UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

WINTER BOARD MEETING

February 1, 2006

Desert Research Institute
755 East Flamingo Road
Las Vegas, Nevada 89119

NWTRB BOARD MEMBERS PRESENT

Dr. Mark Abkowitz
Dr. William Howard Arnold
Dr. Thure Cerling
Dr. David Duquette
Dr. B. John Garrick, Chair, NWTRB
Dr. George M. Hornberger
Dr. Andrew Kadak
Dr. Ronald Latanision
Dr. Ali Mosleh
Dr. Henry Petroski

SENIOR PROFESSIONAL STAFF

Dr. Carlos A.W. Di Bella
Dr. Daniel Fehringer
Dr. Daniel Metlay
Dr. David Diodato
Dr. John Pye

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Dr. William Murphy
Dr. Leon Reiter

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What are the most significant physical and chemical controls on release of radionuclides from the waste package? What is the qualitative and quantitative level of understanding of those controls? What is being done at present to develop a better understanding of the most significant controls on the release of radionuclides from the waste package? What key processes and events affect the estimate of mass/activity flux from the waste package and engineered barrier system? What is the technical basis for the models used to evaluate those key processes and events? What are the key assumptions and uncertainties associated with these models?

Mass and Activity of Key Radionuclides Potentially Released from the Unsaturated and Saturated Zones over Time

For a potential unit release of radionuclide mass or activity from the drifts, what is the rate of release from the unsaturated and saturated zones, respectively? What are the key processes that affect these estimates? What is the technical basis for models used to estimate the mass or activity flux out of the unsaturated and saturated zones? What are the key assumptions and uncertainties associated with this estimate? Given a unit release from the drift tunnels or unsaturated zone, respectively, how do releases from the unsaturated and saturated zone vary temporally and spatially?

Management and Technical Support Peak Dose Sensitivity Analysis

What key assumptions were used in the peak dose sensitivity analysis? What are the most

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(Continued)

P R O C E E D I N G S

8:07 a.m.

GARRICK: Good morning and welcome.

My name is John Garrick. I'm Chairman of the Nuclear Waste Technical Review Board. And, on behalf of the Board, I'd like to say we're very pleased to be back in Las Vegas, a city of frequent visits by the Board, and the primary location of most of our public meetings.

As you know, we, at each of our meetings, introduce the Board members, and I want to do that first. And, in my case, I am in the role of a consultant these days, primarily on the application of the risk sciences to a variety of industries, space, chemical, nuclear, transportation. And, my background and areas of interest are risk assessment and nuclear science and engineering.

Now, as I introduce the Board members, I want them to raise their hands when their name is called. And, the first one I want to introduce is Mark Abkowitz. Mark is Professor of Civil Engineering and Management Technology at Vanderbilt University, and Director of the Vanderbilt Center for Environmental Management Services. He chairs the Board's Panel on the Waste Management System.

Howard Arnold. Howard is a consultant to the nuclear industry, having previously served in a number of senior management positions, including vice-president of the Westinghouse Hanford Company, and president of Louisiana Energy Services.

Thure Cerling. Thure is a Distinguished Professor of Geology and Geophysics and a Distinguished Professor of Biology at the University of Utah. He is a geochemist, with particular expertise in applying geochemistry to a wide range of geological, climatological and anthropological studies.

David Duquette. David is Department Head and Professor of Materials Engineering at Rensselaer Polytechnic Institute in Troy, New York. His areas of expertise include physical, chemical, and mechanical properties of metals and alloys, with special emphasis on environmental interactions. His current research interests include studies of cyclic deformation behavior as affected by environment and temperatures, basic corrosion studies, and stress-corrosion cracking.

George Hornberger. George is the Ernest H. Ern Professor of Environmental Sciences and Associate Dean for Sciences at the University of Virginia. His research interests include catchment hydrology, hydrochemistry, and transportation of colloids in geological media. He chairs the Board's Panel on the Natural System.

Andrew Kadak. Andy is Professor of the Practice in the Nuclear Engineering Department of the Massachusetts

Institute of Technology. His research interests include the development of advanced reactors, space nuclear power systems, improved technology-neutral licensing standards for advanced reactors, and operations and management issues associated with existing nuclear power plants.

Ron Latanision. Ron is an Emeritus Professor at MIT and a principal and Director of Mechanics and Materials with the engineering and scientific consulting firm, Exponent. His areas of expertise include materials processing and corrosion of metals and other materials in different aqueous environments. He chairs the Board's Panel on the Engineered System.

Ali Mosleh. Ali is the Nicole J. Kim Professor of Engineering and Director of the Center for Risk and Reliability at the University of Maryland. He has performed risk and safety assessments, reliability analyses, and decision analyses for the nuclear, chemical and aerospace industries. He chairs the Board's Panel on Repository System Performance and Integration.

Henry Petroski. Henry is the Aleksandar S. Vesic Professor of Civil Engineering and Professor of History at Duke University. His current research interests are in the areas of failure analysis and design theory. Ongoing projects include the use of case histories to understand the role of human error and failure in engineering design, as well as models for invention and evolution in the design process.

We also have two consultants with us today. Dr. William Murphy. Bill is an Associate Professor in the Department of Geological and Environmental Sciences at California State University-Chico. His areas of expertise are geology, hydrogeology, and geochemistry. Dr. Murphy has worked with the Board many times in the past, and we are pleased to have him with us today.

And, recently retired from more than 31 years of federal government service, and about half of that on the Board staff, we are pleased to have with us today Dr. Leon Reiter. Leon's areas of expertise include probabilistic seismic hazard assessment, probabilistic volcanic hazard assessment, and total system performance assessment. And, on behalf of the Board, Leon, I want to thank you for a career of distinguished service, and we look forward to having access to your expertise.

There is one other group I would like to introduce as a group that's with us today, distinguished group. The Board, some three years ago, started participating, at least having representatives participate with other similar boards around the globe that advise their governments on matters pertaining to nuclear waste, and in some cases other nuclear matters. And, we have now had three meetings. We had a meeting initially in Paris, and our second meeting was in Berlin, and our third meeting, which I have had the privilege of chairing the last two days here in Las Vegas, for the purpose of seeing how we, and each of our advisory boards, can maximize our effectiveness in advising our respective governments on matters, in this case, pertaining to nuclear waste.

And, we have representatives from Germany, from France, from Japan, from Sweden, from the Nuclear Energy Agency, and, of course, from the U.S. And, I'd like to ask these distinguished members of the International Advisory Board Group to stand at this time, and I encourage those of you to interact with them and question them and help us continue this process of an effective exchange with the international community, such that we can all adequately address this international issue.

Thank you very much.

Now, at the beginning of each meeting, there's a few routine things that we do. One is we read the following statement for the record, so that everybody is clear about our behavior as Board members.

Board meetings are spontaneous by design, even though it's kind of a daunting, stuffy environment we have with reporters and what-have-you, we nevertheless express ourselves pretty freely, and we want to be able to continue that. But, when the Board members speak extemporaneously, it is important to realize that we are speaking on our own behalf, not on behalf of the Board. When a Board position is articulated, we will do our best to make that known to you.

In December last year, the Board sent a letter report to Congress, last year, that was just a couple months ago, we sent a letter report to Congress and Secretary of Energy highlighting some Board findings from the calendar year 2005. The findings contained in that report are based on information obtained during 2005 at various fact-finding meetings, public meetings, laboratory visits, and field excursions. And, there are copies of that letter report available to you out front. The findings that we made in that report are grouped into four areas, and I will briefly highlight some of them now.

In the area of the waste management system, DOE announced a decision to evaluate a canister system for transportation, aging, and disposal, thus gave birth to another acronym known as TAD. The Board believes that this system warrants examination, and recommends that DOE determine first-hand the compatibility of possible TAD canister designs with the capabilities for storage, handling, and transportation options at each reactor site. We are also following the development and use of DOE's Total System Model, given the model's potential to increase understanding of how everything fits together, of the interactive effects of various waste management system design and operational components.

DOE's analyses and understanding of the engineered system continue to evolve in a number of areas. However, the Board reported that it did not find compelling DOE's arguments for screening out localized corrosion of the waste package from deliquescent salts at high temperatures. And, the main reason we reached this finding was that the data that we saw that was presented to the Board were not for temperatures above 150 degrees centigrade.

DOE's studies of the natural system and natural processes continue to enhance fundamental understanding of their potential behavior. For example, DOE presented experimental data indicating that the transport of neptunium may not be significantly delayed by co-precipitation, unlike uranium. The Board is encouraged that the Office of Science and Technology continues to support fundamental investigations regarding source term phenomena and the radionuclide transport characteristics of the natural system.

Finally, with regard to repository performance assessment, the Board recommended that, in addition to and parallel with the repository compliance case, the performance assessment being the primary document against which the technical issues are resolved in the licensing of the repository, that the DOE should also develop a realistic analysis of repository performance. The Board believes that such an analysis would be invaluable for fundamental understanding, for informing key constituencies, and for building confidence in the DOE's estimate of repository performance.

Now, let me turn to today's meeting, and set the stage, if I may.

In evaluating the technical validity of DOE activities, the Board considers a number of factors, including (1) the relevance of the activities to the performance of the total waste management system and repository systems. By performance, we mean safety and throughput. (2) the extent to which the activities are integrated and compatible with the total waste management and repository systems, and (3) the extent of fundamental understanding of the technical and scientific issues involved. To make these determinations, the Board assesses project priorities and issues on a regular basis. And, we have such a session scheduled later this month, as a matter of fact.

An example of this is today's meeting. The agenda focuses on specific factors affecting the radiation dose at the accessible environment of the proposed repository. Key parameters for assessing radionuclide processes include the mass flux of water and relevant radionuclides entering and exiting the different barriers and zones of the repository. Barriers to radionuclide mobilization and transport into the natural system include the surface geology above the repository horizon, the engineered barriers consisting of the drip shield, the waste package, and the invert; and the byproducts of the degradation process involving different chemical and mineral phases of the mobilized waste. The quantity and chemical form of the radionuclides in the source term have a major impact on radionuclide transport, retardation, and retention in the natural system.

Our objective is to evaluate DOE's fundamental understanding of radionuclide containment and transport processes that dominate the radiation dose at the accessible environment. That's what we want to do today. Such understanding includes knowledge about uncertainties in dose calculations over times that include the peak dose and the likely behavior of the repository system and its individual components.

Fundamental understanding of radiation dose calculations implies knowledge of the processes that affect radionuclide behavior and how such processes might be perturbed by episodic events such as earthquakes and volcanic activity, and knowledge of the behavior of different waste forms and how they contribute to radionuclide transport. It also implies a basic understanding of the methods of analysis and their connection with the supporting evidence, be it from the site characterization program, analog investigations, or general scientific knowledge.

Of particular interest to the Board are DOE estimates of the amounts and chemical form of the dose-contributing radionuclides entering and exiting different hydrogeologic units within the unsaturated and saturated zones of the repository. Increased understanding in this area could provide a better accounting of the contribution to radionuclide retention and transport of specific elements of the repository system while providing greater visibility into the difference between the flow of water and the transport of radionuclides, a difference that could prove to be important in demonstrating the waste isolation capability of the repository.

One of the complications of modeling the disposition of radionuclides in the proposed Yucca Mountain repository has to do with scale. Because of the relatively low water seepage rates and small quantities of radioactive material (mass and curies) exiting the engineered barrier system in comparison to the very large scale natural hydrogeological system, it is extremely difficult to take full credit for the geochemical processes that may be important to quantifying the isolation capability of the natural system. It is a matter of grams and curies of a few radionuclides interacting with megatons of material. In part, this is why the Board has asked DOE to present predictions of mass and radionuclide activity fluxes over time and space at the interfaces of the repository subsystems. The Board believes that a clearer understanding of such scale issues and the processes involved could lead to greater confidence in the performance assessments of the repository.

Now, a few words about the agenda. Today, DOE and its contractors will be making four presentations, beginning with an introduction by the almost new Chief Scientist of the Office of Civilian Radioactive Waste Management, Dr. Russ Dyer. And, Russ will explain what I mean by almost. Russ has many years of valuable experience with the program, and we look forward to hearing his thoughts. And, he's certainly no stranger to us.

Next, we're going to hear from Mike Ryan. Mike is chairman of the Nuclear Regulatory Commission Advisory Committee on Nuclear Waste, and will present his views on conservatisms, non-conservatisms, and uncertainty in dose calculations and the risk-informed approach to dose calculations. As a health physicist, an editor of the official journal of the Health Physics Society, and Chairman of the Advisory Committee on Nuclear Waste, Mike has substantial expertise in the human health effects of radioactivity.

Next up following Mike will be Tim McCartin of the U.S. Nuclear Regulatory Commission staff describing the implementation of a dose standard beyond 10,000 years.

Following Tim's presentation, the agenda will focus on mass and activity fluxes through repository subsystems, first of water seeping into and out of the drift tunnels. And, after lunch, we will discuss radionuclide releases from the waste package, waste form, and drift tunnels over time. We will then have a presentation describing the mass and activity of key radionuclides potentially released from the unsaturated and saturated zones over time. The last presentation will be a study by the DOE Management and Technical Support describing their peak dose sensitivity analysis over a one-million-year time frame.

And, as usual, following the presentations, we have scheduled time for public comment, which is an aspect of our meetings that is extremely important to us. If you would like to comment at that time, please enter your name on the sign-up sheet at the table near the entrance of the room. Of course, written copies of any extended remarks can be submitted, and will be made part of the meeting record. Some of you have asked about questioning during the course of the presentations. Our preference for that would be for you to write down your questions, and submit them to Linda Coultry, she will be visible in the back, and leave this information at the sign-in table. And, we will cover as many questions as we can, time permitting.

And, then, finally, I would like to ask all of you to turn your cell phones and pagers to the silent mode, including myself. I was guilty of not doing this yesterday.

Without further ado, I am pleased to introduce Dr. Russ Dyer. Russ has recently assumed the role of Chief Scientist for the Program, a position that the Board has long recommended. He brings a wealth of experience to the job, and we look forward to hearing his views.

Russ?

DYER: Thank you, Dr. Garrick. I would certainly like to welcome the NWTRB back to Las Vegas, and for visitors for whom this may be the first time here, welcome.

Let me take a minute and talk about this facility that we're in, because it's relatively new here in Las Vegas, and it is unique. Across the way is the Atomic Testing Museum, and if you have a chance to visit the Atomic Testing Museum, I certainly urge you to do it, because it captures the history of a program, a facility and a group of people that had far-ranging impact on science and technology, not just in Nevada, but in this nation and throughout the world.

My own experience in there, an hour in that museum will just whet your appetite. It takes about three hours to really do it justice if you're interested in the progress of technology and the role that atomic testing had in that.

With that, let me get started. For the record, I'm Russ Dyer. I am currently the Assistant Deputy Director for Science and Technology of the Office of Repository Development. But, I'll tell you a little bit later what Dr. Garrick was alluding to. We have a change underway within the program.

I'm pleased to be here today to address the Board. Since our last meeting in November, there have been a number of changes, organizational changes, in the Department, and progress has been made in engineering and science and we'll detail some of that progress for you today.

Today, I'll provide the Department of Energy's Office of Civilian Radioactive Waste Management program and project overview. The overview will focus on organizational changes, the funding for fiscal year '06, engineering and science. Afterwards, I'll take questions from the Board on the points I present.

In organization--let's go ahead and have that slide up, if we could, please--there have been a few changes to the OCRWM organization since the November meeting, and more are forthcoming.

The announcement of Edward Sproke as the nominee for the Director of the Office of Civilian Radioactive Waste Management was made some time ago. Mr. Sproke is awaiting confirmation. Until his confirmation, Paul Golan is the acting director. Whenever Mr. Sproke is confirmed, then Paul will drop back to his permanent position as the Principal Deputy of OCRWM.

Now, what we have up on the screen here is an organization chart that was just approved a few weeks ago, but is not yet in effect. We need a transition period to move from the existing organization to this new organization, and I'm going to talk about some of the key points on this organization.

This is, let me first just talk about some of the general philosophy behind this organizational construct. It's a flat organization. All of these managers, managerial offices, report directly to the Principal Deputy here. It does away with the distinction between east and west organizations. You will notice that there is not--the Office of Repository Development is not on this. So, we have organizational structure that will employ people both in the east and in the west.

It shifts several responsibilities and functions. I'm going to walk through some of those. There are 13 direct reports to Paul Golan. And, let me talk about two boxes on here to start with. As Dr. Garrick mentioned, I've been named the Chief Scientist, so the Office of Chief Scientist is here on the left, and there's a progression here, which goes from, stepping across from Study, Design, License, Build, and Operate, from left to right on here. So, we have the Office of the Chief Scientist--for those of you in the back of the room, I'll leave these out, so it will take a while to get through this. The Office of the Chief Engineer here, Paul Harrington is currently acting in that position. That's one of the positions. We've had two positions that we have active recruitments on. That's one that we hope to fill very soon.

The other that we have an active recruitment on is in the Office of Quality Assurance. Vinnie Brown vacated that office, and Mike Ulshafer is acting in that office there. Continuing on across in Licensing, we have a new individual in the Department, Mark Williams. In the new construct, he will head up the Regulatory Authority Office. Currently, he's taking the place of Joe Ziegler in the Office of License Application and Science.

Let me tell you a little bit about Mark Williams. Mark joined us in November as the Director of the Office of License Application and Strategy, soon to be the Regulatory Authority Office. Mr. Williams previously worked for the Department of Energy's Office of Environment, Safety and Health, where he was involved in the safety of DOE nuclear facilities for 15 years. He also has more than 25 years of nuclear related experience, including licensing and reactor regulations at the Nuclear Regular Commission, process control and engineering associated with the light water reactor at Bettis Atomic Power Laboratory and Testing on Naval reactors at the Pugette Sound Naval Shipyard.

If I continue on on the slide, down at the next level, we have an Office of Waste Management, Chris Kouts will be the manager of that office, and Office of Logistics Management, Gary Lanthrum currently of the National Transportation Program, the Office of National Transportation, will be heading up that office. And, there is an office on here that is vacant, and that's Disposal Operations, and that is a construct that we're putting in for the future. We don't intend filling that position for the foreseeable future.

