Jerry. We'll ask for questions from the Board first. Nelson?

NELSON: Nelson, Board. Two things. First, every time in your plots you see--I see an axis labeled fracture and a package has failed, or fraction of packages. That always refers to those packages that are in drip zones only?

MC NEISH: Yeah, in the ones that I've presented.

NELSON: In all of these figures?

MC NEISH: Yeah.

NELSON: Okay. The second thing relates to your key additional data requirements in terms of additional evaluation of dripping. I'm wondering if you could tell me what specifically you might think about doing in that context? And I must admit that we met about two weeks ago, at least part of the panel that's here, dealing with what might well be called rock mass degradation and lining degradation, and most of the analysis that we saw that related to how the lining would deteriorate, which has a strong impact on how seepage is going to develop, were really related towards the lining stresses themselves and not so much in how the rock mass was deteriorating. And I really think that not only grabbing information about drips that you might by going to Rainier Mesa or one of the other easily accessible analogs or tunnels, that makes sense, to acquire drip information is important, but also to really--if there has been an analysis sequence that really tells us how the rock mass is going to deteriorate from the standpoint of PA, I don't think we've seen it and we'd very much like to see it. And if it's not, we'd like to know it. Because I think that a major part of how the water is going to re-establish itself in the near field has to do with how the rock is deteriorating and how the fractures and the joints are going to behave as the water moves back in.

MC NEISH: Yeah. We haven't done those analyses in detail and we need to do them. The thermal hydrology people have started to do some evaluations where they're collapsing the drift in a discrete fashion to try to see how that's affecting the return flow. But we don't have any results on that.

NELSON: Yeah. And they've done some things with some of the mineral precipitation that Steve Frishman was talking about before, but I don't think it's been an integrate analysis so much as it's been like Jim Blink's drawings that indicate a process that is suspected to happen.

MC NEISH: I think that's where it is, and we're taking some steps. I mean, in '99, we're planning on doing some-- yeah, it's the type of analysis that needs to be done for the LA.

BULLEN: Other questions from Board members? Sagüés?

SAGÜÉS: yes. Have you gone through the exercise of assuming that you have 100 per cent juvenile failure? I say this because of the following. That will give an indication of how much--what would remain of the defense in depth for the entire repository concept if one would eliminate this part of the engineer--it specifically will illuminate what would remain to be ascribed to the waste form in terms of cladding and so on.

MC NEISH: We haven't done that analysis.

SAGÜÉS: I'm very curious to see what it would be, because it would be just simply turning off one component of the system.

MC NEISH: Right. We've kind of avoided that from a realism standpoint for one, but also the politics of showing something like that are not great. But we could very easily do that kind of calculation to see exactly what you're talking about, and we've done the analyses where we look at various sub-system performances, you know, what happens at the edge of the waste package, at the edge of the EBS, and things like that, so you see how much each of those components are buying it, but we haven't turned that switch to totally get rid of the waste package.

SAGÜÉS: Because the Figure 19 that shows the sensitivity of those to the juvenile figures and the like near the end, this would simply indicate that the repository is relying more and more on the waste package as the isolation, the main isolation device. But, again, I don't know what happens if we take it out completely. Maybe something remains from the geologic part of--

MC NEISH: Yeah. Well, Mike's analysis showed, you know, the waste package is definitely the key driver.

SAGÜÉS: Thank you.

BULLEN: Jeff Wong, Board?

WONG: This is Jeff Wong, Board. Can we look at the graph on 12? Now, I'm a biologist so I'm confused. In my small thinking, a patch means something that covers a hole, but for you guys, a patch is a hole.

That graphic on patch penetration, I mean, I'm looking at your legend and I'm looking at my legend, and I'm confused. That upper blue line up there is for a million years or for 10,000 years? Because on mine, it looks like it's for 10,000 years.

MC NEISH: Well, I don't know which one you have.

WONG: I mean, it looks to me like that line would indicate that you'd have more failures at 10,000 years, which is not--

MC NEISH: No, 10,000 is the black line.

WONG: Okay. That's black? I have bad eyes then, too.

MC NEISH: The first few curves here--well, 10,000 years is not visible on this one.

WONG: I have one suggestion then if you do that again, try to expand your scale so people can see way down there in the edge.

MC NEISH: Okay. Sorry about that.

BULLEN: Other questions from the Board? Questions from the Panel? Steve Frishman?

FRISHMAN: I'd like to follow up on what Priscilla started, and that's that I think you probably do need to look at both the liner and rock mass barrier scenarios, because I think in the time periods you're talking about, the one thing you won't have is drips. You'll have water contacting that waste package somehow, but probably not like Mike was showing, as a drip off somewhere above the spring line. And anything in the way of contact and water contact between a piece of rock and the metal and metal that may have been one way or another physically disturbed by a piece of rock is probably going to accelerate the failure rate rather than decrease it. But I think it's probably important to think in terms of for the time periods involved, while your whole case here is sort of relying on drips, drips are probably the one thing you won't see.

MC NEISH: Yeah, in fact we call them drips, but the model is not so sophisticated that it really is a drip. It is water contact on the waste package. Whether that is affected more by having the rock there as well--

FRISHMAN: But is it free water contact, or is it contact with something else involved as well? And that does make a difference.

MC NEISH: Yeah. Yeah, that's a good point.

BULLEN: Any other questions from the Panel? Questions from the Board Staff? Carl Di Bella?

DI BELLA: Jerry, if you could turn to your overhead Number 10, where you have elicited opinions from your waste package panel of experts for various environments, what actually are those environments? Do you have some sort of quantitative description for them? And more specifically, where do they come from? I mean, why are they whatever they are and what's the distribution of those environments?

MC NEISH: I have a little bit more detail on it. Maybe--is anybody here from the expert elicitation that could answer that?

SHOESMITH: David Shoesmith, Mine Geological Disposal Board, one of the members of the expert panel. I apologize. I was talking to somebody else. I didn't hear the question.

MC NEISH: Carl was just trying to get a better definition of these environments that you were asked to elicit the corrosion rates for.

SHOESMITH: Oh, for those? Of the three environments, the moderately acidic I don't understand because I would have called that neutral, moderately oxidizing, that's what we would envisage would occur if you had no drip and general distribution of humidity and temperature, so there's no chance of pushing the pH down. And the second one, acidic and moderately oxidizing, was what we envisaged to be possible, between the CAM and the CRM, so on the surface of this material, you might drive the pH down, but if you don't get a--from the corrosion products on the CAM, it's not aggressively oxidizing. So it's a little bit more acidic, but it's not oxidizing. The bottom one is the worst case, if it's sufficiently oxidizing to drive localized corrosion.

I know it's--for me to ask a question right now, but I haven't heard any discussion of localized corrosion of the CRM in this presentation, so I'm confused a little bit about how you got to your wetted surface predictions. I should reserve that question for tomorrow. I apologize. I couldn't keep my mouth shut.

MC NEISH: Does that answer? I think also the expert elicitation report, which is supposed to be coming out within a couple weeks, should have a clear explanation of what environments they've looked at and why.

DI BELLA: Carl again. Don't go too far, Dave. Did these environments come from the panel, or just the distribution of the environments? I know the cumulative distribution functions of corrosion rates came from the panel. But how about the environments themselves, where did they come from? From the panel?

SHOESMITH: Well, the environments came from an iterative procedure. There were calculations at Lawrence Livermore on how you might go, estimates of what the concentrations of those environments would be, predictions by Joe Farmer on what the pH--what pH you could get between a carbon steel--a crevice between carbon steel and the CRM, so that's where it started. And then we argued about whether the ferric iron concentration could drive the pH any further, and whether it could produce the oxidizing conditions. So the final environment, acidic and highly oxidizing, is the one that came out in the expert panel. Acidic and moderately oxidizing is that that was predicted by Joe Farmer's calculations. There was general agreement that this was a reasonable set of environments.

BULLEN: Bullen, Board. Just to follow up on what David Shoesmith said, there was not a detailed evaluation of localized corrosion in the corrosion resistant material, but that's folded into the failure distribution curves that you have, and I assume that there will be a detailed analysis in the VA report that we'll be able to understand.

MC NEISH: I assume so, too.

BULLEN: You assume so, too. Okay.

Any other questions from the Staff? Leon Reiter?

REITER: Leon Reiter, Staff. Jerry, I'm trying to sort of figure out the relationship of your Figure 18.

MC NEISH: This one?

REITER: Yeah. I'm trying to relate that to the uncertainty discussion we heard earlier today by Mike Wilson, and Mike showed in one of his plots some uncertainty in dose time histories for the base case, and he showed a number of realizations, 60 of which had actual doses, 20 had zero doses. And as Priscilla pointed out earlier, that spread is about eight orders of magnitude. Okay? And what we see here is a spread of about four orders of magnitude, and this spread appears to occupy the upper end of Mike's plot. In other words, this seems to be at the high dose level.

Now, Mike in some of his partial correlation coefficients looked at the factors that caused some of the uncertainty. Number one was seepage, and then the others were various aspects of C-22, and for some reason, juvenile failure, which I don't see being an important factor here.

So I'm trying to understand how your plot relates to the kind of stuff that he did and why is it that you occupy the upper--have the higher doses just attributed to C-22? Is all the variation in the lower doses just due to lower seepage? Maybe you can put it all together for me.

MC NEISH: Maybe. I think one of the things is that these 5th and 95ths were pulled off of, you know, something like this. And if we look down here to the 5th, we've got potentially a pretty long tail on that, which could be giving us those zero dose releases. If we say the CRM corrosion rate is even off of this figure, then that's where we might be seeing some of those zero dose releases. But this takes the value for the CRM corrosion from the 5th percentile of the distribution. I don't know how the tail looks on that.

REITER: Well, it's not so much the zero doses. I'm trying to understand how all the sensitivity and uncertainty tests relate to each other, and I have trouble doing that.

MC NEISH: If you put his horse tail plot up on top of this, it's going to be pretty close.

REITER: Well, but it looks to me like your four orders of magnitude of uncertainty occupy the upper four orders of magnitude of uncertainty in Mike's plot. But yet Mike also indicated that the largest factor affecting it was the seepage fraction. Now, since you haven't affected seepage fraction, I mean one could make the assumption, well, seepage fraction is going to lower that does. I can't figure out how all these uncertainties and sensitivities weave together to give me a consistent picture. It could very well be that I don't understand what's going on. I may be misunderstanding something.

MR. ANDREWS: Let me try it. This is Bob Andrews, M&O.