Down at the bottom level, we have what I'll call the support functions, the Office of Project Controls, Ken Powers currently the associate deputy in the Office of Repository Development, will be heading up the Office of Project Controls. The Office of Procurement will be Suzanne Mellington. The Office of Government Services, Rich Minning, and finally, the Office of External Affairs, Allen Benson. So, those are the 13 boxes in the new organization that we'll be standing up over the next several months.

Let me move now to the next topic, which is Lead Laboratory. Several weeks ago, in January of 2006, the Department announced the designation of Sandia National Laboratory as the lead laboratory to support the Office of Civilian Radioactive Waste Management. As Lead Laboratory, Sandia will provide management and integration services for all Yucca Mountain scientific programs, including the science that supports OCRWM's license application, and its defense in the Nuclear Regulatory Commission's review process. Sandia will perform this work in collaboration with supporting organizations, such as other national laboratories, subcontractors, federal agencies, universities, and expert panels.

Lead Laboratory will fall under the purview of the Chief Scientist. Let me go back a bit and talk about some of the other things that fall--that have been listed as expectations within the Office of the Chief Scientist responsible for coordinating scientific work on the program, that's all scientific work throughout the Office of Civilian Radioactive Waste Management, including post-closure science, the Lead Laboratory and the Science and Technology Program.

The Chief Scientist will be the program's primary point of contact with international groups or agencies, such as the International Atomic Energy Agency, and the Nuclear Waste Technical Review Board.

Let me move now to the fiscal year '06 budget. As discussed in the November meeting, Congress approved $500 million in OCRWM funding in the fiscal year '06 appropriation, of which $50 million was reserved for reprocessing. OCRWM is currently seeking a legal interpretation to determine if OCRWM or another DOE program, for instance, the Office of Environmental Management, is authorized to spend the $50 million. As you are aware, there is a legal foundation for the missions and functions of the Office of Civilian Radioactive waste Management.

The $450 million appropriated is $172 million below the fiscal year '05 funding levels, and $201 million below the fiscal year '06 request. Recently, the federal government announced a 1 per cent rescission on top of other reductions in the federal budget. That's across the board. This rescission will result in an additional $5 million reduction to the OCRWM fiscal year '06 budget.

The Department is in the process of developing its fiscal year '06 annual work plan to establish program priorities in light of this budget reduction. Critical Decision 1, which we talked about previously, that's the corporate decision making process within the Department of Energy for major systems. We have a proposed change that we would be submitting to the DOE decision hierarchy associated with the incorporation of the transportation, aging and disposal of canisters into the system concept. This is the TAD concept that Dr. Garrick talked about. And, requirements management are some of our top priorities. So, CD-1 and requirements management.

In addition, the Department is continuing processing of refinements to the Total System Performance Assessment. However, until the annual planning is complete and impact of the Critical Decision 1 decision and the overall program is determined, detailed priorities and science are not yet set.

Let me move now to engineering. And, most of this is associated with work being done to support this Critical Decision 1 milestone. As stated in the November meeting, the Department announced its plans to simplify the design and operations of surface facilities. This simplified approach involves changing from a bare fuel model to one based on the use of TADs, the transportable, aging, disposable capable containers. The Department directed its contractor, Bechtel SAIC Corporation to, one, stop work associated with activities that supported primarily their fuel handling at the repository, other than that required for limited bare fuel and off-normal operations, and, two, to develop this package of supporting information to support the Critical Decision 1. And, that would be a package that includes conceptual design that addresses a simpler surface facility and canister operations, including, as I said, the conceptual design and associated planning information.

The Department currently plans to submit the CD-1 package to the Energy Secretary's Acquisition Advisory Board for review and approval in the spring of this year. If the Board gives its approval, the Department will proceed to implement the TAD concept.

The Department agrees with the Nuclear Waste Technical Review Board that the TAD design will require close coordination among DOE, utilities and the regulator, and this will be very important in the selection process to ensure compatibility with repository operations. The Department expects to pursue the TADs concept, consistent with this principal.

Let me move now to requirements management. In December of 2005, the Department issued a letter to Bechtel SAIC stating effective immediately, no engineering and preclosure safety analysis technical work products, subject to the quality assurance requirements and description document, may be approved until DOE re-validates the Yucca Mountain Project technical requirements baseline, and contractually conveys applicable requirements.

The Department's action was the result of a recent OCRWM concerns program investigation into allegations that the project had not maintained or properly implemented its requirements management system, resulting in potential inadequacies in the design and control process. Since these findings raised uncertainty regarding the adequacy of design and products developed under the YMP design and control process, the Department has made resolution of these issues a top priority.

To resolve this issue, the Department has taken immediate actions, has plans to take remedial actions, and actions to prevent recurrence. And, some of these actions, I'm going to list now.

First. The Department of Energy re-validates the project's technical requirements baseline, and contractually conveys these applicable current requirements to BSC.

Secondly. BSC procedurally maintains and flows requirements down to implementing mechanisms for programmatic requirements or products for technical and procurements requirements.

Third. BSC internally validates that processes exist and are being followed, and requirements are appropriately traced implementing mechanisms and products.

Fourth. BSC provides DOE documentation that their actions are complete. And,

Fifth. DOE verifies that the BSC processes exist and are effective.

Let me now move to science. The DOE presentations on today's agenda will cover a range of post-closure topics that affect the assessment of dose. The primary emphasis and focus will be on processes and models developed for 10,000 to 20,000 year time frame of performance.

For today's topics, the presenters will provide their best estimate of performance critical parameters under the constraint that assumptions made need to be well justified. This will allow the Board the opportunity to more fully explore the appropriateness of these assumptions in these technical areas. We have an aggressive agenda today. With the limited time allotted for each topical area, each DOE presenter will provide background information, then summarize the basis and significance of the key processes. Significance described with respect to individual components will not be discussed, since the TSPA is still in draft.

In addition to processes and models, the Department will also present scoping peak dose analysis, conducted to provide insights into the load of different processes and events on peak dose, and additional information on other features, events and processes not included in the analysis.

I'd like to caution the Board and the audience that the peak dose results presented today are informative in nature only. The results are not intended to demonstrate compliance with any standard, and, thus, should not be compared to any proposed or final regulation.

In addition, the information presented on processes and models represent what is in the current project baseline, which, as I said, is subject to change if the baseline changes as a result of the fiscal year '06 planning process, namely, the incorporation of TADs into the baseline.

In summary, the Department is committed to examining canister-based design and operations as a top priority. The Department believes that the best operations are those that are simplest and most straightforward. The canisterized approach incorporates this philosophy. The Department understands that the selection of TADs is important, and the coordination among DOE, utilities and the regulator will be crucial to the selection process to ensure compatibility with repository operations.

Annual planning is underway. Upon completion of the annual planning and the CD-1 process, the program's priorities and a new baseline schedule for license application submittal will be established. In addition to Critical Decision 1, the Department anticipates that requirements management improvements to the Total System Performance Assessment will continue to be top priorities on the program.

That's the end of my prepared notes. I have one personal note that I would like to add here. About 16 years ago, I started interacting with a brash young staffer with the Nuclear Waste Technical Review Board, and we were struggling at that time with developing a way to estimate performance of a geologic repository system over a very long time period. And, Leon was involved in some of those early very chaotic meetings, and I have fond remembrances, and we will miss Leon. So, thank you, Leon.

With that, I would like to take any questions from the Board.

GARRICK: Thanks, Russ.

Okay, let's see what the Board has to say. Henry, and then Andy.

PETROSKI: Petroski. Who indicated that under the rubric of engineering, that the design was going to be looked at with the view towards simplifying it, going to a simple design? Does that mean you're going to look at the existing design and try to simplify it, or is there going to be an effort to go back, say, to square one and--is it a simple design from scratch?

DYER: I think it depends on what opportunities were given by the TADs concept, incorporating the TADs concept. There's some of that that can be done with I think relatively minor changes to some of the existing components. There may be great benefits from introducing a whole new approach, and I haven't been that close to the design process, so I can't tell you for sure where we're headed for that.

GARRICK: Andy?

KADAK: Kadak, Board.

I have a number of questions, but I'm particularly interested in what you think the schedule is for this project. It didn't appear that you said anything about that. Could you kind of fill us in about the licensing submittal? It sounds like you're going back to the drawing boards, as Henry just mentioned. Give us a flavor as to where you are relative to timeline.

DYER: We are trying to develop a timeline. We're trying to figure out exactly what we need to do, what the work that needs to be done, and then we'll work out the schedule of how to accommodate that work. We do not have a schedule yet.

KADAK: And, just relative to your role, I was also a bit confused, and in one statement you said was surprising, you haven't established detailed priorities in science yet. I mean, explain that, please. I'm surprised.

DYER: We have some alternatives that are on the table. Bechtel SAIC prepared a proposed work package that we are in the process of reviewing now. It accommodated earlier priorities that we had in the program, and they believe that it accommodates the priorities we've placed on incorporation of TADs into both the design and into the performance assessment.

We're looking at ways that we might enhance that. I was involved in meetings fairly late last night, looking at what we really need to do to put the best program before us. So, I would say that we have maybe 80 per cent of the program pretty well understood, but there's quite a bit that we're going to make sure that if there are important things that need to be done now, that we bring them into the program now.

KADAK: Thank you.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

I took your comments, Russ, to imply that the Department of Energy is pretty much adopting the strategy of full utilization of TADs to the maximum extent possible. And, I was just curious whether the decision has been made up until this point in time on studies that DOE has been conducting alone, or whether there have been discussions with some of the other stakeholders, particularly the utilities, about the transfer of risk to utility workers, because more of the bleming activity will be going on at the sites, the willingness to take fuel out of dry storage and put it back into pools for repackaging, and a number of issues that if I were a utility, I would want to have a seat at the table. Could you please comment?

DYER: There have been, and there will be discussions with the utilities that have been a fairly informal level to date. I expect that that will kick up with time.

GARRICK: Ron?

LATANISION: Latanision, Board.

Russ, I'm curious if at this stage, you could give us an indication of the organization of the office that you will lead in terms of the staffing? Is it premature to do that, or--

DYER: It's premature. Right now, I have a staff of one, me.

LATANISION: May I make a suggestion? I've always felt the absence of someone who would take responsibility or would be the visible leader for such issues as materials, for example. And, I think that senior leadership has always been a concern to the Board, not only in terms of materials, but joining processes, and so on and so forth. I would just recommend that you should consider appointing to your staff some people who provide that senior leadership, because I've always felt that absence.

DYER: Well, one thing that I'll inherit is the current Science and Technology Program, which, as you're aware, has the thrust areas, of which materials performance is one of the thrust areas, and there are other thrust areas. And, there's pros and cons of bringing some kind of an expert body such as that into the mix. I'll be looking at that.

LATANISION: Latanision, Board.

Just to follow up. I mean, to be really blunt, I mean, if I were asked who is in charge of materials on this project, I think there should be an answer to that question. And, right now, I can't tell you, and I'm not sure that anyone can, who is responsible for materials, and it's a critical issue in terms of the waste package, and so on.

DYER: I don't disagree. This will give us an opportunity to do that, because everything will be under the Chief Scientist.

LATANISION: Thank you.

GARRICK: David?

DUQUETTE: Duquette, Board.

As you know, I was privileged to attend a recent meeting of materials issues relative to possible changes in the canister design, using amorphous alloys instead of C-22 for the outer layer. With the TAD concept being reexamined, or examined, you can look at it either way, there was a TAD concept, I think, some years ago, will the Science and Technology Program impact on the license application, on the TAD design, will it change the design of the repository? What's going to happen to the new concepts that are being introduced on the materials side for the canisters?

DYER: Right now, we still have that program, as well as other structurally amorphous materials, the specific program

Dr. Duquette is talking about. We're still in an R&D program on that, so we still have some things to prove out to ourselves, and one of the things that we need to understand is when and how to bring advancements such as structurally amorphous materials into the program. When is the appropriate time to do it? Right now, we still haven't convinced ourselves that it's at a stage that we're ready to bring it into the program. As you're aware, there's some pretty aggressive testing that's due this year, and into next year, and I think that will--that gives us the opportunity to improve our confidence that those materials have potential advantages associated with the use of those materials can be brought into the program. I think, actually, we've got a meeting next week between the S&T Program and the Design Program, here to talk about opportunities and talk about timing, start working the details that you're talking about here.

DUQUETTE: Duquette, Board.

The first one, I want to congratulate you on supporting that program, even if it doesn't work out, it's what the, I think, the S&T Program was supposed to do.

But, my question really had more to do with if you're going to be redesigning the containers as TADs, will the new materials aspects impact that design process, and will that, in turn, impact the design of the vault itself?

DYER: In the near term, I would say no, because I don't think that program is mature enough for us to bring it into licensing at this point in time. In the future, it may well bring about a change to the licensing basis.

GARRICK: Okay, I have questions from Bill Murphy and Thure, Andy, Howard and myself, and then we'll probably cut it off and go to our next speaker. So, Bill?

MURPHY: Bill Murphy. I'm the consultant to the Board.

You mentioned that we'll see results that are relevant to a 10,000 or 20,000 year time period, and I recognize that a great deal of work has been done to evaluate how the system will work on that time scale, do you perceive a different set of issues or a different set of priorities to the scientific problems associated with a million year time scale?

DYER: To a large part, that depends on the final form of the EPA standard, and NRC's implementation of the EPA standard. If you can use and justify the use of the models for the 10,000 to 20,000 year period and extrapolate those out in time, if there were some models that can't be legitimately justified, you've got to identify what those are and how you're going to address them. And, I don't think there is an easy answer to that.

GARRICK: Thure?

CERLING: Cerling, Board.

It's always unpleasant to find out you only have 80 or maybe 70 per cent of the money that you anticipated. And, so, my question has to do with does this mean that you will cut things across the board at 70 per cent? Are there key pieces of the puzzle that cannot be funded at the appropriate level and will cause a significant time lag, and what sort of time lag does this sort of funding inequity result in, and how are you going to make your case in the next two to three years that this has to be changed?

DYER: Well, identifying what the critical pieces of work are that need to be done to support a path forward, either in science or in the design and engineering arena is where we're putting our priorities now. Much of that depends on what comes out of the conceptual design associated with this Critical Decision 1 package, and how we see the impacts of that propagating through the system. And, then, responding to that to make sure that we have the work in place to support that. So, I see that being a key priority.

As you're well aware, we've got a lot of relic issues also that we have been and will be working on, and it's going to be a real challenge to make sure that those don't get pushed off the table as we focus on emerging priorities.

GARRICK: Howard?

ARNOLD: Arnold, Board.

Dr. Duquette alluded to the fact that TADs had been considered in the past and rejected, and we still have some uneasiness about the implementation. I think it's easy to see the advantages of TADs, but the disadvantages I think need to be addressed and put to rest. as you alluded to, our uneasiness over some aspects of it, and I think it would be really worthwhile to make a connected story out of why the previous decision was wrong.

DYER: I don't disagree with you. I'm not prepared to do that right now. I can offer something to the Board. There was kind of a lessons learned that we put together in a multi-purpose canister, as it was called at that time, which we can make available to the Board. I think it's somewhere in the archives, I'm sure. But, I would suggest that might be a good topic for a future meeting also.

GARRICK: Andy?

KADAK: Yes. I have to prioritize my questions, because I had so many.

GARRICK: Please do.

KADAK: I will try. All right, I'll make two brief ones. One is I'd like to really understand the status of the Total System Performance Assessment. I understand there's been a review made, quite critical, wondering what you're doing to resolve that. That's question number one.

And, two, there's been a lot of discussion on the

Global Nuclear Energy partnership, which talks about solving the world's nuclear waste problems, and I'd like to know what the impact of that program might be on Yucca Mountain.

DYER: Okay. Let me take the second one first. It's hard to say, because I haven't seen a coherent one. We, the nation, hasn't seen a coherent policy or any legislative proposal associated with that yet. We see inklings of it, for instance, the $50 million in the appropriation for this year that was associated with reprocessing suggests that there is a sense of urgency and importance in bringing back reprocessing into the nation. But, I haven't seen, or we haven't seen an overall context that that fits in. So, it would be total speculation on my part to say how that's going to all fit together.

As far as the Total System Performance Assessment, Bechtel SAIC had an independent review, looking at the TSPA. They've been looking at it since sometime in '04, I believe. There are a number of comments, some are quite critical, some we agree with and they need to be addressed and fixed, some we disagree with, and trying to figure out how to move forward in that process is one of the challenges that we have.

KADAK: But, do you have any estimate as to when that will be resolved?

DYER: I would hope that we would be able to put that behind us by this summer. But, it's clear that the TSPA that was being reviewed by that team is not the TSPA that we'll be able to take into licensing. So, we'll have to start work on a follow-on TSPA that accommodates the new EPA performance requirements.

GARRICK: Two quick questions, Russ. Garrick, Board.

Number one, is it intended to have your counterpart in the Lead Lab, in other words, is there going to be in the Lead Lab a Chief Scientist for Yucca Mountain specifically?

DYER: I'm not sure how the Lead Lab proposes to construct things. We're starting discussions actually tomorrow about the transition. It's going to take a while for the Lead Lab to stand up, just the administrative framework of how to do project control, quality assurance program, how we are actually going to flow money to and from Sandia to the other organizations. So, I don't know the answer to that.

GARRICK: Yeah, I'm thinking of kind of the model of the Waste Isolation Pilot Plant, where there was such an approach.

DYER: Since one of the reasons for selecting Sandia was because of their experience in the Waste Isolation Pilot Plant, they may well choose to bring some of the organizational structure and approaches that they use on WIPP to this program. But, so far, we haven't talked about that level of detail.

GARRICK: A final question. You only have to read the paper to learn that DOE, or the Department, is considering a number of initiatives that could impact the management of radioactive waste. And, one of the issues of some concern to the Board is how these initiatives might impact the course forward and the focus relative to Yucca Mountain. I'm thinking of initiatives having to do with reprocessing and spin-offs from the Generation Four studies, and what have you.