When Mike runs his multiple realizations, everything is changing. You know, infiltration is changing. Percolation is changing. Seepage is changing. They're all being sampled, all simultaneously. What Jerry has done here is just say I'm going to just change one thing. I'm only going to change the waste package degradation, keeping everything else fixed at my mean climate, or I guess there's probably climate change in here, but my mean percolation for my expected case, my mean of the seepage, et cetera. And these are specific realizations.

So I think Mike's lower values either correspond to lower infiltrations, which is very possible and, therefore, less seepage, or you're at the lower end of that seepage range given a particular percolation, which also lead to lower package failures. So in this case, for long-term average climate, 30 per cent of the packages get wet. That's not the case in every single realization, though, but it's the case in these three realizations. But Mike showed cases I'm sure where it was zero per cent got wet, and if zero per cent get wet, zero per cent fail in 100,000 years.

REITER: I guess I would feel more comfortable if somehow this range existed somewhere in the middle of Mike's range where all the uncertainties up and down could affect it one way or the other. And, again, Mike, I'm just looking at Figure 4 of Mike's plot, and it looks to me like it's in the upper end. I don't quite understand that. In the higher doses.

ANDREWS: For the cases where there is release, this is probably the thing that's driving it. For the cases where there's no release, I mean, what you're saying is a very good point, that it's very useful to parse out multiple realization runs into different bins, if you will, and see what it is within a particular bin that drove the results rather than look at all the results, all hundred results in there at the same time. And if you did that and looked at those that are in the 20 per cent bin that never failed, then you say okay, what is it that drove me to have no failures in 100,000 years, and it's probably because there was no seepage in 100,000 years in those realizations. We don't know that. I mean, I was just speculating here, and that's a good point that we should look at the different bins of results and parse out within the different bins what drove the results, and that's very doable.

REITER: I guess if you have a logic tree approach, that would be a lot easier.

ANDREWS: Yep.

REITER: Well, the bottom line is I know you have important measures of uncertainty and sensitivity, and it's not quite clear, at least to me it's not quite clear how to separate out these things and put them all together.

ANDREWS: Well, in this particular case, it's clear in both cases. Mike shows it once, and Jerry shows it again. So you have it from two different angles that the CRM degradation rate is significant to long-term performance. Did it explain all of Mike's results? No, there's other factors going on in Mike's results, but it's the number two or three factor in his results, and this shows why.

BULLEN: Any other questions from the Staff?

WILSON: Can I try?

BULLEN: Okay, Mike.

WILSON: This is Mike Wilson. Basically, Jerry is showing something that can lower the dose down by something like two orders of magnitude. But if you pile on top of that then a lower infiltration rate and you pile on top of that a low sampled seepage fraction, you could push that down several more orders of magnitude, and that's the explanation of what you're asking.

REITER: But you also--

WILSON: It's things piled on top of each other, not just one thing that makes it low.

REITER: Right, but it doesn't go down; it could go up.

WILSON: It can go--the highest one was only less than one order of magnitude higher than that, and that could have been because of a high infiltration rate. Remember this is using the median infiltration rate and a median value of the seepage fraction. It can go higher, too.

BULLEN: Any other questions from the Board Staff?

If not, I will declare we take a break until 3:30.

(Whereupon, a brief recess was taken.)

BULLEN: Could I have everyone please grab their sodas and return to their seats so we can get back on schedule? In particular, could I ask Board members and the Panel members to return to their seats up front, please?

In a continuation of our presentation on the TSPA-VA base case, we have Dr. Jerry McNeish who's going to talk to us for the next hour about near field geochemistry, waste form dissolution, radionuclide mobilization, and engineered barrier system transport. So he's got probably four summary slides to which we'll ask questions.

MC NEISH: Before I get started on this one, I want to apologize to the guys that actually did the work on the last one. I didn't acknowledge them. There's a whole group of people in Las Vegas that are working on the waste package degradation modelling effort. Joon Lee is heading up that group, and he has several people working for him, Kevin Mon, Bryan Bullard and Dennis Longsine, I want to acknowledge them because they're the guys that actually did that work.

As Dan mentioned, I want to talk about EBS processes. In particular, I'm going to walk through what we've done in a quick fashion for the near field geochemical environment, the waste form degradation and radionuclide mobilization, and then also for engineered barrier system transport. And I guess to point out on this figure up here, basically dealing with these processes, the near field geochemical environment, the waste form alteration and then radionuclide mobilization and EBS transport.

I'll skip the next two slides. Just to get us a little more focused on the engineered barrier system, this is a schematic of the reference design that's being used in our base case. With the concrete liner in most of the drifts, several different types of packages, commercial spent fuel, high level waste, and we're also evaluating DOE spent fuel. Underneath the packages is a pier and invert system, and so what we're concerned with is once the packages have degraded, then what's the environment there and how does the waste degrade, and then also be released from the engineered barrier system.

For the near field geochemical environment for this performance assessment, we've made some big steps from previous performance assessments which didn't incorporate very much in terms of geochemical information. We've discretized the EBS at several locations to try to compartmentalize it so then we can evaluate the geochemistry at those locations, and define several scenarios based on the thermal conditions, pre-boiling, boiling and post-boiling, and then looked at these locations which are defined based on discrete locations within the engineered barrier system.

Now, we've done some of the initial evaluations of the gas and water compositions at these various locations, which I'm going to show on this next slide. We started to evaluate the geochemistry of the incoming water; that water interacting with the concrete liner. Also another location where we're looking at the geochemistry as right at the surface of the package to interact that incoming water with the iron oxide. And then within the waste form itself, looking at water reacting in there.

There are a couple other locations that we'd like to look at but we haven't accomplished that yet for the viability assessment.

So the abstractions that we're using, or the simplifications of the geochemistry in the viability assessment primarily are looking at these things, developing the gas and the water compositions as a function of time. We've taken as input the results from the modelling that Mike described previously, the 2-D mountain scale thermal hydrology results to get our gas flux and air mass fraction, and then also used data from pore gas and single heater tests in order to derive the air compositions.

The water compositions are then calculated at several locations, and they include effects from the thermal conditions and in-drift reactions, so the materials in the drift, the concrete and the waste package materials, and then also in-package reactions with the spent fuel.

And then from these analyses, we are providing several pieces of information for the total system performance assessment, including pH as a function of time, the total carbonate and also the ionic strength, which is primarily a factor in the colloid transport.

Just an example of some of the results. You've already seen other results from Bob's presentation, but we are using the pH as a function of time for the waste form degradation, and so we're looking at the incoming water interacting with the iron oxide of the package, which gives us a slightly elevated pH initially, and then stabilizing. So that's in the base case.

And then for a sensitivity case, we have mixing of the waters coming in from--into the drift with the concrete liner, and also the waste package.

NELSON: Why don't you have concrete in the base case?

MC NEISH: Because we made an assumption that the liner fell fairly quickly, within a couple hundred years, and so there was going to be basically not much interaction of that incoming water with the liner. And we assumed that the--actually, Blink's drawings show the liner down around the side of the package, and so our water would just come onto the top of the package and be native water interacting with the--

NELSON: Well, the most recent ones that he's got show the things lasting a bit longer, I think. So you say there's no concrete over the first--in the 10,000 years, you don't have the concrete at all?

MC NEISH: In our base case, we don't have it at all, yeah, for the incoming waters.

SAGÜÉS: So the base case considers a non-reinforced concrete?

MC NEISH: Well, in our base case, we don't take any evaluation of the liner itself. So it's essentially not there in the analysis, that liner.

And in this one, I believe he's looking at non-reinforced concrete, but I'm not sure on that.

One of the key aspects of the analyses is what is our source term, and we've again had to do a simplification or abstraction of the inventory in order to be able to model it within our constraints, and we have basically three different waste forms that we're looking at, commercial spent fuel, high level waste, and DOE spent fuel, and we've taken the existing information on those particular sources and then depending on which waste form we're looking at, we've either developed what we call a blended inventory in the case of commercial spent fuel, we've combined the PWR and BWR inventory and assumed that it all goes into 21-PWR packages. And that comprises 63,000 metric tons in the repository.

For the high level waste, we've combined the four different waste types, or from four different locations, Savannah River, West Valley, INEEL and Hanford, and blended that into a high level waste inventory which we assume goes into a five pack, has five glass canisters within the waste package. And that comprises 4,667 metric tons of heavy metal for the repository.

And then the final category is DOE spent fuel, where we start out with over 250 different types, and the people with the National DOE Spent Fuel Program have developed a categorization of that fuel into 16 different categories, which we've been evaluating from the standpoint of dose to come up with our surrogate inventory. And we've looked at what are these--what are the doses from these individual categories and taken the top dose producers and put them into essentially a blended inventory here. This is primarily the N-reactor fuel for the DOE spent fuel, and that's the remaining 2,333 metric tons for the base case repository.

Now, in the EIS which is being conducted at the same time, they're looking at additional waste forms, and also additional volumes or tonnages within the repository.

The next two slides walk through the conceptual model for degradation and mobilization of the waste form. Basically we assume that once the waste package fails, the waste form is exposed to the drift environment so that it has the same temperature and relative humidity conditions as the drift, and we assume that water films are adsorbed on the waste forms.

The waste form degradation is represented by intrinsic dissolution rates for each of the different waste forms. So we have a commercial spent fuel glass and a metallic dissolution rate for the DOE spent fuel. So there's three different dissolution rates there. And then the radionuclides are considered available for mobilization congruent with this dissolution rate.

Then we can mobilize the radionuclides at this dissolution rate either into diffusive or advective pathway. For those packages which are getting dripped on, the primary release mechanism is through advective transport, and for the rest of the packages which are not getting dripped on, they'll fail much later, but their only release mechanism is through diffusive release. And the radionuclides are mobilized at aqueous solubility limits.

We've taken a look at--and I'll present some preliminary sensitivity results on the effect of secondary phases on the release, but the secondary phase formation and then dissolution is not incorporated into the base case.

This slide is just a pretty picture of the waste form, showing the same information that Bob has presented in a little bit different figure. But basically we have our fuel pellets within the cladding. This is a cross-section of that, and then ultimately, they'll be emplaced in the waste package in the drift. And what we're looking at is sort of a lumped source term within the waste package.

Now, we'll move on to Dan's favorite topic, cladding and cladding credit. We've developed a cladding model which is incorporated into the base case, and it has several different processes included. There's a certain fraction that has early time failure. These are rods which arrive at the site already failed. They were failed at the reactor. There's also a portion which has stainless steel cladding, and we assume that that cladding also fails as soon as the waste package fails.