Can you say anything about what--and maybe this gets into the Critical Decision business that you mentioned earlier--can you say anything that would give us some confidence that the Yucca Mountain project is not going to suffer from these initiatives as a result of any possible distractions, or the feeling that now there are alternatives are developing, and that's a basis for delays, et cetera, et cetera?

DYER: If you go back in time five to eight years, some of the same issues were being brought up--this is when partitioning and transmutation was a big thing--I think there's recognition on all sides that no matter what system you go with, reprocessing, if you are able to get a transmutation program eventually in place, all of these approaches require a geologic repository of some kind. And, I think there is a realization that under any approach, you need a geologic repository. I haven't seen anything that takes away from the need for a geologic repository. Some of the approaches, some of the proposals may balance the urgency of it, but the ultimate--

GARRICK: Well, it's the urgency part that I'm most interested in. I agree with you that a repository is needed, no matter what alternative is considered. But, the question really focuses on the impact that these initiatives have on the Yucca Mountain Project. And, that's all I was trying to address.

Okay, thank you very much. It's a good start. We're only a couple of minutes in time, so now we'll hear from our distinguished visitor from the Nuclear Waste Group in the Regulatory Agency.

RYAN: Mr. Chairman, members of the Board, thank you very much for your invitation to be with you today. I had planned to be with you a few months back, but took ill and was in the sad position of not being able to speak, which is always troublesome. But, I appreciate the invitation to come again today.

What I would like to do today is to share with you some ideas on conservatism, non-conservatism, and uncertainty in dose calculations, and how to risk inform our thinking perhaps on dose calculations. We very often hear about dose conversion factors in whatever country, whatever venue, and there are lists and tables of dose conversion factors in one form or fashion, and what I want to do is highlight for you today where they're certain, where they're uncertain, and how we can improve our thinking about such dose conversion factors.

Next slide, please. The views that I present are my own, so I want to share with you that thought.

Back in history, 1999, the U.S. Nuclear Regulatory Commission issued a policy paper that described its risk-informed and performance-based concepts and how it should apply to NRC's regulatory work. It's an important benchmark for radiological performance assessment, and I think it should be extended to our thinking about dose conversion factors as well.

I'd be remiss if I didn't take the opportunity to put forth the risk triplet of Kaplan and Garrick of what can go wrong, how likely is it, and what are the consequences. I promised John when I was on the ACNW I'd use this slide in almost every talk I give somewhere. So, I'm happy to give it. But, it's not a set of questions, it's not inappropriate to ask about dose factors.

Do the metabolic models that we ascribe to them in the reference calculation fit the circumstances at hand? We now have, for example, dose conversion factors for young adults, for children, and for adults. Do the chemistry and physics of the setting in which we assumed various physiologic parameters represent the setting in a particular case at hand, and so forth. So, there are things that can be different in dose conversion factors, and there can be consequences in what we calculate for dose.

If we use some risk insights from assessments using these principles, we can highlight important radionuclide contributors to risk and their significance. We may, in fact, find some radionuclides that we don't need to worry about that we classically have worried about, and we might find some radionuclides that we have to pay a little bit more attention to the details of what one calculates for a dose.

Traditionally, there are a range of tools that yield a range of results in our understanding of risks and performance assessments. We have gone from extreme bounding cases, the case of assuming everything that can go wrong does go wrong, and we calculated dose, and if that numerically satisfies us against some standard or against some way of thinking about it, we say good enough.

Next is bounding analysis, which is a little less extreme in the assumptions, followed by sensitivity studies where we examine various issues, parameters, whatever it might be to say what happens if, and then one-off calculations and comparisons where we take these kinds of analyses and gain insight and understanding from comparison. And, then, finally, probabilistic risk assessment, which is a more fundamental and perhaps better way to do some of these analyses.

The applications for risk-informed approaches, of course, apply to all aspects of a performance assessment. Inventory. People often assume that's a trivial matter, but it's something that needs attention. What is the inventory? What will the inventory be over time? And, what physical and chemical form might it be in?

The source term. What fraction of the inventory leaves and goes somewhere else, and under what process? What are the released fraction of radionuclides, what gets released from the engineered system? Does that change it in some way physically, chemically, or otherwise? What are the interactions in the near field, that is, where is the source and the packaging and the system influence still exist, and what are the interactions in the far field? Typically example of a far field parameter of risk significance is Kd. What is a Kd? What are the geochemical processes that can impact? And, then, finally, uptake and dosimetry estimates.

We spent an awful lot of time over the years talking about Kd and transport, but we haven't talked much about the equivalent parameters in the intake and dosimetry calculations, and there are some of interest.

Internal dosimetry 101 is there are inhalation exposures typically involving, but not exclusively involving, the respiratory tract. It's true that you cannot have an inhalation exposure without having an ingestion exposure. Part of what you breathe in ends up in your stomach. So, it's a complicated situation. There are ingestion exposures directly in food and other stuff. And, then inunction for the completion of the three I's is skill absorption, which is mainly an issue for tritium and radionuclides that enter in the workplace through wounds or other kinds of exposures where the skin is broken.

In the inhalation case, aerosol science is a key part of assessing, for example, workplace exposures that have little substrate for the radioactive material, a much different circumstance than inhaling dust where the actual physical dust itself can be a big part of the exposure.

Environmental exposures can involve significant amounts of substrate mixing and interacting with the radioactive material. So, does that infinitely dilute solution of radioactive material behave the same when it's interacted for decades or hundreds of years with earth materials, and does the exposure result in the same dose?

Biological sciences are also important with the aerosol science where we deal with solubility, deposition, absorption and clearance kinetics. All of these factors can significantly influence dose.

An interesting reference point in this arena is physiology. My typical dose factors assume that for light work, we inhale 0.54 cubic meters of air per hour, with a respiration rate of 12 per minute. Light exercise is 1.5 cubic meters of air per hour, and a respiration rate is 20 per minute. Well, obviously, this is a direct scaling factor to what's inhaled. I guess I'm not in very good physical shape, because my respiration rate when I'm doing work is much higher than 20 per minute.

And, the other assumption is that all the air breathed in is breathed in through the nose. I don't think that's the case for most strenuous activities. I actually did a little study, I look around the room and see who has their mouth open and who has just--and, believe me, we're not all nose breathers.

GARRICK: Actually, the ones that are asleep.

RYAN: So, I find this interesting in that it is an interesting reference point, but I think we owe it to ourselves to examine and risk inform our calculations, our particular setting and our circumstance to determine whether or not this is useful and applies and is accurate for what we're trying to accomplish.

And, I'll pick on another one. The GI tract uptake fraction. The selection of a GI tract uptake fraction is a scaling factor to dose. In ICRP reference documents, you will find 5 times 10^{-4} is the moderate case where absorption is moderate for unspecified compounds. And, for slower insoluble oxides, it's 1 times 10^{-5}. In 1983, I'm sad to say, 23 years ago, Dave Kocher and I did a review of GI tract uptake fraction estimates, and at that time, came up with the number of around 10^{-3} might be more appropriate for environmental species.

Again, when the doses are fractions of a micromillirem per year, this may or may not be important. But, in terms of demonstrating understanding, I think it could be very important, just to cover the base and make sure that we do understand the basis for these dose factors, particularly in the plutonium case, and understand whether or not that's helpful.

Very often, in the absorption case, tritium, about 50 per cent of the tritium that's inhaled is assumed to be absorbed through the skin. If somebody is in protective clothing, for example, that may or may not be true. But, in the worker case, we always rely on bioassay. If you take bioassay measurements of all sorts, if there has been as exposure, and what do we do for that individual? We develop an individual specific biokinetic model. We then kind of set aside the dose conversion factors and do it through the individual. So, it's interesting to see the range of values.

Very often, the individual model doesn't look too much like the ICRP model.

HORNBERGER: Is that tritium as hydrogen?

RYAN: Usually, there's a water vapor. It's very rare to see a workplace exposure with anything other than water vapor, and hydrogen, of course, exchanges very rapidly with the moisture in the air. So, it's almost immediate. In fact, an intake of tritium uniformly distributes into hydrogen pool in the body in about two hours.

Another example. I-129, there's lots of interest in I-129, but in a recent paper that Dave Moeller and I published, we looked very carefully at this, and it is very dramatically dependent on the stable iodine daily intake rate. If, for example, the daily intake rate is around 400 milligrams per day, which is one of the key references we found, you over estimate the dose using the reference dose factor by a significant amount.

The story continues however. One of the interesting comments we received out of the publication of that paper was that recent data by Hollowell and others have suggested 150 micrograms per day. And, guess what? That would under estimate the dose, if that's the intake rate and you use a reference dose factor.

The important message here is that in this case, as in the case for Carbon 14, the stable element of carbon, the dietary intake pattern directly influences the dose conversion factor. And, that should be accounted for. Interesting to think about. The reference factor, by the way, in ICRP Publication 72 is based on an intake rate of 200. So, if you're 400 intake rate, you're over estimating the dose by a factor of 2 just on that one parameter.

Carbon 14, the same way. It's interesting that there are variations by a factor of about 1000 on intake assumptions for food, water and models. The key assumptions count, and we actually summarized, and look at the estimated dose based on various assumptions, from 10^{-2} microsieverts per unit intake, to 10^{-5}. All legitimate assumptions in the various settings in which they were assumed, but it's relatively important to make sure you understand how the stable element dilutes carbon in the dose factor.

Let me talk about some interesting extremes. 10 CFR 61 is based on an extreme bounding case. The probability of intrusion into a low level waste site is 1. That's the assumption. The probability of intrusion into Class C waste is 1. Exposure is maximized via all pathways to the resident farmer. In a way, the resident farmer has to be unemployed because he gets external exposure for 18 hours a day. He has to be an expert agriculturist because he has to grow his entire inventory of food in exhumed waste. I don't know how you grow food in ground up mop heads and coveralls and shoe covers and ground up metal. But, nonetheless, all of these assumptions were made to set the concentration values that are in 61, 54. It's a bounding case, and it's an extreme one. But, it certainly can mask overall risk, and certainly can mask our insights into what contributes to risk. That's the message.

Interestingly enough, in 61, 58, there is a provision to allow for alternative concentrations, calculations from alternative scenarios of exposure as long as the principal requirements for protecting the public health and safety are met.

RESRAD, commonly used calculation to analyze for disposition of solid material. Is it a bounding analysis? It's specific to groundwater, and it ends up basically assuming that one is using leachate as drinking water. Water is taken through some disposal unit, through a theoretical drinking water source, and there's no dilution and no dispersion in that water from the leachate. So, in essence, you're drinking the leachate. It depends on analysts choices for parameters in the models. Infiltration rate, of course, being a key one, aerial projection of the waste being another. And, again, risks may be masked or overstated. Important processes can be missed, channeling, for example, rather than sheet flow.

MARSSIM and D&D applications are a step forward.

It's a statistical approach, using, of course, the Wilcoxon rank sum statistical test to assess residual contamination. The good news here is different analysts will get similar results if they follow the method. It's appropriate in that way. It's a more rigorous approach to uncertainty analysis. The next step, of course, will be to tie those residuals to risk. How do we understand what they mean?

10 CFR 63 is a stylized and prescribed approach, a representative volume. It's required by law. Is it representative or conservative or not conservative? Does it address all the sources of variability? And, it helps to understand risks by exploring these conservatisms, both positive and negative.

In these short few minutes, I hope to introduce you to the idea that dose conversion factors and dose calculations shouldn't be left unexamined from the standpoint of how we assess risk and what's important. Should it be done for all radionuclides? Perhaps not. But, for those radionuclides that are long-lived, mobile in the environment, and give an inherently high dose per unit intake from any reckoning, it probably ought to flow to the top of the list.

We often hear about Plutonium 239, Neptunium 237, two I've mentioned, Carbon 14, I-129, Technetium 99, and perhaps a handful of others, but these kinds of detailed assessments may not change your view of their relative importance, or of their potential dose consequence. But, I think informing those calculations by understanding what variability there might be in these radionuclides and their behavior can help certainly solidify our understanding of risk.

There's are broad spectrum of approaches to risk informing decision making, particularly in the performance assessment part, and particularly in dosimetry. The goal should be that there should be realism that presents best estimates and transparent assessments of risk.

I think we need to challenge some of this old reference wisdom and dose factors, and understand the foundations for all parameters. I think more importantly, we need to know the limits of our established scenarios.

And, finally, we need to really understand when we're using extreme bounding analysis and when various other tools, like sensitivity studies, one-off calculations and probabilistic risk assessment are more appropriate. All of these can play a role in dose calculations, and all have strengths and weaknesses, and some are better than others.

So, with that, that's it. Thank you very much.

GARRICK: Thanks, Mike. Let me lead off with a kind of a question. As you know from your ACNW experience, and from the proceedings of the Board, there has always been an interest in getting to the fundamentals of what's really happening. We've talked extensively about it's important for the experts to communicate what they indeed believe is the performance capability of the repository.

This is an interesting example of being able to separate that kind of analysis from a compliance analysis. And, given that the Yucca Mountain doses are driven by a relatively few radionuclides in particular, Iodine, Technetium, Plutonium, Americium, and Neptunium, are you aware of anybody turning up the microscope on those specific radionuclides and taking into account some of the things you have mentioned here that might have an impact on, if we had such a thing, the experts' realistic assessment of the performance of the repository?

RYAN: I think, as I mentioned Dave Moeller and I have actually published now four papers on this topic. We looked specifically at I-129. We took up Carbon 14 next, and I think the other key radionuclides you mentioned also deserve scrutiny. To my knowledge, at this point, I don't know of anybody else that's done these kind of detailed probes into, you know, what the risk informed results might be for dose factors. But, it's certainly worthy of study.

GARRICK: Yes. Yes, Ali?

MOSLEH: Mosleh, Board.

Dr. Ryan, I'm looking at your conclusion and was wondering if you have any preference on the methods, approaches that you would list, and particularly, I was interested in learning your opinion about the more comprehensive use of PRA type approach to capturing all these uncertainties that you had mentioned? Would that not be a comprehensive--for addressing all these?

RYAN: You know, it sure could be. Instead of assuming a dose factor as a rigid number, you could, after studying a particular radionuclide's dose factor, make a decision as to how you might treat that in a PRA. I think that would be an interesting thing to do. Of course, it could be as simple as the dose you calculate, even with an extreme dose conversion factor, is so unimportant that it tends to fall off in importance. But, then, you have a risk insight, don't you?

My point is that by using reference factors without challenging them, and their fundamentals, that we miss that opportunity to better risk inform.

GARRICK: Mark?

ABKOWITZ: Abkowitz, Board.

Mike, I found your primmer interesting, and I couldn't help but think, as I looked at the different forms of analyses, whether or not you or someone else has actually looked at TSPA, and the various models and assumptions that are in TSPA, and tried to classify each element of TSPA in terms of what type of analysis was used to establish that particular set of parametric relationships. And, I'd be kind of curious if you're aware of, or whether you think there would be value to having a scorecard where you could break down TSPA into various components and associate with each of them the style of analysis that was used. I think that would help the Board with its understanding, probably the DOE as well, and also give us a better appreciation for what kinds of issues are propagating in terms of the biases in the models.

RYAN: Again, I'm not expert enough on TSPA to give you a full answer. Much of that earlier work was done at ACNW, and in its reviews prior to my watch. I do think that risk informing whatever performance assessment code you might be using, and the way you've described is often helpful, because it tends to allow you to focus on issues that tend to rise up in risks, rather than those that do not. Whether it's adjusted dose issues or whether it's geosphere or near field or far field or whatever it might be, I think that kind of thinking helps you to organize your priorities a little bit better.

GARRICK: Ron?

LATANISION: Latanision, Board.

Mike, could we turn to your last slide, last conclusion slide, Number 23? It seems as though there has to be a lot of judgment made on which of these approaches to use under what circumstance, and I wonder for someone who is relatively uninitiated, mainly myself, could you give us a couple of concrete examples of under what circumstance you would, as you say, they all have strengths and weaknesses, but under what circumstances would you use PRA, for example, as opposed to a sensitivity study?

RYAN: I think in the absence of other information where I don't have a foundation to make those judgments, PRA is a helpful tool that can begin to help develop those insights.

LATANISION: So, that would be the way you'd lead off. And, then, you perhaps--

RYAN: You could. On the other hand, for example, in radiation production in the workplace, I think there's such a wide body of experience that you can very often come up with sensitivity studies or bounding analyses that will tell you for a work activity, you've accomplished your radiation protection objectives. I think they all have a role. But, you know, I think we have to get a little bit more specific in when to decide. And, I think it's hard to, up front, say let's always use PRA, because that's the gold standard, and we start with nothing and end up with risk insights, I don't think that's well schooled. I think you have to look at the individual case and make that decision.

I think very often in the environmental performance assessment area, you're often in a situation where one-off calculations and comparisons, and sensitivity studies get you a long way down the road to risk informing. They can also help you design a better PRA. That's where you really want to end up.

GARRICK: The main plus of a comprehensive PRA, that's what it was invented for, is to provide you a mechanism of developing realistic models. That's when the whole concept evolved out of frustration of not having realism in the investigations of what could happen, an accident, for example, and it was the intention of PRA to fill that need, and that's why conservative or bounding PRAs are nonsensical. They don't make sense. PRA, by definition, is a realistic model.

Yes, Leon?

REITER: Leon Reiter, Consultant.

Mike, I understand a few months ago, a committee of the National Academy relooked at the linea extrapolation hypothesis, and concluded that it's still operable. On the other hand, there's a French group came out that said that it was not appropriate. I wonder if you'd talk about that, and what implication this might have for the TSPA?

RYAN: That's a great question, Leon, thank you.

I think you've characterized the Academy study that not much has changed from year one through six, that they've really recognized updates in the epidemiologic studies and a few other things, but they could not step away from the LNT as a basic model for radiation risk. I was fortunate enough to be in France a couple of weeks ago and heard a talk on their study, and, in fact, the Commission at our last briefing asked us to further investigate the French report, and we're planning now with the French to have them come and give a presentation on their report. So, it's a little premature for me to comment on their report, other than to say I believe they're recognizing a level of 1 millisievert or 10 rad. as a dose below which they're having a hard time demonstrating risk.

REITER: Let's say that they're accepted, I mean, the community accepts that maybe it's like 100 millirem, I guess--

RYAN: It's e millisievert. 10 rads. I'm bilingual, so I can help you with that. It's 10 rad.

REITER: So, what impact would this have on--

RYAN: It's a great question. You know, it depends on how you apply the idea. If you apply it to all radiation exposure, well, let's include background, let's include medical exposure that folks get as part of the radiation environment we live in, I don't know. If you also look at performance in the workplace, you know, the nuclear industry has a very clear record of decreasing occupational exposure. So, it's one of those issues that crosses from a science question, one of the risks at low dose for various kind of effects, whatever they might be, versus what kind of policy judgment do we make about it. What I can tell you a little bit about at the moment is science aspects of it. The policy aspects of it may be above my pay grade.

GARRICK: Okay. Andy, then Ali, and then David.

KADAK: Kadak, Board.

Based on your comments, do you have any feel, and your knowledge of the licensing of Yucca Mountain, do you have any feel for if one were to apply this kind of analysis, in terms of really understanding impacts on humans, what do you think the numbers would do relative to public health risk?

RYAN: I guess when you accept the idea that all the doses whoever has calculated them to date are very very low, my guess, and it's strictly a guess, because we haven't done all the analyses to firm that guess up, is that I don't think impacts would end up changing very much relative to any kind of comparison to a standard. But, I think it's a weakness to not have that analysis in hand.

KADAK: Let me try to interpret what you just said. Are you saying that if you apply a realistic model, as we're trying to apply realism in the performance of the natural barriers, and other things, and if you consider the fact that humans don't breathe like, you know, through their mouth and through their nose, and other things relative to the leachate, as you call it, the doses--and I'm assuming this is what is assumed in the DOE analysis--that the dose numbers could be significantly lower?

RYAN: Well, I'm by no means an expert on DOE's analysis, so I can't speak to it. Let me speak in a more general way, if I may.

KADAK: All right.

RYAN: If you accept reference dose factors, you could be over estimating or under estimating dose in whatever setting you might want to apply it. My simple point is I think we need to understand it in the setting in which we apply it, what error, uncertainty, conservatism, non-conservatism, might be implied. And, I think it's a little bit of all of those.

GARRICK: Ali?

MOSLEH: In your opinion, these uncertainties are mostly uncertainties due to lack of understanding of the models and the processes, or is it more like the parameters and the data that you need to inform those models?

RYAN: I think our understanding of the basic physiology is certainly robust. I think it's often a combination of a lack of how we can assess for a specific exposure, a specific physical form, a specific chemical form, and then make a dose calculation on that basis. If you look at any occupational internal exposure, we end up with calculating for the purposes of reference using reference models. But, then, we very specifically say all right, we're going to do bioassay measurements, whole body counting, body fluids analysis, whatever it might be, and we determine a specific model for that individual.

A simple case is tritium. The reference excretion half-time is 12 days. Well, what happens if it's eight days? The dose goes up by that relative ratio, or down, rather. If it's 16 days, it goes up. The point is without knowing what that individual's biological retention half-time is for tritiated water, you really are estimating the dose. You're not determining it as precisely as one could. So, I think to answer your question, it's those metabolic parameters that describe, you know, that come from the physiology for an individual, and then the specifics of the physics and chemistry of what's inhaled or ingested.

GARRICK: David?

DUQUETTE: Duquette, Board.

This is probably more a comment than a question, but could you go to Slide Number 4? I spent most of my career looking at the first two as a corrosion person, because normally, you don't get called in unless you already know what the consequences are, and usually, they're pretty dire. The first two, of course, are the things that PRA is somewhat based on.

RYAN: Yes.

DUQUETTE: In this particular project, there's been an attempt by the project to eliminate corrosion as a possible source of radionuclides, because the probability of it occurring are not very high. At least two members of this Board disagree with that particular conclusion. But, the fact of the matter is that I think you can take the wrong path if you don't take a look at what the consequences are. And, I think the first two address PRA, and I personally think that quite often, when people assess probabilities, they do it incorrectly because they don't always understand the mechanisms of what's going to happen.

RYAN: Fair enough.

GARRICK: Any other questions? Any questions from the staff?

(No response.)

GARRICK: Thank you very much.

RYAN: Thank you, Mr. Chairman. I appreciate the time to be with you.

GARRICK: Our next speaker, Tim? Tim McCartin from the NRC. You might tell us what your job is there, Tim.

MC CARTIN: I work for the Nuclear Regulatory Commission in the Division of High Level Waste Repository Safety. And, in that capacity, I really have two roles. I'm the Senior Advisor for performance assessment, but I also have the lead role for developing the regulations for Yucca Mountain.

And, today, in truth in advertising, although the title says Implementation of a Dose Standard after 10,000 Years, if someone was expecting I was going to provide the entire context of our implementing the dose standard, I apologize. I am going to focus on two aspects of our implementation in the time I'm allotted. And, those two aspects are, one will be the kinds of quantitative analyses, and our understanding to date with respect to implementing a peak dose standard with respect to dosimetry and performance assessment. The other is, and I'll get into a little more detail on that, is our basis for specifying how to represent climate change.

And, you will notice Dr. Gordon Wittmeyer, who my colleague, is much more informed with respect to the climate change, and if I get questions on that, I will most likely defer the hard ones to Gordon and take the easy ones for myself.

Next slide? I'll give a slight introduction on the purpose of proposed Part 63 to set up those other discussions that will get into our understanding from an inventory standpoint, from a dosimetry standpoint, and then for the most part, talk about the representation of climate change.

In terms of our regulation, EPA has proposed a standard for a peak dose limit beyond 10,000 years. NRC is implementing that by adopting that standard. There are some other aspects with respect to how the performance assessment is to be done, et cetera. And, I'm not going to get into the details of that, but the dose limit is probably the most noticed aspect of the standard. But, there are other aspects to that.

Secondly, EPA did specify to update the dosimetry with respect to the public dose assessment. For people who don't know, there's a slight distinction between the EPA standards and NRC regulations. EPA's standards are for the public exposures. NRC regulations are for--we adopt that public exposure limit, but we also specify our own limits for worker exposures. And, so, when EPA specified use for current dosimetry for public dose estimates, we have also taken the initiative to, rather than having two different types of dose calculations, that use that same newer dosimetry for the workers.

And, lastly, the specification of climate change at Yucca Mountain after 10,000 years, EPA specifically identified the NRC to come up with a value for representing that climate change after 10,000 years.

Next. Okay, and, with that, I just want to go into first, our perspective from an inventory standpoint. And, this is just looking at the inventory going out in time, and you can see just using the 1,000 year inventory, and be aware there certainly is a fair amount of very short-lived radionuclides that have decayed by 1,000 years, but using 1,000 year inventory in terms of curies as a metric, you can see at 10,000 years, it's approximately a quarter of what it was, going down to 100,000 years, it's approximately 2 per cent at 100,000 years.

GARRICK: This is strictly based on decay?

MC CARTIN: Strictly decay. Just based on curies.

GARRICK: Are you going to show us a similar curve based on performance?

MC CARTIN: I have some information on performance. I don't know if it will be as much as you would like, but I do have some, and happy to talk to the basis for our calculations.

GARRICK: That's the one we're really interested in.

MC CARTIN: Okay. And, if one went out to a million years, my recollection is that the 2 per cent drops by about an order of magnitude. It would be about .2 per cent if you carried it out to a million years.

Next slide? With respect to those time frames, one of the important radionuclides, one of the things that the Commission is interested in, and the Staff is interested in, is we have a perspective in the 10,000 years. Now, if you went out to a million years, how does your understanding of what nuclides are important change over that million year time period. And, you can see the thousand years were dominated by americium and plutonium. Going out to 10,000 years, we still have Plutonium 239 getting a little more important, as the americium and the Plutonium 240 decays away. Going further, if you go out, you can see you still have Plutonium dominating at 100,000 years.

I would point to one of the interesting things to the far right of that 100,000 year curve, you have neptunium starting to show up as a contributor of that. Clearly at 1,000 years, in terms of curie amount, it is not a significant amount of the inventory.

Next slide? Carrying it out further, you can see there is Thorium 230 has some significant in growth, and is the dominant radionuclide at 300,000 years with respect to curies. And, if you go all the way out to a million years, you can see neptunium.

One thing I would like to point out in our analyses over the last two decades, we have always had neptunium as a significant contributor, and a significant nuclide to look at, even though it's only recently, the idea of a million year calculation has come into vogue. The concept, you know, you don't need a million year requirement to know that neptunium was an important radionuclide, and you can see at the million years, it is the single largest inventory. But, we had that with our 10,000 year calculation. We were still interested in neptunium.

Next slide? That's some perspective at least on how the inventory changes over that million year period. Now, as I stated, the EPA standard has suggested using newer dosimetry, and if I look at Federal Guidance Report Number 11, which is September of 1988, that is the current basis for doing dose calculations. What EPA is proposing is really moving to Federal Guidance Report 13, which is September of 1999, newer dosimetry.

And, the next slide will show the changes for some of the key radionuclides. And, you can see for some, in terms of the ingestion dose conversion factor, for some of the nuclides, they increase, most notably the first two, technetium and iodine increase by about a factor of 2. Those typically are nuclides that show up in 10,000 year assessments, because they're highly mobile, relatively soluble.

Neptunium dropped a little bit more than an order of magnitude. Thorium 230 increased some, and the americium, plutonium and uranium dropped somewhat.

Next slide? In terms of the relative dose conversion factor, if I look at FGR 11, the older dosimetry, you can see neptunium was the most dominant radionuclide from a dose conversion factor. Technetium may look like there's nothing there, but technetium, although there's a fair amount of technetium in the repository, very long-lived, it is an extremely low dose conversion factor, and there is a value there, it just doesn't show up on this scale it's so small.

Next slide? Going to Federal Guidance Report 13, the newer dosimetry, you can see sort of what's happened with neptunium. Everything else has sort of come down to similar contributions. Neptunium is no longer the dominant radionuclide, and other nuclides are comparable to the dose conversion factor for neptunium. And, that's in terms of, as you will see for our analyses, we were interested in prior to the newer dosimetry, neptunium has always been the dominant radionuclide in our dose calculations. And, so, we've implemented new dosimetry and done some calculations.

Next slide? Everyone is always interested in dose estimates. I will say I debated long with myself on this one in terms of what's the best title to give this slide. And, I chose the word "illustrative." I want to say that we are in the process of revising our models. It is a work in progress. This curve is for us, we were using it primarily to see with the newer dosimetry--

GARRICK: But, it is a Yucca Mountain illustrative?

MC CARTIN: Oh, absolutely. Yes. We're in the middle of revising our models, and I wouldn't want to say this is where we will end up with our new model. But, we were interested in would the newer dosimetry change the relative importance of neptunium, and you can see the red line is neptunium, it still dominates the overall dose curve in our analyses. That peak is, I think, somewhere on the order of 150,000 years, and around 9 millirem, and it tapers off.

GARRICK: You've already alluded to some of the reasons why maybe this is a different curve than we've seen in the past. Are you going to expand on that a little? This is very different in both the time of peak and in the magnitude.

MC CARTIN: Well, I can give you some perspectives on why it looks the way it is.

GARRICK: Okay.

MC CARTIN: And, what changes could alter things in the future as we proceed. Number one, we have a waste package that fails primarily between the period of 40,000 to 80,000 years. Those models continue to be improved, enhanced, based on new data, new information, and I can't say what that change might end up, but we are revising the corrosion models. That's primarily why you see the peak around 150,000 years, it's related to a waste package that on average, is failing at around 60,000 years, and that's the transport time to get neptunium to the accessible environment, for the most part.

KADAK: The failure mode is what did you say, corrosion?

MC CARTIN: Corrosion, yes.

KADAK: Localized?

MC CARTIN: General.

KADAK: General?

MC CARTIN: Yes.

HORNBERGER: This one indicates that neptunium is transport limited and not solubility limited?

MC CARTIN: Neptunium can oscillate between solubility limited and release rate limited, depending on the particular selection of parameters and the time period. Time period is affected by how much water is infiltrating at that particular time. But, also, for our calculations, we don't use a strict release rate. We have a calculation that it goes through, but on average, our release rates from the spent fuel are on the order of 10^{-4} to 10^{-5}, depending on where you are in that curve. When you tend to be at the higher end, and you have a lot of water, there could be a solubility limit. When you're at the lower end, and littler water, you won't have a solubility limit. So, it's not strictly controlled by solubility universally, but it can be, depending on the particular selection parameters.

And, we continue to look at the release rate. I would say based upon previous TSPAs, that is a large difference between ourselves and the DOE models, that DOE tends to have, my understanding, and I will say I could be wrong, but my understanding, tends to have a much higher release rate, and they take credit for cladding. We, on the other hand, for the source term, we have a much lower release rate, but we take no credit for cladding. And, so, that can be a significant difference, depending on particular parameters. But, that's one reason you're seeing those doses, and this is using the ICRP, the new dosimetry, ICRP, as implemented in Federal Guidance Report 13, that an order of magnitude higher would take a neptunium dose of 8 millirem, and make it 80.

There are other differences. One thing we do not, in this particular calculation, we do not have Carbon 14 in our calculation. We, due to isotopic dilution reasons, we do not transport Carbon 14. We are relooking at those assumptions and analyses to further confirm that, partly because in some of the DOE analyses, Carbon 14 is not a dominate contributor, but is a non-trivial contributor dose. But, we don't have Carbon 14. We think isotopic dilution would render it rather small. But, we're reconfirming that.

This particular calculation also does not have colloids. Depending on your assumptions for the generational colloids, and for the transport of colloids, that could be different. We have done analyses in the past where we felt the colloidal contribution was limited. The next version of what we're working on today is looking at an appropriate way to look at the generational colloids within the waste package, given the waste package materials and the internals, and then also the filtration and transport of colloids in the far field. And, that's something that could change the results to date. Those don't result in that.

Other than that, you know, we continue to, as information comes in, be it the solubility of neptunium, be it the retardation factors in the unsaturated/saturated zone, we continue to update the model, and traditionally, we have modified the parameter ranges somewhat based on new information. I don't know if I would expect big differences there, but that work still is being done.

PETROSKI: Could you clarify? What does that low curve include?

MC CARTIN: Everything. All the radionuclides in our inventory. There's approximately 21 radionuclides.

DUQUETTE: You assume that all of the packages fail at the same time?

MC CARTIN: No. There is a distribution of packages, and, like I said, generally, the waste package failure times are varying from 40,000 years to 80,000 years. We have eight sub-areas, eight regions of the repository where we represent corrosion of the waste package. It partly depends on the temperature in terms of when they fail, however, within a sub-area, one of those eight sub-areas, when we use a representative package. So, when a representative package fails, they are all assumed to fail.

KADAK: How sophisticated is this model relative to what DOE has prepared? And, is this the best estimate? I mean, are you just making gross assumptions trying to just get a feel for what's going on, or are you trying to really predict peak dose?

MC CARTIN: I would like to think we're trying to give our best shot at representing the performance. However, there is--and, I think this is a strength of the system--there are at least two independent groups, ourselves and DOE, EPRI also is out there doing calculations, but, looking at ways to represent this. There are some areas where I believe we have a more sophisticated approach. There are some areas where DOE has a more sophisticated approach. And, it would take, I mean, in the limited time I have, I really would be pressed to go into all the examples. But, certainly, I'm willing to talk to you off line somewhat.

But, it's a variation, and it really, I mean, probably the best example I can give, in my own opinion, is the source strength. And, we looked at the degradation rates of spent fuel, and cladding. We felt that there was a better basis for the degradation of spent fuel for very long-term behavior than credit for cladding, so we did not look a lot at credit for cladding. We spent most of our time looking at the basis for the release rates from the spent fuel. You know, whereas, DOE is taking at early times, certainly a substantial amount of credit for cladding, and has a very fast release rate from the spent fuel. And, I think that should be looked on as a strength to the system. Different analysts look at the system and see where they are more comfortable ascribing performance to, and go down one path.

GARRICK: Tim, I knew this slide would create a great deal of interest, and I applaud you for showing it. We are very grateful to you for doing that. But, I think we'd better go on. I will give Ron--

LATANISION: Mr. Chairman, I do have to ask this question. My friends on the other side of the table here just provoked me, so I will do so.

I just want to make sure I'm clear on the point of the mode of corrosion failure that you're looking at. My understanding is that if you look at the data that's been generated, and if you assume that the waste packaged mode of degradation is by uniform corrosion, by general corrosion, and the system is operating in a passive regime, then this system ought to have about a million years of life. So, I'm wondering how you then translate that into a 40,000 or 50,000 year failure, if it's not something like localized corrosion.

MC CARTIN: Certainly. That part of the model is being revised currently, and there is certainly with respect to the types of chemistries that we'll be seeing, I would say that is a correct assumption, that the container lifetimes from uniform corrosion are going longer. But, you know, it's still, we're in the middle of that.

LATANISION: But, just to follow that up, if that were the case, then presumably those curves are going to move around quite a lot. Am I correct? I mean, that peak that you're showing--

MC CARTIN: It could move out further in time.

LATANISION: Right.

MC CARTIN: But, it wouldn't be a spread, say, from 40,000 years to a million years.

LATANISION: No, I know. I just meant that it would move from--

MC CARTIN: Oh, absolutely.

LATANISION: It could be significant.

MC CARTIN: Yes. However, I will say we have looked, just the way of calculations, what if I just move that out 500,000 years, and have all the waste packages fail within, say, 500,000 to 600,000 years. The dose curve goes down some. It doesn't really drop that much, because it's dominated, in our calculations, dominated by neptunium that essentially has a very long half-life, and is still pretty much there.