There's a capability for creep strain cladding failure, but this doesn't contribute much to the overall failure of the rods in the model. And then there's two additional components which are contributing a significant amount to the total cladding failure. There's the mechanical failure model which is based on analyses where we assume some conditions for rock fall through the degraded package, and then also we've, as Bob mentioned earlier, we've incorporated a corrosion model similar to the C-22 corrosion. It's actually 10 to 1,000 times slower corrosion than the C-22, and that component is also incorporated in the cladding model.

Now, the next figure tries to lump all these things together to show you the key contributors. The way we implement it in the total system performance assessment is basically exposing the fuel as the cladding fails. And so we--the cladding model gives us a fraction of the fuel exposed as a function of time, and there's a small fraction that's stainless steel cladding, it's a little over 1 per cent, that we assume fails as soon as the packages fail. Then there's the mechanical portion and also the failure due to corrosion.

Now, in the probabilistic analysis, we're sampling over this range. So there could be as low as a couple per cent of the fuel exposed, and as high as about 50 per cent of the fuel exposed at a million years.

BULLEN: Before you leave that one, this is Bullen, Board, do these types of failures only occur in the packages that have been dripped upon, and so that that fraction of fuel exposed is only 40 per cent at best of all the packages? So it's like--

MC NEISH: Well, yeah, it's going to happen in all packages, but primary--well, the most packages that are failing are the ones that are dripped on. So there will be some packages that fail at very late times due to human error or aqueous corrosion, you know, just from relative humidity conditions. And those, once they fail, they'll start in on the cladding failure curve as well. So they will have some surface area exposed.

BULLEN: But the comment that was made earlier was that the ones that don't get wet, do fail by moisture or oxidation, but that doesn't kick in until 700,000 or 800,000 years?

MC NEISH: That's right.

BULLEN: Is that incorporated into this figure?

MC NEISH: Well, no, this is not--it's not sequenced as far as time. You'd have to ship this out for the later term failures. So the corrosion and the failure wouldn't start until you had the waste packages fail at very late times.

BULLEN: All right. But when the waste packages fail at very late times, then there's absolutely no mechanical protection; right? Because by the time you've gotten through all the C-22 by general aqueous corrosion or dissolution or both dissolution, there's nothing left of the C-22, so there's no structure component. So the mechanical failure is just rock fall at that point? I guess I'm trying to put the time frame on this. This is 700,000 or 800,000 years, plus another million?

MC NEISH: For those late packages, yes.

BULLEN: Okay.

MC NEISH: We have very little amount of the fuel exposed in those late failing packages. It's only going to be the couple per cent.

NELSON: I really don't--Nelson, Board--I don't understand the sense of mechanical staring at 10 to the fifth years, the way that shows. Would you explain to me what that is?

MC NEISH: That's kind of hooked into our waste package degradation, so we're saying that you have to have a certain amount of the waste package degraded before you can get a rock actually falling into the package and breaking the rods.

NELSON: So that will only happen after 100,000 years? I mean, I'm looking at the--I don't understand time.

MC NEISH: Well, yeah, that's when it starts, is right around 100,000 years.

NELSON: 100,000 years. But does that make sense with the fact that--

BULLEN: 100,000 years of the packages that have been dripped on?

MC NEISH: Right.

NELSON: Right. But in the areas of tunnel where you're expecting rock fall, you expected the rock to have already fallen and filled up the tunnel by 100,000 years.

MC NEISH: Right. But the waste packages themselves wouldn't have degraded very much until this time period. So that there essentially wasn't an area for the rocks to ingress into the packages. It's a controversial topic. So let's show some results on it.

Basically, this shows our base case, which has cladding in it versus the no cladding case, and you can see at early times, we're gaining over an order of magnitude in terms of reduced dose. By adding in the cladding, at later times, they're coming together. And this is primarily due to the fact that at early times, we've got only those few per cent failed due to the stainless steel. But then once you reach closer to 100,000 years, you start seeing additional surface area exposed from the cladding.

This shows, you know, for the first 100,000 years that there's not much variability in the cladding model. We've only got those few couple of per cent to play with, because the mechanical and the corrosion models haven't really kicked in.

So then if we go to the million year plot and look at the base case, which you've seen many times before, and then the no cladding case, out here, you know, several hundred thousand years we're again getting roughly an order of magnitude difference in the releases.

The explanation for, you know, why is the base case kind of tailing off, you know, with its cladding model which is supposedly still failing rods, one explanation is that the rate of failure of that cladding is slowing down with time, so you're getting less neptunium actually available for release. So you're not really at the solubility limit for the neptunium like you are in the case with no cladding, which has all the waste available for exposure.

And this just shows the impact of selecting between the 5th and 95th percentile for those mechanical and corrosion modes of the cladding, and shows us we get an order of magnitude difference in--from the top failure to the bottom amount of failure, the least amount of failure of the cladding.

BULLEN: Jerry, this is Bullen, Board. Before you leave that one, why is there no significant variation in the 5th and 95th percentile up to, I don't know what, 250,000 years?

MC NEISH: It's primarily because there's not much cladding failing up until then.

BULLEN: So you're at the bottom--

MC NEISH: Nothing much going on, just a couple per cent here, and then you start seeing a big divergence.

As far as solubilities go, most of the radionuclides are released at their solubility limit, and the solubilities that are incorporated into the total system PA are sampled over a range of with a different distribution for each isotope that we're looking at.

And in the current base case, the solubilities are essentially the same as they were in TSPA-95, the prior TSPA. Neptunium is the outlier. We've reevaluated that solubility and the values have been reduced by a factor of 100. Bob mentioned this briefly, so I'll just put this slide up quickly, but we've--in TSPA-95, basically we used information from these data, which were derived from experiments coming at the evaluation from over saturation. So it's expected that those were stable phases. This information is from the recent test by Finn and it's believed that these conditions are a little bit more representative of what we might see in terms of the solubilities. So Dave Sassani is the guy who's done this work. He elected to reduce the solubilities, still keeping in touch with the upper solubilities, but also trying to cover a little bit of the range of the data that's been recently collected.

And I think there's ongoing work to evaluate the neptunium solubility. And as you can see, it's a pretty significant factor. The next slide just summarizes the approach the modifying that solubility.

So if we look at selecting from that new distribution, the 5th and 95th values, you know, it's pretty straightforward in terms of what kind of a dose release you get--or dose rate you get. Changing the neptunium solubility by an order of magnitude, you know, effectively changes this dose by an order of magnitude.

An interesting plot here shows that, you know, you can see that effect at early time, but then once we start seeing more of the effect of the cladding at later times, you don't see that spread any more because you're not at your solubility limit. You're more constrained by the fuel that is being exposed.

Now, one other evaluation that we've done is to try to look at what happens if we do incorporate information from the secondary phase development. As the waste form degrades, it potentially forms secondary phases, which then must dissolve and they perhaps dissolve at a different rate than our original waste form, and so we could effectively get a reduced solubility for that. And we've done some modelling using AREST-CT to try to evaluate that, and there's some preliminary results from that, basically just plugging in the updated solubility, which gives us--again, it's effective reduction is the same as the reduction in the solubility limit.

So it's important to try to continue those evaluations and perhaps do some more experimental work to try to see what, you know, to what degree we expect that secondary phase to control the releases.

This is just looking at the million year plot for the same evaluation. And, again, we see a little bit of coming together at later time as the cladding takes a little bit more effect in the releases.

One other aspect of the TSPA-VA that is kind of a new feature, hasn't been in previous performance assessments, is looking at the colloid releases. We've incorporated a colloid fraction into the waste form, and this effectively can increase the release from your waste package, and decrease the travel time. How significant it is depends on the stability and reversibility or irreversibility of the attachment to the colloid.

We've considered four different types of colloids, and these types are, you know, clay, iron oxide, spent fuel waste form and glass waste form, and we're only looking at plutonium isotopes for this.

The final point here is reversible sorption is considered with the ratio of the amount mobilized on the colloid to the amount dissolved, this Kc parameter, which ranges from 10 to the minus 5, to 10, and that's based on some laboratory data. And I think Dave Sevougian is going to talk a little bit more about colloid transport in the UZ and the SZ.

Just a couple figures on EBS transport. We've talked a lot about the movement of water through the system, and its importance, and this figure just tries to tie all those things together again, looking at dripping flux coming into the drift. Perhaps there's a portion of that water that is diverted either behind the liner or on the tunnel wall. We currently don't incorporate that in our analyses. Then eventually, you know, depending on whether the tunnel is collapsed, we may have either dripping water or water seeping onto the waste package, through the package, and on through the invert and into the natural barrier system.

Another place where we might have some diversion of the water is around the waste package. Instead of all of it going through the holes that are created by the dripping, perhaps some of the water may go around the package. That's not contributing directly to the releases from the waste package.

Again, once we've breached the waste package, we assume that the water vapor--essentially the water vapor conditions in the drift are then transferred to the inside of the waste package.

Most of these calculations for degradation of the waste form and then transport of the radionuclides out of the waste form, out of the waste package, out of the invert are conducted within the total system PA code RIP.

This slide just summarizes the EBS release. It occurs when the waste package is breached. The cladding must be also breached for the commercial spent fuel. For the high level waste, we don't take any credit for the canister that's around the glass. We just assume that once the package is breached, the glass is available for degradation. Then the waste form degrades, radionuclides are mobilized and transported through the EBS in the dripping areas by advection and in the non-dripping areas primarily by diffusion.

This column lists some of the things that might improve performance if we keep water off the packages, or if we are able to defend and say that the cladding remains substantially intact, then that can provide a barrier to the waste form release. If the waste form degradation is slow, that helps improve the performance. But this has to take place. We have to make sure that our packages last a long time so that the waste form is not hot when the package is breached. Otherwise, the waste form degradation is fairly rapid.

The radionuclides can, you know, if there's a way that we can come up with a way to make colloids immobile, maybe there's some filler to add to the invert or something like that, that could improve our performance. And also if we can make more of the packages rely on diffusive release instead of having advective release, then we're doing better there, too.

The final sensitivity plot shown here, and this is evaluating the amount of water that actually goes into the package. In the base case, we assume that all of the water that's seeping can go into the patches that open up, based on the patch area, and what we have here is an evaluation that says, well, perhaps some distance away from the patch also is collecting water. And you can see that that affects things for early times, but again at around the 400,000 to 500,000 year mark, we start having no impact from that effect.

The maximum amount that can go in is ten times the patch area. So that's our factor there.

BULLEN: Jerry, this is Bullen, Board. Is this driven by the fraction of the fuel that's exposed?

MC NEISH: It's driven by the fraction of the package that's open.

BULLEN: Right. But this is also inside you've always got a constant rate or constant area of the fuel that's exposed; right?