GARRICK: Are you saying that--I don't want to prolong this unduly--are you saying that the impact is principally on the time of peak dose, but you don't expect a major change with respect to the magnitude of the dose?

MC CARTIN: To date, that is correct.

GARRICK: Okay.

MC CARTIN: Given all the other assumptions, as other things change, you know, I will say I don't want to--you're getting a snap shot. It's not a final, you haven't gotten to the end of the movie yet. So, it's just one part of it. But, yes, I mean, I guess I wasn't overly surprised by that.

GARRICK: Yes. Carry on.

MC CARTIN: Yes, next slide, please.

Getting to the, as I said, the EPA, they suggested that the assessment for climate change could be limited to the effects of increased water flow through the repository. And, they said the nature and degree of climate change could be represented by constant conditions after 10,000 years.

Next slide. And, in that, they said NRC should specify in regulation the values to be used to represent that constant climate after 10,000 years.

Next. And, in looking at that, clearly, temperature and rainfall are the most straightforward things when you think of climate change. But, looking at the problem, really, the part of that that affects the performance of a Yucca Mountain repository is deep percolation, how much water gets to the repository horizon. And, so, rather than specifying a temperature and a rainfall, we felt a more direct way was to go directly to deep percolation, and have a representation of that.

Certainly, we recognize that that deep percolation rate, whatever it is, is controlled by a variety of processes, such as the precipitation, the temperature, evaporation, plant transpiration, those things have to be considered in getting to the deep percolation, but we thought that it would be a more direct way to represent what should be used for the climate change, rather than, say, going to the first factors of temperature and rainfall.

Next slide. In estimating that deep percolation, clearly, one has to look at some range for the mean annual precipitation over that million years. There was an understanding in the literature we looked at that the glacial-transition/monsoon states tended to dominate the long-term climate states. These were wetter conditions. On average, we would expect it to be wetter during the next million years than it is today.

Next. A harder thing is what's arranged for the fraction of precipitation that ends up as deep percolation. And, I'll talk about those in a little bit more detail.

Next slide. In terms of precipitation, looking at analog sites based on vegetation related to packrat middens. There was analog sites that the estimate for the rainfall was on the order of 266 to 321 millimeters per year, representative of the last glacial maximum. I've been criticized before for this for showing the three significant figures to that, and how could I possibly do that. I will say that was the number that was reported in the reference that we used. And, so, I felt we should not change the numbers in the report, just reporting what was used, but we do recognize the limitations in any type of calculation of this nature to three significant figures. But, there is a rainfall rate.

Next slide. In terms of the fraction of precipitation that results in deep percolation, there, we had to draw upon calculations. Some of the models in our TPA code that account for and support the values we have in the performance assessment that consider the temperature, soil depth, evapotranspiration, and what we found for the two ranges of precipitations, 250 to 420 millimeters per year, that approximately 5 to 20 per cent of that precipitation ended up as deep percolation. And, so, that's our fraction for the amount of rainfall that ends up at the repository horizon, giving the modeling that takes into account such things as the evapotranspiration, which is highly dependent on the temperature.

KADAK: So, you used 20 per cent, so that's fine.

MC CARTIN: Well, here's what we did. And, so, what we took was we took--we had that range for precipitation of 266 to 321, and then we had a range of 5 per cent to 20 per cent for the fraction that would remain, and we got a range to be used for deep percolation by just doing a straight arithmetic multiplication. And, so, we ended up with a range for that long-term average of 13 to 64.

Now, some might ask, well, if you have to specify an average, why don't you just give a single value. And the reason we didn't was that we think it's useful that you have a range that in the performance assessment, then you're going to get an understanding, well, how important is it when you get towards the higher end to the lower end. And, it's not a huge variation, but you will be able to, in the results, see some sensitivity to what that specification was. And, so, we thought that was valuable information that we think would assist any safety decision the NRC might make in the future.

In terms of the distribution for that range, we looked at it that really deep percolation is a result of a lot of multiplicative processes. So, that would suggest a logarithmic distribution. And, when we look at those numbers, there's limited basis for favoring either end--well, in actuality, really, for favoring any number within that range. And, so, we said a log uniform distribution would seem to not be unreasonable for a range.

Next slide. With that, you end up with if you did that range, the mean value from that range and that distribution is a value of 32 millimeters per year. That's approximately six times larger than the infiltration rate, deep percolation rate estimated by some for the site today, although that's assuming the estimate is on the order of 5 millimeters per year. There are ranges for that. So, we do end up with a value that's deeper, that is more infiltration to the repository, we believe, and in a qualitative sense that if you are assuming that a lot of the time it's going to be wetter and cooler, and certainly the cooler conditions really enhance the infiltration, that it was a reasonable proposal to go out and seek public comment.

Last slide. And, with that, where is the process today? The EPA comment period ended on November 21st. The NRC comment period ended on December 7th. We did get comments on the approach for estimating the deep percolation, as well as other items. We are in the process of considering those comments. We would expect to finalize our regulation shortly after EPA finalizes theirs. So, we will not be finalizing--we can't finalize ours until we see what the final EPA standard is.

And, with that--sorry about that mid-talk delay, but--

GARRICK: It was a highlight. Okay, David, Thure, and then Bill?

DUQUETTE: Duquette, Board.

Given that NRC is sort of a junkyard dog on this whole thing, and as you're the regulator and have to pass through whatever DOE gives you, and at the present time, at least some of the numbers that I seem to recall from the DOE data are larger than your dose numbers, do you think the NRC as a regulating body would lean towards accepting, assuming that the DOE standard meets the EPA standard, how do you think you're going to compare your data with their data?

MC CARTIN: Well, and I probably should have qualified this, the calculations we've done for the past 20 years or so and the performance assessment tools, and techniques, we have are done in a way to assist our review of the DOE license application. And, the fact that we have a particular number doesn't really help DOE one way or another. It's a way for us to get insights. We obviously understand our code very well, and we can do changes to it, and things of that nature.

The bottom line is we need to understand the assumptions and the bases for DOE's performance assessment, and look at their defense for what they have proposed. And, there are cases where I think we have an approach in our PA code that helps us better understand what some of the assumptions might mean, and allow us to ask more informed questions of the Department. But, basically, it's really the Department's performance assessment.

As I said, an example, in our code, we currently take no credit for cladding. Well, we do have a parameter where we can do--take credit for cladding. We haven't invoked it. But, that's an example of things we're aware of, some of the things the Department has in their code. This is a way for us to better understand the problem. But, ultimately, it is based on the Department's. I don't know if that's helpful.

GARRICK: Thure?

CERLING: Cerling, Board.

Two questions. I didn't ask any questions on Slide 11, so I get to now.

GARRICK: So, look out.

CERLING: One of the things that's of particular interest to me is why the Neptunium 237 goes down instead of perhaps even continuing to rise, or reaching a plateau, and I was just wondering why that is in your model?

MC CARTIN: Well, part of that is, as I said, you know, we're on a 10^{-4}, 10^{-5} release rate, so the spread of that--the larger part of the release, you're looking at around 100,000 years, so neptunium is depleted. I mean, if you think of a release rate on the order of 10^{-5}, it's gone. And, there's some lingering--some of the parts that you're seeing there is uncertain--

CERLING: Well, then does this mean that all of the waste packages have failed by 80,000 years, or what?

MC CARTIN: Oh, yes. In our performance assessment--

CERLING: So 100 per cent have failed between 40,000 and 80,000?

MC CARTIN: Yes.

CERLING: Okay. The second question that I had had to do with Slides 9 and 10, which were the difference in the dose conversion factors. And, everything is normalized to 100 per cent of the maximum, and, so, could you just help us along on what are the--what happened to the absolute values?

MC CARTIN: Well, I mean, I could put them in. I don't know if it would be--I mean, where you're at about the 50 per cent point for neptunium versus Plutonium 239, it's a factor of 2 difference, I mean, in the absolute values.

CERLING: No, I know that. But, if you compare this to Slide 9, Neptunium 237 I think has 100 per cent value, so we don't really know--if plutonium stays the same, and suddenly is now 100 per cent, then we can work that out. So, what has happened--

MC CARTIN: Go to the previous slide. This is the slide that is comparing, when I switched from FGR 11, the older dosimetry, to FGR 13.

CERLING: Right. Well, there's still no absolute values.

MC CARTIN: Well, neptunium dropped by a little more than a factor of 10. So, it would be ten times less--the absolute value would be ten times less. The dose would be ten times less for a unit intake of neptunium. Likewise, with iodine and technetium, it's approximately twice. Those absolute values of the dose--

CERLING: No, I see. I guess with an Excel spreadsheet, we could figure this out, but it's kind of hard to put it all together with the three slides. I guess one could put it together.

MC CARTIN: Well, the only reason for the other slides was this showed you neptunium dropped a lot. But, if you go to the next slide, one of the things that's helpful, in my mind, was that neptunium was a very large contributor. Technetium might change some, but you can see overall, that technetium dose co-efficient is extremely small to begin with. So, a factor of 2 change in technetium is taking a very small number and making it a little bit larger. It's still a very small number. That was the only perspective that these slides were trying to provide.

GARRICK: Let me go to Bill, Bill Murphy.

MURPHY: Bill Murphy, consultant.

My first question was Thure's first question, why did neptunium drop between 100,000 and 200,000 years. And, I think I understand the answer. It provokes, in my mind, another, where does it go? Have you devoted attention to considering where this neptunium that's been released and transported out of the system ultimately accumulates, and is that a potential hazard?

MC CARTIN: In terms of if it wasn't withdrawn from the aquifer and it stayed in the aquifer?

MURPHY: Or if it became concentrated at a discharge point, for example.

MC CARTIN: Oh, boy, about five or ten years ago, we did look at what if it was never taken out, in which case generally, one might say, well, there's no dose if it remains in the aquifer. But, if it continued onward, and recognizing there could be some spring deposits, where there could be some seepage, and at the time we looked at it, there was--I mean, obviously, this is going much further in time because the potential delay mechanisms are extremely long because you're in the alluvium, which tends to be fairly absorptive. But, you get to a point where if you had spring deposits, you have more of a inhalation dose from things that get up in the air and the dust and things, and it doesn't tend to be nearly as much in our calculations than the current assumptions, where you're drinking two liters of water a day of contaminated water, in addition to irrigating crops and consuming crops, that the inhalation dose was significantly less.

MURPHY: Thank you. I have one other question on a different subject. You said that the nature and degree of climate change can be reasonably represented by constant conditions after 10,000 years. And, I wonder about the affects of transients in flow systems on dose effects. For example, in environmental systems, the first flush of the season is the big effect. And, in fact, a variable climatic regime where fuel oxidizes and material become labile for 10,000 or 50,000 years, and then there's a flush, could lead to substantially, hypothetically substantially greater peak doses than a continuous process even at a higher flow rate.

MC CARTIN: We have gotten comment in terms of the issue of, gee, maybe a variable climate still retaining that average would be worse, if you will, quote, unquote, worse than say a constant. And, it's something we're looking at.

There are a number of assumptions that I think if you're going to do that performance calculation, that you need to think about with respect to how the flushing occurs. There's a part of that that the timing is probabilistic of when those, say, a large climate change might occur is not known, so that would be a variable. There's other things. It certainly depends on the timing and extent of waste package degradation in terms of when did they fail relative to that time. And, so, there's a lot of subtleties to the calculation that we are looking at. There are other things. I know, once again, my understanding of the DOE analysis that I will say, I won't say I know exactly this is what happened, but I know when they did the water table rise for climate change, that was done instantaneously. And, if you raise the water table up instantaneously 100 meters, and now you have all the radionuclides that are in 100 meters of the unsaturated zone are now instantly available for transport in the saturated zone, that certainly will create a large pulse.

The question one has to step back and say, well, this instantaneous rise of the water table 100 meters, is that actually the way it's going to occur. And, so, there's a lot of things I would say it's a--you're right, there are subtleties to how climate changes, but one needs to think through the problem. We're in the process of doing that, because we certainly got comments to that effect in this approach.

GARRICK: Okay, Ali, Leon, Howard, and then David Diodato.

MOSLEH: Mosleh, Board.

It's back to your peak slide. I understand these are illustrative, but are they illustrative mean values, or something else?

MC CARTIN: Yes, this was a mean value calculation. It was a probabilistic calculation, and I'm representing the mean dose curve.

MOSLEH: And, how do you think the median curves would fall?

MC CARTIN: I debated, because I knew someone was going to ask that question about whether to add other percentiles, including the median. Given the nature of the calculation and our continued revisions to the code, I felt comfortable in presenting the mean curve. The median, in general, will be less, but how much less, I don't know. But, in general, it's less.

GARRICK: Leon?

REITER: Leon Reiter, Board.

Tim, is it correct to assume that disruptive events are not included in this?

MC CARTIN: Yes. I should have said that. Yes, this does not include igneous activity.

REITER: Could you give us--or seismic activity, or are seismic activities included?

MC CARTIN: There was some inclusion of seismic activity, but it did not have a significant effect in these analyses. That is an area of our model that we are looking at.

REITER: Could you give us any feeling as to if you included igneous activity, and other parts of seismic activity, how might this curve be affected? Which parts might be affected?

MC CARTIN: Well, a failed waste package is a failed waste package. So, the fact that we don't, if we were taking credit for cladding, and seismicity was affecting the cladding, it would have potentially a larger effect. In our case, we are not taking credit for cladding. In general, I would not expect a dramatic difference, but I will say, you know, all of this please take with a grain of salt, that the way to project things forward in time is something that with respect to seismicity, and these other processes, we're looking at. But, you know, like I said, all the waste containers were failed at around 60,000 years.

REITER: You don't expect igneous to have much effect?

MC CARTIN: Pardon?

REITER: What about igneous? You don't expect igneous--

MC CARTIN: I personally would be somewhat surprised if igneous was changed dramatically, for the simple reason the one aspect of the repository system when represented, igneous activity occurring early on, early on I will define in the first 1,000 years, has the effect of releasing short-lived radionuclides that can't make it any other way except this release. And, as you go out in time, our 1,000 year doses are dominated by the short-lived radionuclides. At 10,000 years, they're not there anymore. And, so, the things that were causing the larger doses were the short-lived radionuclides that dot out to an air pathway.

So, assuming, once again, we continue to revise and understand, but given, if you look at that inventory slide, you can see Americium 241 and Plutonium, one of the plutoniums, maybe 239, that were very large sources of the inventory at 1,000 years, at 10,000 years, they're essentially gone.

REITER: But, if you assume an intrusive dose, you might have releases later on.

MC CARTIN: Oh, I was talking about extrusive. Certainly you could. Once again, I think this curve is representing all waste packages fail, with the probability of one, essentially. The igneous activity, it might fail some waste packages at some later time, but it has a lower probability. In the groundwater pathway, we would still expect to see neptunium dominate the dose curve. But, once again, those questions need to be evaluated, and I'm giving you my, at least, sense of the calculation.

GARRICK: One of the reasons you don't think it would have much of an effect is things like the probability of an intrusive event or an extrusive event is very small?

MC CARTIN: Yes.

GARRICK: Okay. Let's hear from Howard, and then David Diodato.

ARNOLD: Arnold, Board.

I'm interested in the cladding. Is the fact that, you're assuming no credit, is that based on good analysis, or are you working to see whether it's due some credit?

MC CARTIN: Well, I mean, I'm struggling with the word "analysis." I would say there isn't a quantitative calculation that we've done, but it was a recognition that if you had some slight perforations, or pits in the cladding that allowed oxidation, unzipping of the cladding relatively quickly, the cladding is extremely thin, that over thousands of years, some seismic events, other things that we felt that we would prefer to, as Dr. Garrick would say, we turned our microscope up on the release rate from the spent fuel, and we recognized that possibly you can come up with some basis for that, we felt, for our effort, we want to focus more on the looking at issues with respect to the degradation rate of the spent fuel. That's not to say that, you know, the Department will have a basis. They will review it, but like I said, we have not included it in our calculation.

As with every performance assessment, and I would say--one thing that I would like to say, I don't think anyone tries to be overly conservative in the performance assessment. You're always trying to do what you think is a reasonable estimate of performance. But, you generally have limited resources, and, so, you look at areas where I think I'll improve that part, and this part, I'll let go. And, in our case, we felt that the cladding credit was not as important in our mind to the degradation of spent fuel. But, you have many of those questions of where do you devote your resources in developing a model.

GARRICK: Okay, David?

DIODATO: This is Dave Diodato, Staff. I'll be brief.

Tim, I appreciate you doing this presentation. On Slide 16, I'm going to focus just on the precipitation question, with the assumption of long-term average climate specification that EPA gave you the opportunity to decide to choose to put in the regulation.

This 266 to 321 millimeter per year number, when you gave this presentation at the public meeting of the Advisory Committee on Nuclear Waste at the NRC, some questions came up about that because that was based on the USGS open file report, where the analyst found the regression coefficient in the analysis was only about .75, and that was based on analog sites and other information. So, there were questions raised about that in that meeting, and I just wonder since that time, you have some confidence in these numbers so far, but are there any new analyses to date that have increased your confidence in this range of precipitation?

MC CARTIN: Well, we got comments on our specification, and we are in the process of looking at it from a variety of different approaches. With that particular range, we don't have anything right today. I mean, I will say part of it that I was somewhat, you know, currently, the precipitation at Yucca Mountain is on the order of 125 millimeters per year. If you double it, you get 250. You're sort of at the low end of that range, and, you know, doubling of rainfall, we are a little higher. I know the ACNW expressed an interest that it's too high. We are looking at that.

The only thing that I've done, as much out of curiosity to see what it was, I took in our performance assessment, we use 100,000 year maloncovich cycle to represent climate change over a million years, where we did a gradual up and down. Using average conditions there for that cycle and rainfall, I just did a time average over, you know, accounting for the wet and dry periods, just over time, and got a value, and I know people will think I cooked the books on this one, but it ended up being 33 millimeters per year, on average, accounting for the cycle. And, our average came out to 32, and that's maybe pure chance.