MC NEISH: Right.

BULLEN: And so without--if these curves would diverge at the 250,000 year mark like the other curves do?

MC NEISH: Yeah.

SAGÜÉS: Excuse me. When you say seepage into the waste package, you mean there's a hole in the waste package?

MC NEISH: Yeah.

SAGÜÉS: And then water drips inside that hole? And how do you quantify that seepage? I mean, what would be the units over which that mean and 95 percentile, how do you quantify that?

MC NEISH: It's just a flow rate into the package.

SAGÜÉS: The flow rate inside the package?

MC NEISH: From outside to inside.

SAGÜÉS: Inside. But that wouldn't matter too much, would it, in the sense that once you have a hole in the package and the water drips into it, if that's the only hole and you have a typical drip like, say, 50 gallons per years, wouldn't that thing fill up after a few years?

MC NEISH; Well, probably.

SAGÜÉS: Making it independent of the rate.

MC NEISH: I mean, our model assumes that once you have a hole in the top, you also can release out the package so there's no built-up inside the package of water.

SAGÜÉS: In other words, the thing that determines the amount of release is how much water you get in and out of the package? Because the thing is solubility limited as opposed to corrosion rate limited?

MC NEISH: Some of the radionuclides are solubility limited and some are dissolution rate limited.

SAGÜÉS: And those curves have taken both things into consideration, both the corrosion rate and the solubility limits?

MC NEISH: Right. This early portion is due to technetium, you know, which basically flushes. I mean, the waste form degrades and it's available for dissolution.

SAGÜÉS: In other words, you get a concentration of technetium in the water, and you're going to exceed that concentration, and what determines the release is--is that what it is, or is the corrosion rate--the rate of dissolution of the technetium into the--in other words, is the rate of dissolution limited or is it solubility limited?

MC NEISH: Technetium is rate of dissolution limited.

SAGÜÉS: Okay.

MC NEISH: Which is very fast. I mean, the dissolution is very fast. It's effectively not limited. That's the controlling factor.

BULLEN: Bullen, Board. Jerry, what you mean is it's the spent fuel dissolution rate is the limiting factor, not the technetium. I mean, the dissolution of spent fuel.

MC NEISH: Well, right.

BULLEN: So when the spent fuel dissolves, then the technetium is immediately available for release. So the limiting factor isn't technetium dissolution or technetium solubility; it's the dissolution rate of the spent fuel.

MC NEISH: The whole waste form.

SAGÜÉS: Is there dissolution rate, like the corrosion rate of the spent fuel--if that is what is causing it, then it shouldn't be dependent on the--necessarily on the amount of water that is dripping into it. Because the water could come out with a high concentration of stuff, you know.

MC NEISH: Right. And that's kind of what we're showing. There's no much effect.

SAGÜÉS: An order of magnitude.

MC NEISH: Yeah, our factor is an order of magnitude essentially.

Just to summarize, we've included limited near field geochemical environment information in TSPA-VA. We've upgraded our waste form degradation and radionuclide mobilization models from previous PAs. The spent fuel dissolution model has additional terms in it, some of which we use and some of which we don't yet use. We're still looking for information from the near field to be able to incorporate everything. The high level waste glass dissolution model has also been update, along with the neptunium solubility, and we've added the colloid model.

Several things have a significant effect on the EBS transport, including the waste package and cladding, longevity. The neptunium solubility is obviously a very sensitive parameter. Control of water flowing through the system is important, as well as colloid control, although plutonium doesn't show up in the early releases, it is a contributing factor in some of the cases at later time.

Additional data requirements; perhaps better definition of how water is interacting with the waste package and the waste form. It could help us to understand that release. Also, taking a look at how water is released through the package and the invert, whether or not the invert degrades with time in our current case, we don't degrade the invert. It could become--at late time, it could become a potential source of--or potential place for capturing some of the radionuclides. And then also additional definition of some of the geochemistry along the flow paths in the EBS could help with this evaluation in terms of telling us whether or not we're being controlled by certain materials that are within the drift.

So that's all I've got.

BULLEN: Thank you, Jerry. Questions from the Board? Alberto?

SAGÜÉS: I have a question on the chart where you compare a cladding versus no cladding. If I understand that correctly, that would be, I forgot the number, it's this one in here.

MC NEISH: Right.

SAGÜÉS: I find it intriguing, that initial part of it, before the two lines converge after about 200,000 years. I was also looking at, you know, this would go with this other one where you showed the initial. Is that the effect of the stainless steel cladding that is the difference before 200,000 years, the difference between the cladding versus no cladding case?

MC NEISH: Right. Just a few per cent of the fuel is exposed.

SAGÜÉS: Some of the fuel is already exposed, but then why would that make a difference then if you have cladding or no cladding? I don't quite--I mean, I'm just trying to understand why the two curves converge.

MC NEISH: All of the fuel is exposed.

SAGÜÉS: All of the fuel is exposed?

MC NEISH: And in the other case, it's only the stainless steel fuel that's exposed for those packages that have failed.

SAGÜÉS: And why do the two curves converge to one curve then, converge at 200,000 years, and they begin to diverge again? That's the part that I can't quite follow.

MC NEISH: The diverging part I've tried to explain, saying that the cladding in this area, the rate that the cladding is failing is slowing, so that the cladding rate is not at the--it's not continuing at the same pace. And so you're having less neptunium exposed, and so you're effectively not at the neptunium solubility limit. Whereas, in this case, you're at the neptunium solubility limit because all the fuel is exposed. Now, in terms of what's going on here, to try to explain that, I believe--well, I don't know.

ANDREWS: Jerry, let me try. This is Bob Andrews, M&O.

What happens when, and my simple hand calculation had I gone through the rest of it would have shown that the rate of cladding failure, or the total amount of cladding that has failed is very significant for the highly mobilized nuclides, such as technetium and iodine. And failing the cladding over the first, you know, 100 or 200,000 years, those nuclides that are dominating in that case are technetium and iodine, and they dominate in fact over neptunium. So they're more important than neptunium if all the cladding has failed, which is the assumption here. We've just thrown away the cladding.

When you go out to later times now, beyond several hundred thousand years, for the no cladding case, now the neptunium solubility and the neptunium releases start controlling. So there's a difference in which nuclides, and it would probably be best when we illustrate this again to show which nuclides control over which time periods for cases with and without cladding, or with different cladding assumptions. And I'm pretty sure you would see, you know, that--in fact, all of that structure in the first 100,000 years--

MC NEISH: That's due to technetium and iodine.

ANDREWS: It's due to technetium and iodine, yeah.

MC NEISH: But then this up in here is neptunium.

ANDREWS: Neptunium, yeah.

SAGÜÉS: Does the decay have anything to do with it?

ANDREWS: The decay of technetium is 200,000 years, but I don't think--that might have some impact beyond a couple hundred thousand years, but I bet it's minimal.

SAGÜÉS: Okay. That's intriguing.

BULLEN: Other questions from the Board? Panelists? Jean?

BAHR: Jerry, I'm trying to understand for the case of the solubility limited nuclides, what percentage of the unsaturated flux through the mountain is becoming saturated? Do you have any ballpark figures for some times in there, or are we talking about 1 per cent of the flow that you're assuming is in contact with the fuel, with the failed canister, or is it 5 per cent, or is it .1 per cent? You're only going to--it's only the water that's actually coming in contact with the fuel that's going to dissolve those things up to their solubility limit, and there's going to be a portion of the water that's going to be going through other places where it's not contacting the waste; right? And I don't have any feeling for what percentage of the water in these simulations is actually going through the waste and picking up those solubility limited nuclides.

MC NEISH: Well, I think the way it's modelled, it's essentially a mixing cell. So all the water is coming in contact with that waste.

BAHR: So all of the water going through the entire unsaturated zone once you--

MC NEISH: Oh, I'm talking about in the EBS. I'm not sure in the UZ.

BAHR: So what percentage of the water coming through the mountain is going through failed EBS at different times?

MC NEISH: Well, if you look at the footprint of the packages--

BAHR: It's very small.

MC NEISH: Yeah, there's a lot of water that's not.

BAHR: That's not. And do you have a ballpark estimate of what percentage of that is not? That's a significant dilution factor in these concentrations.

BULLEN: Bob Andrews, go ahead.

ANDREWS: If you look at--you know, we have it two different ways. I think it's--if you look at the average, long-term average percolation flux and look at that volumetric flux times the number of packages that get wet, because you have to do that multiplication, you'll find that it's a little less than 1 per cent of the total volumetric flux, maybe on the order of a half a per cent during the long-term average, and the long-term average is what's controlling releases from the EBS and ultimately releases to the unsaturated zone. So it's something in the range of a half a per cent to a per cent of the total volumetric flux is actually seeping into the drifts, and then some fraction of that based on the area of packages exposed, which are changing with time, plus this uncertainty factor, are getting into the packages, and that's probably another few per cent, you know, from 1 to 10 per cent of the amount that seeped actually gets inside a package. So it's in the range of, you know, something less than tenths of a per cent, up to a per cent of the total volumetric flux that's getting into the drifts and getting into the packages.

BAHR: That might be a useful sort of thing to illustrate. I'm trying to get a feeling for how much of the dilution is occurring in the unsaturated zone, to the saturated zone, versus how much of it is taking place as you move down gradient in the saturated zone. And you need that kind of information to be able to tease that out.

MC NEISH: You're right, because when we go from the six regions at the repository level, we may have only 40 per cent of the packages in a region that are contributing, but then that is spread over that whole region as it goes--

BAHR: Right.

ANDREWS: Roughly 1000 of it is in the unsaturated zone and 10 of it is in the saturated zone. So most of it's in the unsaturated zone.

BULLEN: Jean, do you have any more questions?

BAHR: I guess just a comment. That's when preferential flow through the unsaturated zone could become very important, because if there's some mechanism that actually focuses the flow into the areas where the canisters are failing, then you may be--and you have a solubility limit, you may not be--you may have a much greater flux than you would get if you assumed some sort of random heterogeneous system.

WHIPPLE: Chris Whipple. Jean's question prompts me to follow up. It seems a simple zero order analysis would be that the fraction of water or the concentration of neptunium could be roughly guessed by comparing the area of the patches with the total footprint of the repository, assume that the water that goes through the patch area is at the solubility limit, and the rest of the water is clean, and stir. And it would be interesting to know what at different times that is before you get to the SC, compared to, you know, drinking water limits and such things. Because I would think that that area of ratios, coupled with a low solubility would put you in a pretty good place.

BULLEN: Other questions or comments from Panel? Staff? Carl Di Bella?