I also went to, I believe it was the DOE site recommendation where they had an alternative model for climate change, where I think they had a 400,000 year cycle, with these spikes. And, I did the same thing, did a time average of that representation of a million year period, and ended up with 26 millimeters per year. And, actually, I was stunned at how close those were. We continue to look at additional items, different ways to represent climate, and, you know, to date, that's pretty much what we have. There will be a report in the not too distant future that the Center could be publishing, looking at some other alternatives.

DIODATO: Well, I just am a little confused because, if I heard you right, you're arguing for numbers that are an order of magnitude lower than these numbers.

MC CARTIN: I'm sorry. The 26 and 33 were for deep percolation. That is precipitation.

DIODATO: Okay.

MC CARTIN: Using that range, our ranges, on average, when you translate that on the next slide to--but when you translate that to the deep percolation, you end up with a range of 13 to 64. That is, on average, 32. So, that precipitation amount gets you a--yeah, that would be extremely large if you had 266 millimeters of deep percolation. But, that's the rainfall.

DIODATO: Yes, and that's what we're looking for, is the technical basis on the rainfall, because 190 millimeters per year at present; correct?

MC CARTIN: Estimate is around 125. But, you know, no one has a--it's going to be variable. I mean, that's why we were more comfortable with a range. You can't really, a single value is pretty hard to defend.

DIODATO: But, still, no new numbers that would increase your confidence in that 266 to 321?

MC CARTIN: Not yet. And, we're just in--the comment period hasn't been closed that long. You know, we'll have more to say when we finalize the regulation.

DIODATO: Thank you. I appreciate that.

GARRICK: I wanted to cut it off at this point for our break, but I have a colleague here that has a pressing question. And, if he can keep it brief, we will handle it.

KADAK: Kadak. It appears that you've taken much more credit for the natural barrier in terms of time to get to the environment than DOE has. And, again, this gets back to my question about modeling sophistication. What is it that you're doing that's different than what DOE is doing?

MC CARTIN: In terms of the natural system, very briefly, and I'll break it into two components, the unsaturated zone and the saturated zone. In terms of the unsaturated zone, we probably take less credit than the Department in that we have on the order of 50 per cent of the repository footprint lies above Calico Hills vitric unit, where there's porous flow sorption. The other 50 per cent is fracture flow. And, so, there is essentially very little--well, in our model, there is no retardation, so for half of the footprint, we have fairly rapid transport. The Department has slower rates of transport in the unsaturated zone than we do, based on previous models. I do not know what current models is. It's primarily based on the FEIS model, final environmental impact statement model of the Department.

They have traditionally shown more there, more matrix diffusion, et cetera. We take no credit for matrix diffusion in the unsaturated zone.

In the saturated zone, I believe the primary difference is we have a longer saturated zone flow path in alluvium, where most of the retardation is. With respect to retardation factors, we're really, I don't think we're that different, but I don't believe there's a dramatic difference, but there could be some small differences, but I think it's the distance in the alluvium. That would be the primary ones that I'm aware of.

GARRICK: Okay, thank you very much, Tim.

We'll now take a 15 minute break.

(Whereupon, a brief recess was taken.)

GARRICK: Jens Birkholzer, go ahead.

BIRKHOLZER: Okay, thanks for the invitation to speak here. I feel honored, and I hope I can give you the information that you're seeking.

My part of the talk, it's a split talk, it's on the mass of what is seeping into drifts. The second part is Dr. Hardin's topic, and he will talk about seepage and water coming out of drifts.

Next slide, please. This is a brief outline of my talk. It will first present to you the technical basis and key processes affecting seepage into drifts. We'll give a brief introduction on how we propose that TSPA should calculate seepage in the performance assessment. We'll then go into detail on how we predict seepage, both at ambient conditions, long-term conditions, and the effects that the thermal period will have on seepage, give you a basis for technical assessments, some assumptions, some uncertainties. And, finally, discuss some seepage calculation results. Those are results of the probabilistic analysis for the 10,000 year or 20,000 year time frame, if you want, an analysis that was done in Berkeley, which is sort of similar to what TSPA is expected to do, but not identical, there are some simplifications.

Next slide. First, a few definitions. What we define as seepage is the dripping of liquid water, so we're not talking vapor, we're talking liquid water from the formation into an underground opening, because of the forces that amount of water is much less than what is percolating down to the tunnel drifts.

When we talk about the seepage rate, we talk about the mass of water seeping per time, and we give that rate for a drift section that contains one waste package. So, you kind of know what the seepage rate is that's going to be per waste package.

Seepage percentage is the ratio of seepage rate divided by the flux getting to a tunnel drift. In other words if you have, say, a seepage percentage of 5, that would mean that 95 per cent of the water that gets down to a drift will actually be diverted around it and not seep.

Finally, the seepage fraction is the relative number of waste packages that could be affected by seepage.

Next slide. This slide gives you a brief introduction to the processes and factors that we believe are important for ambient seepage. First of all, starting on the right side here, we really have a small scale, or drift scale problem to solve when we look at seepage. The main parameters, sort of in theory, are the amount of water getting to a drift, the shape of such an opening, the capillarity of the formation, and also the permeability that would allow water to divert sideways.

Now, in practice, there are more issues that we need to look at, flows and the fraction where it could be episodic or channelized. We have some excavation disturbance here, so properties change. We may have some film flow along the wall. We may have, you know, certain surface roughness to look at. Also, there could be drift degradation with time or with seismicity, all the way up to a full collapse of drift. We'll talk about that later.

Now, in terms of the flow, it really is dependent on the overall flow patterns in the unsaturated zone at Yucca Mountain, sort of shown here, depending on infiltration patterns, on the flows and the different geological horizons. One important one for seepage purposes is the so-called Paintbrush unit, which is a porous non-fractured unit that tends to dampen seasonal changes. So, below that, we kind of assume we have some sort of a steady state flow behavior that would change with sort of climate changes over long time scales, but not rapidly.

Next slide. Now, if heat comes into play, there are more issues to look at. The most important in terms of seepages, that for a few hundred to a thousand years, maybe, you will have above boiling temperatures right next to your drifts. They will dry out and they will tend to vaporize any water that tries to get next to the--towards the drift, and, so, really seepage is not even an issue in that time frame.

On the other hand, there are changes in properties, maybe because of mechanical stresses that would affect them, or geochemical changes.

Next slide. All right, so we have a bunch of complex processes to look at, and they occur on different scales. The project in the past five, ten years has done a very elaborate experimental and modeling analysis in order to understand and predict seepage. And, the results of that analysis must obviously be moved and propagated into RSPA as reasonable as possible, accounting for spatial variability and uncertainty. Simplifications have to be made, on the other hand, because it is a performance assessment, and you can't translate process model results one on one usually.

The so-called seepage abstraction model does those simplifications and integrates the inputs from various sources, defines the methodology for TSPA to handle seepage, and also provides parameter distributions and look-up tables, which are being used.

Next slide. This is really what will be done in TSPA. There's a two step approach. The first one is a probabilistic analysis of ambient seepage. TSPA loops over time, over locations in repository, and over realizations of uncertainty, Monte Carlo type uncertainty, sampling. It uses look-up tables as a function of some key parameters. I'll talk about those in a bit. Then, it calculates ambient seepage rate and also the seepage fraction over the entire repository, and parts of the repository.

The second step is a simplified bounding treatment of the coupled thermoprocesses. For example, accounting for the vaporization barrier for early time periods, and also, if needed, accounting for changes in properties as a result of mechanical geochemical effects. Currently, there is no adjustment made because the effects in the past have been shown to be small.

So, in short, seepage is a function of location, and will be a function of location in TSPA, and is also a function of time.

Next slide. I'm talking a bit about the technical basis, starting with ambient seepage. Ambient seepage has been extensively tested by about 100 liquid release tests. The water was released above drifts at Yucca Mountain, and then water is captured and the amount of seepage was measured.

In parallel, a pretty sophisticated heterogeneous fracture model was developed on that drift scale in order to understand those tests, in order to calibrate parameters, in order to provide a conceptual model to come up with seepage predictions.

Next slide. Overall, all the tests have demonstrated that there is a significant capillary barrier behavior and significant flow diversion, so seepage is always much less than the water that was injected. the model was shown to be able to accurately capture the data, and it was also shown that seepage can be described as a function of three key parameters. One is the local permeability, and actually it's variability, it small scale variability as measured in air permeability testing. One is an effective capillary strength that includes both the physical capillarity, as well as some small-scale effects that cannot be explicitly handled by the model. And, the third one is the percolation flux.

Next slide. Now, the same model was taken, the same validated calibrated model was taken and was used to do a systematic prediction of seepage over those three parameters, and essentially varying permeability, percolation flux, and capillarity over the ranges that we expect to be the ones to be used over the repository locations we're looking at. And, results of that analysis was a look-up table for TSPA. The blue arrow shows you where seepage would tend to increase in that look-up table, say permeability goes down, capillarity goes down, percolation flux goes up. You have pretty high seepage rates. So, depending on the properties you have, you feed into that look-up table, you get seepage results. We have separate seepage look-up tables for intact and moderately degraded drifts, as well as fully collapsed ones.

Next slide. Why is that? Drift degradation analysis has shown that drifts in the lower lithophysal unit, it's sort of a softer rock unit, may collapse in extreme seismic cases, and what that collapse does is that essentially the diameter of drifts would double. They would be filled with fragmented rock material. There would still be a capillary barrier up here because that material has a very low capillarity, but the seepage overall would be larger because (a) more water has to be diverted sideways, and secondly, because it's harder to divert if your opening is large.

So, for the same parameters, you would expect more seepage into a collapsed drift compared to an intact drift. And, that's an important thing to understand.

Next slide. Now, I said that we have look-up tables are developed for TSPA to work with. They can obviously not work with those tables if they do not have the input parameters described to feed into those tables. And, we have provided those parameter distributions accounting for both spatial variability within the repository, and also uncertainties that we see. I will briefly walk you through those.

Next slide. Permeability effects. What we have to do is we have to describe the variation of mean permeability on the drift scale at different locations in the repository. And, we do that using all the data we have at hand from various air permeability measurements. Some of those have been done on the scale in the niches where the liquid release was conducted. And, some are done with larger packer lengths, or some scaling analysis has to be done.

Next slide. Anyway, we use all that information. We perform scaling analysis to make results comparable. We also adjust for measurements that have been conducted in intact rock to include the impact of drift excavation on properties. We distinguish between geological units, and then we develop the spatial variability distributions.

Now, we consider those to be uncertain because our sample size is not all that large. And, so, on top of using those distributions, we develop also uncertainty distributions. And, I show those in the next slide, please.

This log-normal distribution is a spatial variability, using best data for the lower lith unit, about a 10^{-12} mean permeability. And, then, a triangular distribution was developed that shifts that variability to upper and lower values, essentially to account for the uncertainty in that distribution to be representative for the entire repository.

Next slide. Some of the same procedures done for the effective capillarity, that's the parameter that was calibrated in liquid release tests. We have ten locations, ten calibration values, and then we developed appropriate distributions again following that same methodology.

Next slide. A little bit different is the understanding of the local percolation flux. Here, we use actual modeling results of the three dimensional model scale, unsaturated zone flow model. That is a model that evaluates the steady-state flow fields for the current three climate states expected to occur within the 10,000 year compliance period, the present day, from zero to 600 years, monsoon climate, from 600 to 2000, and a glacial transition climate afterwards, basically getting wetter, as Tim alluded to with time because of those changes.

And, also, there are three alternative infiltration scenarios to include some of the uncertainties that are related to those infiltration predictions. Anyway, those distributions of percolation flux at the repository are being used and fed into those look-up tables.

Next slide. There's one issue with those predictions. The model has a grid resolution of about 100 to 200 meter. That resolution is not fine enough to account for heterogeneity below that scale, and we need to look at the heterogeneity, because our drift scale is five meters, or so. So, we have done some sub-grid modeling to understand what the sub-grid heterogeneity could be, and we actually developed a we'll call it a flow focusing regression curve that is used to broaden the distribution of fluxes from the site scale model.

Next slide. Okay, moving on to the impact that coupled processes may have on seepage. A so-called thermal seepage model was developed that solves for the evolution of seepage over time for the thermal period of the repository. It's based on both the methodology of ambient seepage and a methodology developed for understanding the Yucca Mountain drift scale test, the heater test. That model is actually complemented by pretty similar geomechanical/geochemical models, which I'm not talking about today, to understand what the additional impact of property changes from geomechanics and geochemistry might be.

Next slide. Typical model results are, and those are basically supported by all the heater testing validation we've done, and other confirmation studies, that the barrier for flow to reach a drift during the above-boiling phase is essentially a perfect one. There is no water that could get down that could seep during a period that temperatures are above 100 degrees.

Later, there is a period of resaturation, and that would lead to some delayed seepage initiation if the properties would allow for seepage. Always, that thermal seepage would be less than or approaching ambient seepage would not be above that. So, there isn't really a puddle of water that would all of a sudden flow down and would create a seepage that is much larger than any ambient seepage would be. Those results were fairly consistent over a wide range of seepage relevant parameters and conditions.

What you see here is, in blue, the seepage curve of time for an example case where we have fairly high percolation fluxes, so seepage is likely. And, you see in red the comparison of ambient seepage, if heat was not included. Three different climate stages, and you see that you have a delay in seepage, and then at some point, you get pretty close to your steady state ambient results. And, at some point later, you would probably approach them exactly.

Next slide. So, we use that to come up with a bounding case abstraction of thermal seepage. Realizing that all the complexity of those time dependent results cannot be implemented in TSPA, we essentially said that seepage exceeds zero as long as temperatures are above boiling, or clearly above boiling. And, as soon as that ends, we go up to the respective ambient seepage results, and follow that curve. So, there's some conservatism in that abstraction method.

Advantages. We can use the same look-up tables that we use for ambient seepage, and we can use results of another model scale, thermal model, that provides estimates for the drift wall temperature. And, that is done location by location, so if you have a hot waste package and you're in the center of the repository, that time scale would be larger compared to being at the edge where you might not even have boiling conditions. In that sense, then you wouldn't have that zero seepage period.

Next slide. This is a brief summary of the abstraction we're proposing. As I mentioned, ambient seepage is calculated from look-up tables. There are different look-up tables for collapsed and intact drifts, different parameter distributions for the units that the repository is residing in. There is no seepage for wall temperatures above 100 degrees C. Below, seepage is always like ambient predictions. There are no seepage changes currently due to geomechanical or geochemical processes.

What I didn't talk about is that we do not assume a flow diversion for the case of volcanic intrusion, simply because we are too uncertain about the future properties of the lava that would fill a drift. Is it going to be cooling joints? We just don't know. No seepage during preclosure because of ventilation, and no seepage increased from rock bolts. That stems from a study that I'm not talking about today.

Next slide. I'm giving you some results. Again, it's a probabilistic calculation, similar to TSPA, not identical. Essentially, we take the parameter distributions, move them into the look-up tables and get results.

I should point out that a similar probabilistic evaluation was done to understand the South Ramp seepage that was observed last year in response to those strong rainfall events. And, there was pretty good qualitative agreement.

Next slide. Okay, this is a result that shows seepage percentage over time. Those are the different changes in climates, and a blue curve is your mean climate scenario. It shows that we have about a 2 per cent seepage of all samples that we looked at at present day up to 600 years, about 5 per cent later, then 7.5 per cent, I think, for the glacial transition climate. Meaning, that here, for example, 98 per cent of all water would divert.

In terms of the number of waste packages affected by seepage for that same present day climate, there would be 8 per cent that might see seepage, 18 per cent monsoon, and then I think 21 or 22 for the latest climate, that glacial transition.

Next slide. Now, if we look at the mean seepage rate of all non-zero seepage samples, meaning, wherever we have seepage, we take a mean, it would have about 40 kilogram per year per waste package, at present, 100, and then 150. Now, translating that, 100 kilogram per year per waste package is about one drop of water every 50 seconds, if you assume that there's only one drip location of a waste package. This is not a lot of water.

What I didn't include in that graph is that there is a zero seepage period because of the vaporization barrier. You see here, the predicted timing of the boiling period for different locations in the repository on average may be 800,000 years. So, you could actually cut that part off and put it to zero on average for the first several hundred to thousand years, in most cases. It depends on the location, though. There's quite a variability of seepage rates, depending on the variability of your input parameters.

And, we can go to the next slide. This shows you where that variability comes from. These are your input parameters sampled over 10,000 samples. These are all samples for permeability, capillary strength, percolation flux, and the reddish ones here are those that have given us seepage. So, you can see that the larger fluxes would tend to create seepage. Smaller capillarities would tend to create seepage. Smaller permeabilities would tend to create seepage. There's not a one on one relation, though, even small fluxes sometimes will result in seepage if you have low capillarity and low permeability.

Next slide. Just a few sensitivities. All results so far were presented for intact drifts. These are the mean values again. That would be the result for a collapsed drift. You see that seepage increases quite a bit just because of the size of the drift and the less effect of diversion. This is for different units. So far, I presented the lower lithophysal unit. Up here is the middle non-lithophysal unit, which covers about 10 per cent, 12 per cent of the repository, versus 80 per cent here. Seepage increases mostly because the permeabilities of those units are a little lower. So, it's harder to divert flow sideways.

Next slide. This is just giving you a flavor of what happens if you do not account for spatial variability of permeability and capillarity over the repository. Seepage would decrease by a factor of 2, roughly. If you wouldn't account for uncertainty in those properties, you might see a decrease in 20 or 30 per cent, showing us that it is important to include those in our assessment.

Next slide. This is my last slide here. Conclusion. I think, many of us think that the seepage predictions in TSPA are soundly based on science, and fairly realistic, rather than extreme in terms of extreme conservatism. So, there are conservatisms, obviously. Ambient seepage in fact is sampled without any simplification to the process models we have. So, this propagates right into TSPA.

Thermal seepage is handled with a bounding case treatment. I think that we have adequately addressed spatial variability and uncertainty. Seepage varies in time and space, as you have seen. And, finally, the flow diversion that we see is important in reducing seepage, or even preventing it, in most cases.