DI BELLA: Jerry, your last talk, the one before the break, I asked you a question about the chart that showed the various environments that the corrosion resistant material would see. And you and Dave Shoesmith handily addressed that question. Now I'd like to ask this question. If those environments can exist on the corrosion resistant material, when the corrosion resistant material fails, why wouldn't those same environments exist then on the waste form? And if that's so, are you taking them into account, particularly the acidic environments and the highly oxidized ones?

MC NEISH: You're right. Those conditions would exist in the package, and it's on the books to do those analyses, you know, the interaction of the waters as they come through the package and into the waste form, but we aren't complete with that yet. We intend to do that, but I don't know whether it will be done for VA. Right now, there's no effect other than the pH or the carbonate, there's no effect on the waste form degradation caused by this environment that you're talking about.

DI BELLA: Right. But the pH range that you are analyzing is like six to ten; right? It doesn't go down below six?

MC NEISH: Right.

DI BELLA: Okay. Are you, in the documentation, going to explain this?

MC NEISH: You know, there's a technical basis report that will be done towards the end of the summer, and if it's documented anywhere, it will be in that. I don't think it will make it for the next two weeks.

BULLEN: Other questions from Board Staff? If not, thank you very much, Jerry. And we will move on to our final presentation of the afternoon, and that is going to address unsaturated zone transport, colloids, saturated zone flow and transport, and the biosphere. The presentation will be made by David Sevougian, who is the Total Systems Department Manager of the M&L.

SEVOUGIAN: Okay, I think today, the DOE wanted to give everybody a live demonstration of the drift scale heater test. I know I'm the last talk, and so everybody is anxious to go home, so I promise I won't take any longer than Bob Andrews talked this morning.

Here's the little icons. It's obligatory to point out which part I'm talking about. It's the natural barriers beneath the repository once the radionuclides have released from the packages. So unsaturated zone transport, saturated zone transport and biosphere, and also I'll talk a little bit about colloids.

I like Bob's conceptual model. Just to refresh your memory on unsaturated zone transport, the primary processes are transport through the various hydrogeologic types of rocks, vitric, zeolitized, and the processes are sorption, sorption into the matrix, sorption onto colloids, and then there's matrix diffusion. I don't see matrix diffusion on here.

Some specifics about the model that we used, the actual model we used in the VA, TSPA-VA, it's 3-D, based on a flow model. It's dual permeability transport model. I used the particle tracker from the FEHM code, and it's based on the flow fields, the material properties that we came up with using TOUGH2. It includes colloidal transport in both the UZ and the SZ for plutonium only. I'll talk later about how the UZ flow and transport affects SZ flow and transport.

And I don't know if anybody has talked about the nine key radionuclides. There's three conservative tracers that can diffuse a little bit into the matrix. So they're not quite as conservative you might say as a colloid. Then there's three that represent intermediate release and transport. These three here. This one is kind of off by itself, protactinium, because it has a high KD. And then there's the two isotopes of plutonium.

This is about the tenth time you guys have seen this one, but I just wanted to put it up one more time because this represents the top of the UZ transport and this represents the bottom of the UZ transport. There's no specific connection between the CC and the one and two. There's just a 3-D model. Particles can go wherever they want in between the top and the bottom. This is an overlay of how this rectangle overlays onto this repository outline.

We talked about diversion a lot. You guys have seen a number of slides on that. This is kind of another look at it.

By the way, I forgot to acknowledge all the people that worked on all these areas. There's a lot of them. This one was by Bruce Robinson. So I'm just reporting what other people did.

These are the six regions. The different colored particles are--they're colored differently depending on what region they're released out of. And here's where they go in the 3-D model when they end up at the water table. This is for present day climate actually for no matrix diffusion, so this would be like colloids. And I guess to me the most interesting feature is this area up here where the particles are going laterally over the perched water, and then they go down to the water table.

NELSON: Now, let me just ask. Those two are exactly like these two. Left is the repository; the right is the water table?

SEVOUGIAN: Yes.

NELSON: Now, that feature that runs pretty much north-south is the Ghost Dance, is it?

SEVOUGIAN: No, we haven't been able to associate this with any particular fault actually. It just appears to be more associated with the perched water. That's where the, I guess, the impermeable layer ends underneath the perched water, so it just kind of rolls off, goes down to the water table.

Here's another look at the same thing, but this is the travel time. That was just kind of a pathway. This is the time it took. The other one was present day. This is long-term average. There's two plots; one is the first arrivals here, the 21 per cent travel time. This is the 50th percentile of the break-through curve. And what you see is very fast--the purple is like almost, you know, a few tens of years travel time to the water table. So basically, there's no vitric over in this region. The water just runs very rapidly across the perched water and down to the water table.

This one I think Bob and several people already showed this one, so I won't spend much time on it. It's a pulse release of technetium at the repository, everywhere, uniformly across the repository, and looking at the break-through curve, the total break-through curve over the entire footprint of the repository at the water table for three different climates, present, long-term average and super pluvial. And as has been mentioned before, this is the fracture part of the break-through curve, of the travel of the radionuclides, which is much more significant when you put more water through the system.

That was technetium. Neptunium is about the same, but it's delayed in time. It's farther out in time due to sorption in some of the matrix, rock matrix of some of the layers.

Sensitivity analyses. The NRC is very interested in matrix diffusion. I know they don't think we should take any credit for it, so this was a study on the effect of matrix diffusion in the unsaturated zone. This is technetium in a long-term average--sorry--comparison of the two climate states, long-term average and present day, and these two curves are the present day comparison of no matrix diffusion with matrix diffusion. The same thing for the long-term average, with matrix diffusion and without matrix diffusion.

So for technetium, it doesn't seem to matter very much. For neptunium, it has a little bit greater effect for the two different climates. And, again, these two over on the right are the present day dry climate, and these two are the long-term average. And so it appears to make, for example in the dry climate, you know, almost a few thousand years difference, and in the long-term average in the few hundreds. And since most of the time is long-term average, you would expect, you know, a few hundreds of years is not going to really do much.

And, in fact, that's what happens when you carry this no matrix diffusion and sensitivity out to a dose calculation. For 100,000 years, it really doesn't, with or without, it doesn't really matter much. So I don't think it's much of an issue now.

Moving to the second topic, colloids, and here I should acknowledge Chris Stockman and Ines Triay and a whole host of other people, a lot of different people, you gather a lot of different people on the project together and try to do the best we can in modelling colloids for TSPA-VA.

As I said, it's in both the UZ and the SZ. We only did the two isotopes of plutonium for now as being probable ones that would have the greatest effect from colloidal transport, and it is based on laboratory data, scientific literature studies for stability of colloids, and some observations of the test site from underground nuclear events. So let me put up a couple slides on the test shots at the NTS.

This Benham shot--Yucca Mountain is down here. This shot up here on the test site, this is a blow-up here, so we're looking at the Benham site, and we're looking--they measured plutonium in ER20-5 here, which is about 1.3 kilometers from the test site, from the shot. And what they found was a maximum of .63 pCi/L per liter. The isotope ratio indicated it was from Benham. As I said, it's 1.3 kilometers. This is the depth it was fired at. So it took 28 years minimum to get to this well. When they measured, it was as a colloidal fraction mainly consisting of clays, zeolites and silicas.

So let me explain in a minute how we use that data.

We have two parts of our colloid modelling. The reversal part is in the EBS, it's in the UZ, it's in the SZ, it's all parts of the model. This is what we started out with, a reversible colloid model. It's just like a Kv model. So instantaneous equilibrium is assumed. There's a partitioning coefficient, Kc, that represents the ratio of plutonium on colloidal particles versus plutonium on insolutes, and this was the range, after much discussion, we are using for the EBS.

The lower end of the range was based on literature, surveys, and the upper end I think is more based on some experiments at Argonne where a lot of the fuel is coming off as colloidal particles. However, we found that using this model, although we did get plutonium release, we didn't get it quickly enough to explain the Benham test. And, you know, the probable reason is--or a good guess I think is irreversible sorption of radionuclides on the colloidal particles.

So we recently added a model for irreversible sorption in the geosphere part, the UZ and the SZ. We're conservatively assuming no filtration. We're assuming they go through fractures only, and the way we've done that is with a very low porosity, effective porosity, because the saturated zone model is a single continuum, effective continuum model, and then we had to estimate the ratio of these irreversibly sorbed colloids, which we're calling them fast to the reversible colloids.

And the way we did that, well, we assumed a range, 10 to the minus 10, to 10 to the minus 4, and the median value, 10 to the minus 7, that was based on the ration of the .63 pCi/L per liter measured at that well at Benham to the solubility of plutonium in J-13 water.

I should probably say that we ended up coming out pretty close, maybe you could call it fortuitous, but when these things finally came out of the model, they were coming out at .01 pCi/L, so a little bit lower than what they say at Benham.

Okay, sensitivity analyses on colloids. I have one little study here done by Bruce Robinson on the reversible model, and the range we used only went up to about nine, so these 9, 99 and 999 are real extremes that we didn't think applied, and when you go only to nine, the green curve, you can see you really can't get any colloids coming out for thousands of years.

Moving right along to the third part, saturated zone. I'll show Bob's picture first to remind you of the different processes involved. Here is about 20 kilometers from the repository to a well--well, actually a well in here. First it goes through volcanic units which have a very low effective porosity, short travel time. Within those units, advection and dispersion--advection and matrix diffusion is important, and then also sorption onto the matrix once radionuclides get into the matrix.

At some point in the travel path, alluvium shows up and has a much larger porosity, slower travel times, and the other thing that there's been quite a lot of discussion about is how much vertical transverse dispersion we ought to have in our model. So that caused us to change our model from the talk we gave at NRC, to now. We made some revisions based on the expert, what the expert panel decided, recommended.

The previous model really couldn't model low transverse dispersivities because the gridlocks were too big. So we ran out of computers. We ran out of computer time. So it affects the biosphere. I can't remember how much Bob talked about all this, but I'll go over it briefly. There is still 3-D models involved to determine travel paths. I'll talk about that in a minute. There's a 1-D model to determine transport times. There's a convolution integral method to involve the source term from coming out of the UZ with the SZ, and then there's a dilution factor to account for whatever dispersion there might be, transverse dispersion.

Here's kind of a picture of the regional saturated zone ground water flow in the area. Here's Yucca Mountain up here. It kind of goes southeasterly for some 5 kilometers or so, according to the model, and then it heads back more southerly towards the 20 kilometer boundary, which is right around here. Well, probably right around here at the NTS boundary.

Some of the ultimate release points are at Franklin Lake Playa, and maybe Death Valley, Ash Meadows. Now, the current 3-D flow modelling component of the flow and transport of the current saturated zone model is used to define the general direction of the radionuclide plume and the proportion of the flow pathways that are in different units, like volcanic tuff versus alluvium. And then we define those flowpath lanes for the 1-D transport modelling.

For the 3-D stuff, we assume steady state flow and specified pressure boundary conditions. And the new 3-D model, which is different, as I said, from about a month ago, uses an updated geologic framework model, more realistic framework model. The old model has some very strange looking units in the flowpath that didn't really look like your typical geologic glaring. This uses more realistic glaring, and this is found to be the general direction of the plume.

Then finding that and the proportion in the alluvium, we look at a 1-D streamtube model, and we've got our infamous six regions here, which are now tied directly to six streamtubes, and you can see the flux QUZ coming out of the UZ. That flux is--that volumetric flux is the same as the volumetric flux in the saturated zone. We just assume the water comes down and just flows along the top of the water table. But it flows at a rate given by the saturated zoned modelling of .6 meters per year, and again, as I said, the fraction of the flowpath length is varied--the fraction of the alluvium or tuffs is varies.

This thickness here, the width is probably like a couple kilometers, like take Region 5 here, the thickness maybe is only 10 meters, and dispersion is included by a dilution factor. Here are some unit break-through curves based on a unit concentration at the upstream boundary for the various radionuclides expected value case. These guys out here, the early ones that are plutonium colloids, the irreversible ones, actually come through faster than technetium and iodine, and then you have neptunium, and then the plutonium 242.

That was expected value case. We did 100 realizations of what we felt were the most uncertain parameters in the saturated zone, the most important ones that would affect the results. Here's a listing of the key saturated zone parameters. Of course the dilution factor is the most important. Then there's four effective porosities in the various geologic units. And you see it goes down to quite low value. Actually, for plutonium, it was sampled a little differently. We conservatively sampled more in the low end of the range for plutonium colloids. Then there's the Kd's for sorption for the various elements. And there's this Kc value for plutonium in the unsaturated zone and the saturated zone, it only goes to one. It goes to ten in the EBS, but only to one. We felt one was more realistic in the geosphere. There's a longitudinal dispersion. This is the log of it. And then there's the fraction of flowpath length in the alluvium is sampled uniformly from zero to .3 of the total path length, so .3 times 20 kilometers is 6 kilometers. But 10 per cent of the time we assume that there's no flow through alluvium. That's kind of a conservative assumption.

Here are 100 realizations of just the 1-D transport model. I think this is streamtube 1 maybe. I'm not sure. These high ones are for very low dilution, but the mean dilution factor is ten, so a tenth of this gets you down to .3 in here. So a lot of your realizations will be down in some pretty low concentrations--lower concentrations.

BULLEN: Dave, this is Bullen, Board. Is this break-through curve after it's reached the saturated zone? So this is the travel time from the saturated zone out?

SEVOUGIAN: Yeah.

BULLEN: So it's up to 20,000 years, or actually shown up in a couple thousand years?

SEVOUGIAN: Yeah. I mean it comes through. They all start out pretty--hundreds--pretty fast. There's not much sorption for technetium, and porosity is not all that high. Probably some of these guys down here are more in the alluvium, so they're taking longer. These other realizations would be when you don't sample much alluvium in the flow path.

This next slide is on the convolution method that we used for the TSPA. This line represents the part of the model that's within RIP, and this is outside of RIP. So outside--RIP is the total system model. So outside of the total system model, we first used the step function, as I mentioned earlier, ran it through the 1-D transport model, developed these unit break-through curves. They're sitting out there in some files, some table files, like a library of tables, and they're read in at simulation time. So the total system model runs the 3-D transport model dynamically, comes up with a source of the mass coming out of the UZ that feeds the SZ model, and then these two things, the source term and the unit break-through curves are integrated together, and then you get the final nuclide concentration coming out of the SZ for each streamtube and each radionuclide.

This one I don't want to spend too much time on it because I'll probably get stuck or something on it, but this is the CDF, the blue one is the CDF for dilution factor that we used. It's based on three experts. The dissenting opinion was Gelhar who thought it was hardly any dilution at all. So they gave us dilution factors and we computed some corresponding vertical transverse dispersivities just kind of as a check using a 3-D analytic solution. What you see here is two different dispersivities. There's a half meter vertical transverse, and then 5 millimeters. And you can see 5 millimeters is more in line with what Gelhar was saying. You can see the difference it makes in the plume depth when you get out to 20 kilometers.

And the one sensitivity analysis that we have to show you for saturated zone is for the key factor, which is the dilution factor. This is out to a million years. We ran the 5th and 95th percentile of the dilution factor, and you can see it looks pretty linear. So you've got two orders of magnitude almost when you go from--well, the range went from 1 to 100, so this is like 5 and 95. So it has a pretty important effect.

And I think Mike showed that earlier in his analysis, his million year regression analyses showed the saturated zone dilution factor to be probably I think the third highest most sensitive factor.

Well, I didn't have the exact picture that Jeff wanted, so I decided to throw this one back up there to show that there were a lot of pathways involved in the computation of the biosphere does conversion factors. I can't really address it in detail, and it wasn't our intention to address it in detail at this meeting.

So going pretty briefly over the biosphere, but here are the major assumptions. It was a farmer, average farmer living 20 kilometers from Yucca Mountain. We assumed his present day behavior would persist into the future. All the water for household and agricultural uses comes from a well located at the center of the plume, maximum concentration. Local food stuffs are consumed in the amounts determined for an average person by a site survey that was conducted by the project. And the other parameters are taken, the regular dose conversion factors are taken from accepted national and international sources, used the GENII-S model, looked at 39 radionuclides, three climates and three receptors. The other receptors were--I think there was a subsistence farmer and a residence farmer.

Here is an example of the prediction from the GENII-S model for technetium. So it goes over, it doesn't have a real wide range, a factor of 10 or so on this particular distribution.

WHIPPLE: Can I interrupt for a quick question, David?

SEVOUGIAN: Yeah.

WHIPPLE: I assume that the uncertainties in the GENII-S inputs involve consumption rates of food and water. Were there other distributed variables that were--that result in this distribution for technetium?

SEVOUGIAN: Yeah, I can't remember them all. The most important ones had something to do with how much the plants take up, I think. Jack might be able to answer it. But there a number of them.

GAUTHIER: Yes, this is Jack Gauthier. The most important was something called the drop interception fraction. That's the amount of--well, that's when you irrigate from above, and the amount of water that's uptake through the leads. The second most was the normal plant uptake factor I believe, which is through the roots. I can't remember the rest. Sorry. But I can supply those later.

BULLEN: Bullen, Board. Jack, just a followup question on your GENII-S calculations. Did you do a deterministic calculation, or did you do stochastic sampling of a range of variables, such as the ones that Chris asked about?

GAUTHIER: Yeah, we did the stochastic calculation.

BULLEN: Okay, thank you.

SEVOUGIAN: And then I have the one sensitivity analysis on biosphere. This is taking the 5th and 95th percentile of the biosphere dose conversion factors, and you can see it spans about an order of magnitude range. My recollection is it turned out to be one of the more important parameters in the sensitivity analyses. I'm not sure we know exactly why. I mean, we didn't really expect it.

And, finally, I don't have a summary slide, so I guess I get out of the rest of the question. Right?

BULLEN: Thank you, Dave. Questions from the Board? Jeff Wong?

WONG: These are easy. Just two concerns. Are you worried about your farmer there? Turn around and look at your farmer. He's wearing Tyveck protective clothing.

The other thing is I've looked at your various dose histories through the talks, and after about 200,000 years, it looks like the dose is, for about 100,000 years, is up above 100 millirems, and actually close to 200 millirems per year. Are you concerned with that?

SEVOUGIAN: Yes. You're asking my personal opinion?

WONG: Yes.

SEVOUGIAN: Yeah, I'd probably put a few design options into the--a few enhancements, design enhancements. But it depends, you know, this is a public thing, you know, how long do people want to worry about this. A million years? I don't know. 5 million years?

WONG: Abe, are you concerned with this?

VAN LUIK: This is Abe Van Luik, DOE. I'm concerned if these are the doses that we show on the licensing case. The now, the purposes of these is to show what's important, what we don't know, and what we still need to nail down between now and licensing.

WONG: Thanks.

BULLEN: Other questions from the Board? Parizek?

PARIZEK: Yeah, Parizek, Board. Figure 6 of Andrews' talk this morning, which I couldn't hear, I was trying to get here, I think is a great step forward in trying to take complicated material and render it interesting and comprehendible. I don't think the head dude who's drinking water though at the bottom of the biosphere representation is as good as maybe the figure you just showed us, which is the figure that had all of the cows and chickens and crops, which is a little bit more honest, or a more accurate representation of really how complicated the biosphere is. So something from that diagram ought to be down with the dude besides just drinking water, which--the upper part of that diagram shows all the little things that go on that feed into this. So it's not just drinking water.

You made comments about a new model for a geological framework model. Is that printed up in some place so we can see what went into it, or how it differs from the input from previous models? Because if you're going to use a new one at this point, we have to kind of chew at it and see what it's based on.

SEVOUGIAN: I'll let Bill Arnold answer that one.

ARNOLD: This is Bill Arnold, Sandia Labs.

This revised geologic framework model was provided to us in a pre-release form by Claudia Faunt from the USGS, and those part of the geologic framework model that we used in the saturated zone analysis will be documented in the technical basis report. But I don't believe that there is another document from the USGS that shows that model at this point.

BULLEN: Any more questions, Richard?

PARIZEK: Well, yeah. Obviously the steady state approach is probably useful and necessary at this time in view of the complications with the a complicated model, but on the other hand, we go through this climate change, it's a very major differences in the recharge amounts and the distribution that in fact could make the ground water flow system behave in a different manner we're mixing, could in fact be greater than what the one dimensional calculations suggest. So not to reopen the old problem os having maybe too wide a plume and too deep a plume, the point is maybe now it's still a little narrow and maybe not as realistic as it might be if there is some climate change built into a transient model, spreading that plume around, because that's what would spread a plume. So that's a future concern, but I just offer it at this point because I think youi have to do something with this problem, but I'm not sure you're at the end point yet with it.

SEVOUGIAN: Well, the previous saturated zone model did do that.

PARIZEK: It was very broad for reasons that a lot of people were uncomfortable, and the approach you've taken now I guess mirrors the NRC analysis in some detail. But, again, this is against steady state, it's not allowing for some transients in recharge, which again could be the mechanism that causes more spreading again, so until you have a transient model, you won't know how much spreading you'll get or how much you'll get of dispersion.