Thank you.

GARRICK: Thank you. I'm going to ask the hydrologist member of the Board to lead this discussion. So, George Hornberger, why don't you do that.

HORNBERGER: Thanks. I'll go first. Okay?

So, you have this fairly thorough discussion that you just gave us in terms of how you have calculated the seepage flux in the past. Is anything changing as you move this model forward?

BIRKHOLZER: I don't think the plans are set for changes that we would do in the future. I guess you're probably asking in terms of the changes in our compliance period? I don't think there's anything definite yet. I guess from what I heard from Tim, the future climate conditions wouldn't be drastically different from what we already assume currently for the period after 2,000 years, what we call the glacial transition period, which has an average of I think 20 to 25 per year. And, if you assume that that is stable, you wouldn't have to change that. I don't think there is a great need of revisiting a lot of the process models. I think you might want to try to work on some of the conservatisms that are in that model, if necessary.

HORNBERGER: Okay. Just one other thing. I know the presentations are broken up appropriately, seepage into the drift, seepage out of the drift. And, one of the things, of course, we're interested in trying to figure out is you have water coming down, deep percolation, Tim called it, and let's say under current conditions 98 per cent of it is diverted. In TSPA, that 98 per cent no longer has any impact on the calculation, is that right, as it goes forward? Nobody cares about that 98 per cent because it's not going to carry any radionuclides?

BIRKHOLZER: Well, the 98 per cent doesn't pick up any radionuclides. That's true. Only the amount of water that seeps picks up radionuclides. But, the 98 per cent or, in fact, the 100 per cent, does account for the radionuclide, or the flow in the unsaturated zone below the repository, because that is obviously similarly analyzed with the model that includes the entire flux and doesn't take any 98 per cent out of it. But, locally, in terms of picking up radionuclides and then moving those into the UZ flow and transport, that's only the 2 per cent.

HORNBERGER: Okay. And, do we know that the 2 per cent that comes in is the same amount that's going out?

BIRKHOLZER: That's a question for Ernie. But, I think it is.

GARRICK: David?

DUQUETTE: Duquette, Board.

Could you go to Slide 11, please? In the right-hand picture, the cartoon that's on the right-hand side, you've shown a drip shield being intact. I believe anyway. Does anything change if the drip shield collapses because of a collapse of the drift?

BIRKHOLZER: That again is a question that others could probably answer better than I do. Obviously, if your drip shield is gone, it's easier for--I mean, water gets towards your waste packages, and it wouldn't in the other case. But, how it's handled in TSPA, I might relay that question to Bob, maybe. Oh, okay.

DUQUETTE: Okay, if it's going to be answered this afternoon, that's fine.

BIRKHOLZER: Yes.

GARRICK: Bill?

MURPHY: Bill Murphy, consultant.

You stated that your model accurately captures or predicts seepage data for all the test sites, and I was pleased to see that you looked at the South Ramp occurrence of seepage and you see qualitative agreement there. I'm curious what you mean by qualitative. What's good and what's not so good about your model? And, I'm also curious about whether or not you have considered looking at other analog systems perhaps? For example, I don't know if the tunnels are open at Ranier Mesa, but there are many places there where there is seepage which might represent a different percolation.

BIRKHOLZER: Yes. Let's talk about the South Ramp study first. I'm saying qualitative (a) because our analysis is still ongoing, and (b) because it's very hard from the--what we're seeing in the South Ramp to come up with quantitative numbers of how much water would have seeped. What we kind of know is how many locations or what spots there were, and you could make an argument that maybe 10 per cent of those 300 meters that were affected would have shown at least a bit of wetness. So, that we can compare. We can't really compare quantity.

Now, also, we haven't really assumed to model the exact conditions to do a process model for that. But, we took that abstraction model, adjusted a little bit the look-up tables, because there's a larger tunnel diameter, and we also had to adjust the amount of water getting there. You have to know that the South Ramp is located above that Paintbrush unit that I talked about, that sort of porous median, non-fractured unit. So, there isn't really a dampening mechanism, and that rainfall might have just gone down there pretty hard. And, putting that into this probabilistic analysis, we came up, I think, with a seepage fraction of 20 to 30 per cent, which is a little bit higher than the 10 or 13 per cent. So, that's what I mean with qualitative. We're still looking at maybe we can improve it.

In terms of analogs, we are looking at analogs. We have looked at analogs. We have looked at analogs in caves and in Spain, for example, there's caves that have a similar geology to Yucca Mountain, yet even higher deep percolation, and they seem to show seepage of 1 or 2 per cent, or so. So, it's really not an uncommon phenomenon. Also, maybe the ECRB cross-drift might be a sort of analog if you want to, because it doesn't have the impact of ventilation. So, if you were to see seepage, you probably would see it. We haven't so far. There's water in there, but that's mostly due to other reasons. So, yes, we have looked at that, and I guess we continue looking at that.

MURPHY: Thank you.

PETROSKI: Petroski, Board.

Slide 20. The thermal seepage curve, the slope changes suddenly at 2,000 years. Maybe you said what happens then, but what prompts that?

BIRKHOLZER: I probably didn't point that out too clearly. What we do have is a change, an abrupt, the assumption of an abrupt change in climate at 600 years and at 2,000 years, with an increase in the infiltration rate. And, that increase causes essentially more water to arrive at the drift. At the same time, it kind of cools the system. So, the increase in seepage is really related. It's a pretty fast response to that change in infiltration pattern.

PETROSKI: So, then, what happens at 2,100 years? Why is there another change in slope?

BIRKHOLZER: I guess that's a matter of plotting it. We plotted a few time steps here. So, the drastic change in response to the change in percolation is ended here, and now you have this sort of slow approaching of the thermal field, with decreasing temperatures and less thermal impact, up to the eventual sort of back to ambient temperature seepage.

And, that is then identical to the steady state seepage calculation that doesn't even include these.

PETROSKI: So, you're saying your time step changes, or do you use the time step throughout?

BIRKHOLZER: In that simulation, it changes in response to the numerical necessities for that model.

PETROSKI: But, there is no corner there. That's the question.

BIRKHOLZER: Here?

PETROSKI: Yes.

BIRKHOLZER: Well, if you probably zoom into that, you have a curve there. But, really, what it means is that if you change your climate and you change your percolation, that pretty much fast changes your entire UZ field in a matter of a few years, maybe 10 or 20 years, and once that change is done, you're back to your sort of slow change in decreases in temperatures, slow getting back to the ambient situation, and that's shown here.

PETROSKI: So, you're changing your time step in response to numerical needs? Is that what's happening?

BIRKHOLZER: Yes.

PETROSKI: Well, then, I would have a related question with regard to the grids, such as for the 2D model on Page 19. It's shown up on the upper right there, too. Why is the grid different to the left of about 20 meters, 19 meters?

BIRKHOLZER: Here?

PETROSKI: Yes, why are you changing the grid the way you do there, and how is the grid decided upon?

BIRKHOLZER: Well, there are a few criteria. A, I should have probably explained it better. This is a half drift model, and it's essentially orthogonal to your tunnels, and you have tunnels at Yucca Mountain every 81 meters or so in parallel. So, what we've done is we actually--there would be another half assumption, and then this is the center axis between two adjacent tunnels. So, if you copy those, you would have a sequence of tunnels.

PETROSKI: I understand that.

BIRKHOLZER: No, if we zoom in very close, we know that our processes are mostly--the processes that we are interested in mostly occur right next to where our heat is. Our boiling zone, our vaporization or condensation, our flux processes. So, we are very fine here, and then we kind of get closer as we move out. Yet, on the other hand, we want to go all the way up to the ground surface and all the way down to the water table. So, we focus on what we feel is important, and we were less refined where it's not important.

PETROSKI: So, in a way, you're anticipating the nature of the result. That's what determines the nature of the grid?

BIRKHOLZER: Yes, to some extent. And, that's kind of what you do.

PETROSKI: Yes, but how do you know then in your results that there aren't any artifacts on the grid? In other words, have you done sensitivity studies for the grid?

BIRKHOLZER: Yes, you want to do that all the time.

GARRICK: Andy?

KADAK: Kadak, Board.

Now, at our last Board meeting, I think we talked quite a bit about the 81 meter basis. Now, is it your work that justifies that number and the water flow around this drift? Or, whose work is it?

BIRKHOLZER: Well, I think it supports it. I don't think that our work was--if you want to really design your spacing, you want to do lots of runs, and you probably don't want to look at all the complexities in terms of seepage and heterogeneities. That's what we focused on. But, it does support the results in terms of that water can easily drain in between the waste packages.

KADAK: And, the 81 basis again was you needed that separation to get the water to flow, or could be 79.5 meters?

BIRKHOLZER: Well, I couldn't tell that on the basis of this model because we didn't really change the grid spacing. It was other studies that would have done that. But, what we have seen here is this is the centerline, we have only about 5 to 10 meters of boiling at peak. But, if you march on with time, you are sort of 90 degrees, 80 degrees, 70 degrees, moves out, and actually I think your peak temperature here goes to 70, 75 degrees currently. There's probably a margin that you could work with, but I doubt it's significant. You might be able to go to 70, others might have more insight in that.

KADAK: And, relative to your modeling versus what we just heard from the NRC, what does your model show that's different than their model?

BIRKHOLZER: I am really not in a position to answer that, because I don't know their model very well. Maybe Tim can comment on that.

MC CARTIN: I think this is probably a good example of one of the questions asked to me earlier, the difference in sophistication. We have a much, much simpler model than the Department in terms of the thermal effects, and we have really almost a bucket type of approach in looking at temperatures and for what happens during the thermal phase. So, we have done some what we call off-line analyses with sophisticated 3D thermal hydrologic models to get some sense, but we have a very abstracted model, very simple almost close to a table look-up for the thermal aspect.

GARRICK: Okay, Ali?

MOSLEH: Mosleh, Board.

Two questions. Your abstraction basically is translating the result of your calculation into kind of a table look-up. Right? Is that the level of abstraction? That's what you meant by abstraction?

BIRKHOLZER: Well, I think it's two things. It's using those, it's developing those look-up tables. It's then most importantly populating those tables in terms of defining what input parameter distributions do you need to use to capture the variability that you would have in your entire repository. It's also about making some choices and decision what to do with thermal, or maybe how to categorize results, in example, from drift degradation analysis where you sometimes have full collapse, sometimes you have partial collapse, and yet you want to simplify it a bit and make it handable by TSPA. That's what I mean with abstraction.

MOSLEH: And, in those choices, the decision points, did you do a sensitivity on the impact on the results?

BIRKHOLZER: Well, this probabilistic analysis that I'm presenting here really was for me, in designing abstraction, was a tool for understanding what these choices would mean in changes in seepage. In other words, let's say if I were not clear about what parameter distribution log-normal or normal to use for a certain parameter, I would maybe just run that analysis and I would realize there's only a few per cent change in seepage, and I wouldn't be worried about it. So, I could do that in terms of seepage. What I couldn't do with that analysis is use those results and then propagate them for the entire TSPA to see what that does. That's up to TSPA itself.

MOSLEH: And, I'm not so sure now about what you mean by sensitivities of the type that you have on Slide 28. Does that mean that those sensitivities, you determine that those are important variabilities and uncertainties to capture, and, therefore, the results actually already capture these things?

BIRKHOLZER: Yes. And, in some cases, I would decide it's not worth it, you know, if you do something and you realize that, you know, there's no change virtually in seepage, we shouldn't impose that on TSPA. So, again, based on sensitivity choices.

GARRICK: Okay, quickly, David?

DIODATO: This is Diodato, Staff. I have just two quick questions.

And, thanks for your presentation. First, on Slide 20, I wanted to follow up on Howard's observation here. You said you showed how rapidly the unsaturated zone responds to climate changes, instantaneous climate change. And, my question is, in this case then, what does that mean in terms of the ideas we heard discussed earlier about long-term average, steady state climate, versus the dynamics of the system? That's the one question.

The second question is related to Slide 12. And, here, we have the seepage look-up table. The Board sent DOE a letter a couple years ago asking a question about this table. Just quickly, on the one axis that's probably not visible to anybody who is not looking at the thing in front of them, is the capillary strength. That's increasing as it comes towards us out of the slide. And, then, the other axis of permeability is increasing as it comes from me to you. So, when we look at the rocks that have high permeability and high capillary strength, have lower seepage, in a nutshell.

In lower lith, you show these things plot around 600 pascals on the capillary strength, then about $10^{-11.5}$ on a permeability. The question the Board sent DOE was are there any rocks that DOE had identified or measured so far that plot in this red area of high permeability and high capillary strength? Has DOE found any of these rocks so far, or not? I mean, this is the area--

BIRKHOLZER: Did you get an answer to that letter?

DIODATO: We never got an answer to the letter. That's why I ask you now.

BIRKHOLZER: Are you asking me are there any rocks that would--

DIODATO: That have that characteristic where they plot in that high permeability, high capillary strength field in your look-up table here. Can you think of any examples of rocks where that's the case?

BIRKHOLZER: Well, on that look-up table, if you want, in terms of those two properties covers the range of values that we essentially cover with all parameter distributions, if you want, going to the extreme cases. So, the mean values of those that are most likely to occur, and mean in this case means assuming that we have like average percolation of 5 or 10 millimeters, means that you don't--mean phase doesn't really get you seepage. You have to go to the extremes. So, most of the rocks are typical for not really tending to have seepage. The reason that we go so high in percolation is really because of future climates, because of very focused, local fluxes that may occur close to faults, and issues like that.

DIODATO: Yes, percolations are the question here. The question is the realism inherent in this approach, whether there is realism, whether there are really rocks that have this kind of characteristic that we can identify, or not.

BIRKHOLZER: That range is based on the air permeability testing that was done in many locations, done in the repository units, basically reflects whatever came out of those slides I had on permeability. And, that range is essentially based on the inverse modeling to the seepage tests.

Now, as I pointed out, those capillary values are fairly small, smaller than what you would usually expect for just physical capillarities of fractures of maybe .1 millimeter, or so, and they are small because they do not just include the physical capillarity, but they also include some effects like coarseness, sort of local roughness of walls. So, in that sense, if you, you know, you would tend to think they are small, but they are really calibrated, a range of calibrated results here reflected in that.

DIODATO: Well, I guess I just wondered if you intended at some point to give us a rock that you've analyzed that has these properties, to show us an analysis that has these properties.

BIRKHOLZER: Then, the other one was--

DIODATO: The other one was the dynamics question.

BIRKHOLZER: Okay, the first thing I should point out, I mean, I chose that example here because of these very high fluxes. Those are fluxes of deep percolation fluxes like ten fold those that we have on average, just because I wanted an example where seepage occurs. Now, if you have ten fold the percolation flux that you would have, you know, on average, obviously, you would see a much faster response to a drastic change in climate.

The other thing is that a drastic change in climate, is an instantaneous change, is an assumption that is done for some indications. It is not anything that we would expect, I guess.

DIODATO: But, your analyses do show that with that set of assumptions, you know, rapid climate changes, the system responds in a dynamic and rapid fashion to that?

BIRKHOLZER: Yes. Again, if you have a ten times less average in deep percolation, that would tend on slowing.

DIODATO: I understand. Thank you.

GARRICK: I think we'd better go to our next speak, and I'll turn it back over to George.

HORNBERGER: Our next speaker.

HARDIN: I'm Ernie Hardin with BSC. I hope I don't keep you all from your lunch.

With regard to Dr. Kadak's first question, yes, we do have a report that describes the extent of dryout into the pillar. It runs cases for different values of the flux and thermal conductivity, and our conceptual design report gives the condition that the pillar centerline remains at or below 96 degrees C.

KADAK: Send it to us, okay?

HARDIN: Okay. I'm going to go back here. I want to recognize James Schreiber, Stephen Webb and Alfred Reed of Sandia, who are major contributors to the work I'm going to describe.

Okay, next slide. Here's an outline. Very quickly, I want to talk about the seepage water mass balance, that is, where the water goes in the EBS, the flux diversion algorithm that we use to describe diversion by the drip shield and the waste package. And, then, I want to spend a little extra time, relatively speaking, on the in-drift condensation model, which I believe you've never been briefed on before. So, I'll go through a couple details of the model development and present some example results. And, finally, talk very briefly about how flow in the EBS is partitioned back into the unsaturated zone.

Next, please. This slide addresses the question what key processes affect these estimates. And, so, we're going to address here seepage, thermal seepage and drift-wall condensation and evaporation, also, flux diversion and flow partitioning back into the UZ model.

Next, please. This slide is really the interface between my talk and the one you just heard. Jens, in fact, showed you this histogram here, and these are for the present day monsoonal and glacial transition climate states. These are estimates of the amount and frequency of seepage where seepage occurs, and for the conditions specified here, seepage occurs at 24 per cent of waste package locations.

This is the inflow into the EBS from seepage, and in our EBS flow model, we simply apply continuity. So, the outflow is equal to the inflow.

Temporal and spatial variation of this information then is propagated through the EBS flow model. And, the flow in the EBS is correlated in magnitude to percolation flux because of the seepages. So, that answers those questions.

Next, please. Insofar as continuity is concerned, this is our conceptual view of the flows or the fluxes of water in the EBS. Seepage from the crown, part of it is diverted, if you have a breach in the drip shield, which might be caused by fault displacement in the event of an extreme event, or by general corrosion. Then, part of the flux goes through the breach and can contact the waste package, and then ditto for the waste package.

The fluxes are collected in the invert, and in addition to coming into the invert, there is a calculated small flux, which we call imbibition, which is a capillary flow from the host rock. And, then, going out of the invert, we have flow into the matrix of the host rock, and into the fractures.

Next, please. This slide addresses the technical basis for the flux splitting algorithm. For the drip shield and for the waste package, we use equations of this form. N is the number of breaches, l is the characteristic length of a breach. The big L is the length of the drip shield. And, this is the incident flux, and F_2 would be the flux through the breach. And, this little f here is a random variate that when you sample over a range, to represent uncertainty. I'll talk more about that in just a minute.