ARNOLD: I just have a brief response to that. When we asked the expert panel for their estimate of a dilution factor, we asked them to take into account all processes that could lead to dilution, and they recognized that transient in the flow system would probably be one of the most important processes leading to apparent transverse dispersion. And so the effects of transients are more or less implicitly wrapped up in their estimate of dilution factor.

PARIZEK: But it's actually that run for this case. The transient model was not run for this case. You really don't know how--

ANDREWS: That's correct. I mean, that hasn't been-- that range of dilution factors hasn't been checked, or hasn't been compared with transient models.

SEVOUGIAN: One comment that may be of interest on that is we did do a comparison in the unsaturated zone of a transient model with a series of steady state. And on the time scales as we were looking at, the system will have rated quickly enough so that the series of steady states really look about the same as the transient model. I don't know whether that's true in the saturated zone.

BULLEN: Paul Craig, Board?

CRAIG: Yeah, I think I'm pursuing the same line of reasoning. Your break-through occurs and suggests that almost everything has come through after 1000 years, give or take, which--and even if it's 5,000 years, it's still very small in comparison with a million years. At a million years, you've got your water table wandering up and down by 100 to 200 meters. Isn't that going to change the pathways very significantly? It seems like quite a significant change in the geology, even under almost practically any circumstances, and that's not a transient question. That's a change in the steady state condition as a result of a rise in the water table. Is any of that taken into account?

SEVOUGIAN: I may have to call on Bill again. I mean, I think there is evidence from water table rise.

CRAIG: Well, you're essentially assuming that these tubes are going to remain fixed in location as the water table changes over 100 meters, up and down.

SEVOUGIAN; I think the evidence is just the flux increases.

ARNOLD: Well, what you say is correct. We are assuming that the streamtubes do not move around in space with climate change. We changed the ground water flux through the tubes in response to climate change. There has been some regional scale modelling with the USGS regional scale saturated zone model for climate change scenarios, and in fact that's where we got those changes in fluxes through the streamtubes, was from the results of the USGS regional scale model.

Based on some preliminary results from the regional scale model, the flow paths from Yucca Mountain do not appear to change much in direction in response to climate change.

BULLEN: Other questions from the Board? Questions from the Panel? Chris?

WHIPPLE: David, my understanding is that the general flow pathways in the saturated zone are not particularly well characterized, in addition to the uncertainties in dilution factors we just heard about. And yet I get a sense that the bulk of the work has been modelling and not data gathering. A couple questions there.

Have you all done a systematic review for analogs perhaps of mine drainage or such things in Nevada that could give you some field data that would give you a heightened sense of comfort that you understand something about how saturated zone flow in this system will work?

And then the second question is have you looked at the margin of whether you're better off with more modelling or more field data?

SEVOUGIAN: Let me answer the second one first. I think we do need more data. I don't know what DOE's position is, but I know they're planning, I think they are, at least there's some stuff in the works to expand the C-wells complex, maybe to drill some other wells, and Nye County I think is drilling some wells. I actually have a list here somewhere of what we plan to do for the saturated zone as far as data gathering. Maybe I could--give me a second.

Yeah, Nye county is going to drill a series of shallow and deep bore holes south of the site. Inyo County, there's some sampling going on of regional springs for hydrochemical analyses, evapotranspiration. For colloids, there's a test site, and there's also the Park Service is doing some work on evapotranspiration of Death Valley. The GS is doing the same thing at Oasis Valley. I mean, I think we need more data on the--let's see, what was the first question again?

WHIPPLE: The first one was have you done a search for an analog?

SEVOUGIAN: Yeah, I think maybe Bob can answer. I haven't done--obviously, we had the saturated zone expert elicitation. But I don't know if we looked at analogs or not.

BULLEN: Chris, any other questions?

WHIPPLE: No.

BULLEN: Oh, yeah, here we go. Here's the expert.

HOXIE: My name is Dwight Hoxie. I'm with the USGS, but I actually manage the SZ transport modelling program on behalf of the M&O, so maybe I could offer a few things or give you some feeling of where we are headed between now and the license application.

We have developed a site scale transport model that does cover the entire area of concern, and goes all the way from north of Yucca Mountain down to it's actually 45 kilometers long, so it goes down to about the 25 to 30 kilometer boundary. The difficulty is, and David alluded to it, is that at the time that we developed this model, we did not have a good hydrogeologic framework, so the hydrogeologic framework was sampled on a grid that was 1,500 meters on a side.

You heard about this when you were at Armagosa Valley in January. So the model had difficulties. Also, because of the large grid scale, the grid size, it was really not suitable for advective dispersive transport. We just got too much numerical dispersion.

So the plan right now is to stick with the same model domain, except David also mentioned this, we're redoing the hydrogeologic framework. It is being sampled now on a 125 meter grid size rather than 1,500 meters, and we will probably be doing simulations at a grid size, a computational grid size, on the order of 200 to 250 meters or so. So we ought to be able to do a little bit better at addressing the numerical dispersion problems.

Also, there are plans afoot to instead of doing classical advective dispersive transport, is to use other methods that maybe Bill Arnold could address those, but to essentially refine and enable us to handle very small dispersivities, which we can't do at the present time.

And in addition to this, there are also plans afoot to collect additional data. We are in the process of planning a second tracer complex, you heard about this also in January, that will be located someplace in the down gradient region from Yucca Mountain that would give us additional hydraulic and perhaps transport parameter data.

So I think that we have plans afoot to address these things. Now, how much we can get done between now and a license application time in year 2002 remains to be seen. But I think that at least we can develop a model that we can defend technically.

BULLEN: Thank you, Dwight. Chris, did you have any more questions?

WHIPPLE: No.

BULLEN: Jean?

BAHR: Yeah, I was wondering maybe you could put up

Figure 21, which is the one that shows the streamtube model in the saturated zone. I'm trying to understand what happens at the end of that model. You take the concentrations or flex weighted concentration from each of those streamtubes and average them, or are your doses based on dilution in one particular--one of those streamtubes that has the highest concentration? What's done at the end of those streamtubes? And maybe this is a question for Bill.

SEVOUGIAN: No, I was actually hoping you wouldn't ask that one. That's the toughest question of all.

We kind of do an either/or test. For low dilution factors where the stuff doesn't spread out very much, it will be--we figure the concentration will be confined pretty much, the radionuclides will be confined within their own individual streamtubes. In that case, what we use is the maximum concentration from any one streamtube, from the maximum streamtube.

On the other hand, for higher dilution factors, they tend to spread out amongst the streamtubes, the radionuclides, and so we take the sum of the streamtubes.

BAHR: And when you sum that, is that a flux weighted sum or is that a resident concentration sum?

SEVOUGIAN: I can't remember.

ARNOLD: This is Bill Arnold, Sandia. We take a resident concentration. So conceptually what's happening is with a lot of dilution, each one of these stream tubes spreads out into a fairly broad plume. All of the plumes are more or less overlapping, and as a conservative approximation, we sum the concentrations.

BAHR: Is there a big different in flux rates or specific discharge within the streamtubes by the time you get down to the 20 kilometer boundary?

ARNOLD: The volumetric flux?

BAHR: Yeah, or the specific discharge.

ARNOLD: Well, the specific discharge is the same.

BAHR: Is the same because you're in the same unit basically?

ARNOLD: But the volumetric fluxes among the different streamtubes varies by a factor of--

BAHR: Because your streamtubes have different volumes?

ARNOLD: Right. It varies by a factor of about four, three or four.

BAHR: Okay. I guess in the low dilution case, I'm a little bit concerned about the discretization that you've used in the source area, because maybe, David, if you could go back to Figure Number 4, and maybe we need to look at Figure Number 3 and Figure Number 4 together.

SEVOUGIAN: Which one do you want first? Well, I'll put them both up.

BAHR: Yeah. I realize that your saturated zone source areas were derived from a previous model that didn't use this streamtube analogy and maybe you were doing things differently, but if you were to draw those areas on there, for example, in Area 6, it's really a very small portion of Area 6 that has a lot of particles in it, or maybe not a lot of particles, but a few particles, the black ones, you can see concentrations of your particles that have moved from your unsaturated zone, and those don't really correspond in a logical way to those source zones.

SEVOUGIAN: Well, the thing is if you remember one of Bob's slides on the EBS--I mean, the releases at the bottom of UZ, remember, Region 5 was the highest, and there was a grouping of other ones below that. They're pretty much the same releases from each of the regions.

BAHR: Yeah. But the thing is, for example, what you're doing in Region 6 there, you've got those particles, but you presumably got water that's come into Region 6 at your saturated zone boundary that's coming from elsewhere since it's not coming from the repository zone, and your starting concentration in your flow tube Number 6 is going to already be diluted by that extra water. And Flow tube 6 is three times as thick a flow tubes 3, 4 and 5. So what you're going to see at the outlet of that in terms of a concentration may be somewhat artificially diluted, and if you're looking at the low dilution case and you're looking at the maximum concentration and the maximum streamtube, it does make a difference how you start your streamtubes off.

SEVOUGIAN: I don't think there's any extra water. I mean, I don't know where you got that. But, you know, you have a good point. I mean, clearly you'd want to discretize it as finely as you could to capture the peaks, and we did what we could.

BAHR: But have you done that now in light of this streamtube sort of model and what you're doing with it?

SEVOUGIAN: Well, I mean, these--I mean, all the radionuclides come out here. I don't quite--I guess I don't follow what you're saying.

BAHR: Well, it's all coming out at the very edge of Region 6. So where's the rest of the water in Region 6 coming from, or is there no other water in Region 6?

ARNOLD: Well, there is no other water in Region 6, and as a matter of fact, Region 6 has the lowest volumetric flux of ground water flowing through it, and it also apparently has the smallest number of particles exiting the system.

You're right to the extent that we haven't really tuned this--

BAHR: Yeah. I mean, I'm looking at the boundary between 5 and 6, there's a zone where you've got, you know, the pink and the black that are sort of overlapping there. You've got a zone where it looks like you've got a high concentration of particles that doesn't correspond to any one of your particular zones. And if you divide--if you chop that pie up in different ways, you may end up with different concentrations and the end of your streamtubes.

ARNOLD: Well, except we divide the pie up in the same way for transport as we do for volumetric ground water flux into that streamtube. We take the volumetric ground water flux into the streamtube from the UZ flow model, and apply it to that streamtube in the same way that we take the radionuclide mass flux and apply it to the--

BAHR: Yeah, but let's say that I had--

SEVOUGIAN: I think you're right. Okay? We could have put a stream tube around these pink areas and it might have been a lot higher concentration.

BAHR: Right. For a low dilution case, I think it may be sensitive to that.

SEVOUGIAN: We'd have to look at that in the future.

BULLEN: Any more questions, Jean?

BAHR: No.

BULLEN: Steve Frishman?

FRISHMAN: I had the same two diagrams out and was looking at it from sort of a different way, your four here and Number 45 from Bill, and they look inconsistent to me. It looks to me as if on the one that's up here, it looks to me as if the one that's on Page 4 that's up on the screen is saying that you get essentially nothing coming through Region 6 to the water table, except when you get all the way over to the east. Is that what it's saying?

SEVOUGIAN: All of this--what we found is that the northwest, that stuff comes out in Regions 5 and 6. I don't have the plots with me. We released some pulses in the various regions, like the northwest, northeast, then we looked at, you know, where it was coming out into these regions. We found that the northwest actually is laterally diverted, and most of it comes out under 5 and 6. Is that what you're asking?

FRISHMAN: Okay. Well, I guess what I'm looking at is comparing that to the Page 45 thing where Region 5 showed highest, and you said that was partly because of lateral transfer, but if you look at Region 6, Region 6 is pretty much the same thing as Region 3, and they're all very close together. Region 5 is the only one that's elevated. So it's saying here that you have fairly similar transport downward to the water table in all of those regions, including Region 6, and then on this what you're saying is you get essentially nothing until you get to the eastern boundary. And it looks as if, you know, you're talking doses or you're talking concentration, so it looks as if you're saying that there is a Region 6, that none of the water in Region 6 gets to the water table except on the very eastern boundary. But then your other analysis doesn't seem to go with that, because it seems that you would have region--you know, showing the technetium dose rates, it seems that you would be running higher dose rates if that's what was happening.

HO: I thought I understood, but I think I got it confused. But I just want to clarify, and I think this is a point of confusion, that when you refer to Region 6, Steve, that it is sort of confusing on that diagram on the right. It is not just that northern part of the repository outline. Region 6 pertains to the saturated zone. The regions, the sub-regions for the repository are designated as northeast, northwest, central, et cetera. So when you see 1, 2, 3, 4, 5, 6, unfortunately they happen to all fall in the repository outline, but those saturated zone regions extend beyond the repository outline.

SEVOUGIAN: Not by much, Cliff. Not any more.

HO: But I just want to point out that that repository outline does not limit the saturated zone capture zones defined by 1, 2, 3, 4, 5 and 6.

SEVOUGIAN: Well, they extend a little ways beyond the outline, maybe 10 per cent or so. It's interesting, we haven't fully analyzed it. Okay?

BULLEN: Steve, do you have any more questions?

FRISHMAN: No. It just looks inconsistent to me, and I'll try to figure it out.

BULLEN: Okay. So we're not as transparent as we might want to be. Any questions from the Board Staff? Okay, Leon?

REITER: Dave, are you assuming any sort of other dilution at the well head aside from just mixing in?

SEVOUGIAN: No.

REITER: Okay. So it's the same idea that the well is just withdrawing from the most concentrated part?

SEVOUGIAN: The most concentrated part of the plume; right.

BULLEN: Any further questions from the Board Staff? If not, Dave, thank you very much. And as a true university professor wanting to make sure everybody gets their money's worth, we have 15 minutes left, and I've been ignoring the audience explicitly almost all afternoon. So this is your chance. If anyone from the audience has questions for any of today's speakers, feel free to come to the microphone and ask your question. Rod, please, identify yourself and ask the question.

EWING: Rod Ewing, and this is for Terry McNeish. For cladding failure models, did you consider unzipping of the cladding due to volume changes associated with the alteration of the fuel to these secondary products?

MC NEISH: That's only incorporated in the juvenile failures, so the very early failure. It's not explicitly modelled as an expansion of the fuel area.

EWING: But the juvenile failures come early when there's no alteration; right?

MC NEISH: Right. But those occur early and they have a pin hole which will then lead to oxidation which will cause the expansion of the rod--or expansion on the fuel, and then unzipping of the rod.

EWING: Right. But later on that would happen as well?

MC NEISH: It's not included. It's later on.

Yeah, it's also, Mike was saying that it's not hot enough to cause that expansion.

EWING: I'm not talking about the thermal expansion, but if you take UO-2 and alter it to these uranyl oxyhydroxides that are hydrated, there's a tremendous volume change. That's part of the normal corrosion process, so all it takes is water, which could pass through a pin hole, and I'm wondering if that could unzip the cladding.

MC NEISH: Yeah, we have information that it says it's not strong enough to actually unzip the cladding. It will squeeze out the hole, but it won't actually--

EWING: Are those experiments or expert elicitation?

MC NEISH: I'm not sure. That's from Eric Seedman, the guy that's developed the cladding model.

BULLEN: Thank you, Jerry. Thank you, Rod. Another question? Please identify yourself.

ERIKSSON: Leif Eriksson, COMPA Industries. I'm struggling with the model for the flow and transition of radionuclides. I would appreciate it if you could clarify if the model I have in mind is the right one.

As I understand it, you have reflux of radionuclides down to the UZ zone--from the UZ zone down to the saturated zone, and that varies from there, and they all climb on the same bus that travels .6 meters per year down to the end of the tube, and then travel 20 kilometers. Is that sort of a reasonable understanding of the conceptual model?

BULLEN: Dave, is that correct? We basically get them to the top of the saturated zone, and then they go downstream tubes at .6 meters a year?

SEVOUGIAN: That's right.

ERIKSSON: 20 kilometers?

SEVOUGIAN: 20 kilometers, yes, with some dilution.

ERIKSSON: Have you looked at what the concentration in those horse tail curves would be after 10 kilometers?

SEVOUGIAN: We might have looked at 5. I don't know. You'd have to ask Bill. Why would we look at 10?

ERIKSSON: Well, I mean, I'm confused about 20 kilometers to begin with, because I can't find it in the regulatory documents.

SEVOUGIAN: Well, it's in the DOE interim, what do you call it--

BULLEN: Site standard, isn't it?

ERIKSSON: Interim site standard.

BULLEN: It's the site standard that DOE has decided is going to be their interim target, and they picked a 20 kilometers distance for that. I mean, that's why the 20 kilometers.

ERIKSSON: Okay. What is the condition today, or was the condition prior to the law remanded or took away 40 CFR 191, was that travel distance in excess of--is 10 kilometers, and I'm surprised that you don't use that for your viability assessment.

SEVOUGIAN: I thought it was 5 kilometers.

BULLEN: Well, the NRC's was 5, wasn't it?

ERIKSSON: Well, NRC hasn't changed their regulation yet. EPA 40 CFR 191 changed the maximum travel distance from 10 to 5. 10 CFR 60 still reads 10 kilometers.

BULLEN: David Shoesmith, did you want to comment? Go ahead and line up at the microphone. It doesn't really matter.

MC CONNELL: My name is Keith McConnell. I'm with the NRC. And the 5 kilometer boundary was the--I guess the distance that was used for release rate standard. I think now that we're at a dose or risk rate standard, we're in our own sensitivity studies, using 20 kilometers also.

BULLEN: Okay, thank you very much for clarifying that.

SHOESMITH: I just wanted to address a point raised by

Ron. I think that the reply is actually correct. The difference between the thermal oxidation of UO2 is there's no medium to move the product around. Therefore, it stays and swells at the site to which it is formed. If it's converting or altering in the presence of water, then there is a medium by which it can flow, and it will indeed squeeze along the tube.

However, I think it's very dangerous to say that there's a lot of supporting evidence to back up that argument. There isn't really a lot of evidence. It just makes sense that it will be transported in block.

BULLEN: Thank you, David. Linda Lehman?

LEHMAN: Linda Lehman, State of Nevada. Up until today, I was real excited that you guys were using higher flux rates. And I thought oh, gee, this is really good. We're going to see some real results. But after I heard Bob Andrews' talk and I think Jean Bahr questioned the numbers, too, it was a little bit confusing because the rates, the seepage rates into the tunnel now are not in millimeters per year like everything else. They were in cubic meters. So it was a little bit confusing to me. But when I went back and looked at these numbers, I did some quick calculations as to what would be the flux into the drift, the seepage rate into the drift, and for the regular case, the average is about .002 millimeters per year for the dry conditions. And for the maximum glacial, the super pluvial at 140 millimeters per year, the seepage rate into the drift is only .7 millimeters per year, which is back to almost what we were in the first TSPAs.

And then I just heard Bob say that that's not even the lower limit that gets onto the canisters. It's even a tenth of that. So I just wanted to clarify that even though we're supposedly looking at higher flux rates, I don't really believe that that's been translated down correctly maybe, or like Jean brought up, what about the focusing and perhaps we ought to be looking at maybe some higher numbers into the drift.

I have another question then on--I guess that was a comment rather than a question, unless I'm way off base. What I did was I took .5 per cent of the fluxes, which is what you said in your--so 99.5 per cent of all the flux that comes through the saturated zone is bypassing the tunnels, just to make that clear.

Okay, now I have another question here on--it was on Mike Wilson's discussion of the seepage into the drift, how that was actually calculated. And I'm a little unclear and maybe you can help me with this. You're using a continuum--a fracture continuum model, you said? And then you said something about you wouldn't get flow into the drift unless you had saturations that were--until you're fully saturated. So is this like an equivalent continuum where you have some porosity or something that you can build up? And does your saturation have to reach 100 per cent like we have been doing in the matrix before you get flow in?

WILSON: It's fully saturated fractures. So it's not--equivalent continuum implies that you're somehow lumping the matrix and fractures together. We're not doing that. We're just modelling the fractures, and the fracture has to be saturated locally for flow to enter the drift.

LEHMAN: Okay. So it's somehow being stopped from dripping by this capillary barrier.

WILSON: Right.

LEHMAN: And then when it reaches 100 per cent saturation locally, then it can drip in?

WILSON: That's right.

LEHMAN: Okay. Have you given any thought to relaxing that 100 per cent parameter like you have elsewhere? Everywhere else where you've dealt with matrix flowing into fractures, you've relaxed that parameter somewhat. Is there any thought to doing that?

WILSON: I don't think so. I can't remember. There's a number of things that we want to try related to the seepage model, but I don't really remember that as being one of them.

LEHMAN: Okay.

BULLEN: Thank you, Linda. Any other questions or comments from the audience?

(No response.)

BULLEN: If you think of something overnight, feel free to sign up with Linda Hiatt in the back of the room. We're going to begin tomorrow's session at 8:00 a.m. with public comment again. And I declare this panel meeting in recess.

(Whereupon, the meeting was concluded.)