We take the minimum here because the expression is designed to represent an arbitrarily large number of breaches, and, obviously, the flux through the breach. All the breaches cannot exceed the total incident flux, so we take the minimum.

Same type of relationship for the waste package. And, one of the key uncertainties here is what happens to rivulet flow after contact with the surface of the drip shield or the package. It flows off, and the angle at which it flows off, and the likelihood with which it will interfere with a breach is an uncertainty.

Next, please. I threw this in here to show the laboratory work that was done to quantify the uncertain variate in that flux diversion expression. And, of course, this is a full-scale mockup of a drip shield. Breaches such as these would only occur due to general corrosion. It could take a very long time.

Next, please. This slide addresses the key assumptions and uncertainties associated with the EBS flow, seepage mass balance and flux diversion aspects. One of the key assumptions here is that seepage from the drift crown impinges directly on top of drip shields, and this is a simplifying assumption. It tends to increase or maximize the availability of that seepage for flow through breaches, if they exist.

Another key assumption here is that all leakage through drip shields, through breaches in drip shields, impinges on the waste packages. This is also an assumption that increases the availability of flow for interaction with the waste form. There is an alternative mode of flow here, which is that the flow that finds its way through a breach in the drip shield could cling to the underside of the drip shield and be diverted. That is not included in this model.

There is also the possibility of a mineral scale or debris accumulation on the surface of one of these components, and the approach that we've taken here we think ensures that the average response when a model is applied over many waste packages or drip shields would be honored.

And, I already talked about the key uncertainty in flux diversion, which is just how does flux run off of a curved surface.

Next, please. Okay, now we switch gears and talk a little bit about in-drift condensation. This model that I'm about to describe was developed a couple years ago in response to observations underground at Yucca Mountain, particularly the closed interval of the ECRB cross-drift that you may be familiar with.

I should point out from the onset that evaporation and condensation processes are limited by the availability of moisture in the environment, not by the availability of heat, that is, the waste heat from spent fuel is sufficient to evaporate plenty of or all of the incident percolation on a drift. That has been taken into account in the development of this model.

The objective for developing this model is to represent drift-wall condensation as a contribution to advective transport of radionuclides in the EBS. I think we recognize that condensation is going to occur perhaps everywhere eventually in the emplacement areas. As the system cools down, there is water present, and the temperature differences from point to point in the system may become very small, and those are all conditions with which we are very familiar, and they give rise to condensation in our everyday experience.

What the objective here is is to look at what kinds of fluxes in the environment, similar to seepage, do we need to consider for inclusion in the assessment of advective transport of radionuclides in the EBS.

So, how do we implement this? I haven't described the model yet, but we will implement it as an additional source of water. It's treated as seepage, so it finds its way through breaches in the drip shield, and so on, and it's included in the downstream water mass balance, so it's included in the water that is partitioned back into the UZ.

Next, please. Okay, this is kind of a conceptual run-up to the condensation model. We recognize that condensation occurs in three stages as the repository cools down. In the first stage, the entire emplacement area is at or above the boiling temperature of water, and, so, you get evaporation, but no condensation. You would get condensation over here in the unheated region because of transport.

In the second stage, part of the repository, I think the edge of the layout, has cooled to below the boiling temperature of water. So, now, the drift wall is cool enough to sustain condensation.

And, in the third stage, the entire emplacement area, within that area, the drift-wall temperature is at 96 degrees or less, and now you can have evaporation or condensation anywhere in that area.

The model was developed to describe this stage. This stage here is relatively brief, and we have other modeling results that suggest that the results we did up here can be used here for the purpose of the model.

Next, please. Condensation, or I should say this evaporation transport condensation process we're talking about here is a drift-scale, even repository-scale process. So, you have to start with a repository-scale model for heat transport, and this is how we have approached that here. The repository is modelled using a super-position solution of heat sources, all with decaying characteristic, so its line average heating in a number of drifts.

And, for discussion purposes, I will present results for Drift 7 as examples. So, the super-position then gives us temperature profiles from one end of the drift to the other. The dotted line here and here indicate the extent of waste packages emplaced. This is the boiling temperature up here. And, we're going to apply the model, we're going to develop it and then plug in numbers to use it for 1,000, 3,000 and 10,000 years. These are time slices. It's a steady state model.

Next, please. Okay, describe the model. This is a network model, or you may have heard such models referred to as lumped parameters. It's the type of modeling approach that you would use if you are interested in the bulk response. We want to know how much water could be deposited on the wall of the drift. We're not interested in the fine details of how that, or where that water would be applied, whether it would be, for example, at the lower limb of the drift, or at the crown. We're not interested in that detail. We just want to know the overall magnitude of the effect.

So, what we have done here, this particular schematic gives some of the information. Waste packages are represented as nodes. Drip shield also. The under and upper surface of the drip shield, separate nodes. The drift wall is another node. And, the top of the invert is another, and the bottom is another. And, then, within the air space, we have nodes, and we allow a dispersive mixing type transport process axially within the air space.

There is one process implemented above the drip shield, and another one below. So, you have about 1,000 nodes for a drift, and, so, you have several thousand equations, solve them simultaneously. Again, we do it at these three time steps. We have these nodes. We do include the unheated end regions. That's important because a lot of condensation can occur there.

Next, please. The source conditions are important here. What we have done is we have used that super-position solution to calculate drift wall temperature, and as the moisture source condition, we use the saturation vapor pressure at that temperature. And, then, we use a mass transfer correlation to allow that vapor to issue into the air stream.

So, that source condition is applied at the drift wall, or either at the top of the invert or at the bottom. We'll talk more about that shortly. The evaporation rate in the model is limited in a now linear way by the percolation in the rock incident on the drift footprint. There is a limit there.

Next slide, please. Now, this slide will attempt to briefly explain where the dispersion coefficients come from that we use to describe axial transport. Steve Webb at Sandia has done a number of 3-D CFD simulations. They are steady state, single component simulations using air. The waste packages and drip shields are represented explicitly. He uses 14 waste packages in an array with no flux boundary conditions at the end.

The CFD model also includes 5 meters of surrounding rock, and then he uses a conduction only super-position type solution to fix the temperature on the outside of that annulus of rock.

I showed you a temperature profile down the drift. Towards the limbs of that profile have slope, and we call it tilt, and it makes a big difference whether the boundary condition you apply to the outer surface of the annulus of rock is uniform or whether it is a sloping or tilted boundary condition. That affects the flow. He did it both ways.

To calculate a transport behavior, we put a non-buoyant tracer in the gas phase. We introduced the tracer at one concentration, pulled it out at another concentration, and then calculated the flux. The result is if D-naught is the binary diffusion coefficient in the gas phase of 2 times 10^{-5} in SI units, then the range of dispersion coefficients is roughly 200 to 4700 times. So, this is the tilt case, and this is the uniform boundary condition.

Next, please. So, those are our high and low d-values. Key assumptions used in this model, I described the source condition for evaporation. This certainly maximizes the availability of water to evaporate. The invert is modelled as transmissive to vapor, but not to heat. That turns out to be just a necessary model simplification. But, for the high invert cases, and I will talk about this more in a minute, this certainly, what we're doing is we are applying a saturated vapor source condition at the top of the invert. Now, this is sort of analogous to seepage entering the drift, being diverted by the drip shield and getting into the invert and wetting up the top surface of the invert to a high degree of saturation.

In addition, this model is a mass balance on water, but it is not a heat balance, in terms of the latent heat of water. So, that's an assumption and limitation on the model.

Some of the uncertainties, I've listed three here. Each one is addressed in the model results by using a range of parameters. For the uncertain liquid saturation distribution in the invert, we either set the source vapor pressure condition at the top or the bottom of the invert.

Another condition I haven't talked about yet is whether the air volume under the drip shield is ventilated to the air volume outside. We ran it both ways. Clearly, ventilation will set the vapor mass fraction under or equal to the vapor mass fraction outside the drip shield. It makes a difference.

The dispersion coefficient we talked about. Now, there are mechanisms such as barometric pumping and large-scale gas phase circulation, in both the drift and the rock that could increase transport of vapor axially down the drift. Those will tend to increase the transport of vapor from the emplacement area outside where it condenses, and doesn't wet up waste package locations. So, there are processes that are not included in our development of the dispersion coefficients that might decrease the amount of water available.

Next, please. Okay, here's example results. This is for the ventilated drip shield. Condensation rate here is given in kilograms per year per waste package location. This is condensation on the drift wall. Condensation occurs if the vapor mass fraction in the air is greater than the equilibrium vapor mass fraction at the temperature of the surface where you're contemplating condensation, so, in this case, the drift wall.

Said differently, if the red curve crosses the blue one, you get condensation. But, in this range here, the curves coincide and we get condensation at about 100 kilograms per year per waste package location. Again, this is the emplacement region between the dotted red lines, and we did not plot condensation in the unheated regions outside, but if we had, it would be a strong signal.

So, the magnitude of condensation as calculated by the model then is comparable to seepage magnitude that you saw in those histograms previously.

Next, please. Now, here's the same plot I just showed you, vapor mass fraction for the low dispersion, low invert case at 1,000 years, with the mean percolation. And, here's a comparable plot, but the dispersion parameter now has the high value, 4700. So, now we are looking at the other end of the range for axial transport of vapor. In this case, there is no condensation in the emplacement area. The curves do not cross. They cross outside. So, what's happening here is the--well, the emplacement area stays relatively dry during this time interval.

Now, this result is corroborated by some other modeling that the Science and Technology Program is in the process of completing. Basically, if you have enough transport axially in the drift, you do not see condensation in the emplacement area.

KADAK: Why is that?

HARDIN: Well, you can get the water vapor to go outside the emplacement area, it's much cooler there, that's where it prefers to condense.

KADAK: So, you are arguing for circulation in the drift to avoid condensation on the waste packages?

HARDIN: We have identified that this process is opprent (phonetic), and these two cases are equally weighted in our abstraction that we recommend for modeling.

KADAK: Okay, go on.

HARDIN; Okay. Next, please. Okay, this slide addresses some of the assumptions and uncertainties attendant to this model. We have propagated the three major uncertainties that we've identified into eight cases. We selected the ventilated cases for use in the system model. That is consistent with the controlling assumption in the multi-scale model, which is our thermohydrologic model that we used for the system dose assessment. So, for consistency, we have adopted this one.

If you use this one, if you use the non-ventilated drip shield assumption, you get a different set of cases, and the system behaves a little differently. Now, you have a tube within a tube, and, so, you can have transport axially within the air space under the connected drip shield segments. But, as I think I will show you on the next slide, the condensation on drip shields and waste packages is not included in our abstraction, and is dealt with in a different way in our screening evaluation for features, events and processes.

Now, remember, I introduced the talk by saying that condensation can occur eventually throughout the facility. It's quite likely that condensation will occur. And, so, the consequences of that low-level condensation occurring throughout the facility are excluded. The model describes some of the thermally driven, stronger condensation effects that meet the objective for developing the model.

So, finally, how do you implement results such as I described in a system model? The way we have done this is we had developed statistical correlations, combining for each drift, and for all percolation conditions for each of the time slices. We developed a statistical, it's a regression, and we have one for the probability of condensation to occur on the drift wall at a waste package location, and we have another for the magnitude.

Next slide, please. Now, we get to switch gears and talk very briefly about how we partition flow back into the UZ. This is a schematic from a more comprehensive description of the abstraction. This is how the system model implements the partitioning.

If you start from, you have water effectively flowing from the waste form, through corrosion products, into the invert, then the interface we're talking about is right here. And, very simply, what we have done is if there is seepage, that represents generally a stronger flow than any other capillary flow in the system. So, that seepage, once it comes in, it's coming from fractures, it goes back into the UZ in the fractures. And, the imbibition flux I pointed out is a much weaker flux. It comes from the matrix of the rock, it goes back into the matrix. Drift wall condensation, if it occurs, is treated as seepage.

So, what we have done here, it addresses the drift-shadow effect. The drift-shadow effect is the modification of the flow field by an opening in this unsaturated rock. And, the key to the drift-shadow effect is that below the drift opening, you don't really have much fracture flux. And, so, we have honored that in principle in this abstraction by putting radionuclides, if released from the EBS, into the matrix, unless they are seepage, and then they go into the fractures.

Next, please. So, in summary, I have described the EBS flow model based on continuity. The seepage water mass balance, it's a macro-scale mass balance. Condensation is then added to seepage. The thing that's not included in the mass balance is the source of the condensation. That's evaporation somewhere where you have less water. So, that's an inherent assumption to the representation of condensation. I have described the experimental basis of flux diversion.

For the condensation model, I've been very careful to point out the modeling objective. It's a mass balance. We have identified three key uncertainties, and we deal with them in the abstraction by specifying ranges on key parameters. And, we have summarized those results in statistical correlations, percolation flux being the dominant independent variable.

Next, please. In regard to EBS and UZ flow partitioning, this is really part of an EBS-UZ interface model that governs the transport of radionuclides. But, I have just told you about the flow part of it, and we believe, based on our own sensitivity studies, that we have captured the essence of the drift-shadow effect by doing it this way, and that it's realistic.

So, I'll leave you with one final point. Seepage, plus drift-wall condensation gives you advective flow in the UZ, gives you advective flow in the EBS. So, those are the-- I know that's a key distinction. Without seepage or drift-wall condensation, the transport of radionuclides in the EBS is limited to diffusive processes.

Thank you very much.

HORNBERGER: Thanks, Ernie. So, if I have this right, you actually can have up to roughly twice as much water leaving the drift as coming in, because you're condensing an amount about equal to what is seeping in; is that right?

HARDIN: Right. Or stated differently, we're not accounting for the dry-out effect, whence came the vapor that condensed. Somewhere, there's a very dry place.

HORNBERGER: So, where is that? Where does the water come from? Where are you getting the vapor from?

HARDIN: In general, it comes from wherever it's warmer in the emplacement area.

HORNBERGER: I understand the physics, but you're having condensation every place relative where you have thermal gradients, and, so, you don't have a place that's really drawing out.

HARDIN: I understand the question. The type of--my remarks about condensation being a process that may occur everywhere eventually apply in the very long-term.

HORNBERGER: I know. I understand that.

HARDIN: Where you have the strongly thermally driven process, then what I just said applies, in that it is the hottest part of the emplacement area that is the potential source for the operation that causes condensation.

HORNBERGER: Okay. So, let's take a drift, the center of the drift is the hottest spot, as you've shown, and, so, the condensation at the ends comes from water vapor at the center of the drift. Where does that water vapor come from, unless it comes from seepage that came into the drift?

HARDIN: Okay. It is possible to have--I understand the question. You know, the near-field host rock is permeable to gas, so it's evaporating from the rock, or from seepage.

HORNBERGER: But, you're not taking into account evaporation from seepage?

HARDIN: Seepage is not represented explicitly in this model. That would be another order of model complexity.

HORNBERGER: I forget what seepage was, F_2, or something. You're not decreasing F_2?

HARDIN: That's correct. Because of the conservatism that you're trying to box in is just that, not decreasing F_1 where appropriate, where seepage might occur.

HORNBERGER: One last question. You're considering condensation on the drift walls at the cooler ends because the drift walls are cooler than the drip shield or the waste package. Could you envision getting condensation on the drip shield or waste packages if in fact the latent heat transferred to the drift walls at the ends actually warmed the walls?

HARDIN: Yes. Could I see backup Slide 26, please? It is possible to calculate condensation directly on the underside of the drip shield, or on the surface of a cold waste package. But, that only happens in this model under certain conditions. And, basically, you have to have that high invert, you have to have high saturation in the very top layer of the invert in order to get the vapor pressure up in the air space under the drip shield. This figure kind of represents that. These are the ventilated cases that we used.

I should also point out you also have to have a non-ventilated condition. You have to restrict the communication of gas. So, here are the non-ventilated simulations, so it shows that yes, at the drip shield, we do get condensations at these time slices, and so forth. For the low invert condition, we get the same result we get over here for the ventilated case.

HORNBERGER: Thanks. Other questions? Andy.

KADAK: I'm kind of confused. I thought I understood what you were saying, but then this last exchange confused me again. Is the drift non-ventilated, as a matter of fact?

HARDIN: This is post-closure. We do not take account of processes driven by barometric pressure flow.

KADAK: Right. And, why wouldn't you assume that to be the case for your TSPA?

HARDIN: I think it's another order of model complexity. We can drive this model with a signal, which represents the barometric effect, and the result will be that more water will get transported away from the waste packages. We haven't done that.

KADAK: But, what I'm asking I thought you said, and correct me if I'm wrong, but in your modeling of the water flow, you used the ventilated case as your representative scenario. And, I'm asking if, in fact, it is likely to be less ventilated than ventilated, why not use that as the model?

HARDIN: Okay. Well, the argument for using this case, besides consistency with other models, is that there are plenty of paths by which gas can get around the drip shield. It is not sealed. So, there's a gap in the joint between drip shield segments, and it is not sealed at the bottom either, so gas can get in and out.

KADAK: It's a ventilated drip shield, not ventilated drift?

HARDIN: Yes, sir.

KADAK: Okay. I'm sorry, I got confused. The other question is relative to where the water is. The water is condensing in the ends of the drifts?

HARDIN: Yes.

KADAK: Okay. At some point, the temperature of the waste package will go to below 100 degrees Centigrade.

HARDIN: Right.

KADAK: When will that, roughly, occur?

HARDIN: That occurs within 200, 300 years at the edge.

KADAK: Okay.

HARDIN: At the end of the drift.

KADAK: For the waste package, I'm talking about.

HARDIN: Yes, you will have a cool package at the end of the drift, at a location where the drift wall temperature cools below 96 degrees, within less than 300 years.

KADAK: And, how about the rest of them in the center?

HARDIN: They take much longer. They can take up to 1,000 more years.

KADAK: 1,000 more. Okay. Now, what happens then to the condensation on the waste packages? And, does that not also make another source of water which, as I recall, you said was not included in the waste package analysis?

HARDIN: Yes, but I'm trying to place the question.

KADAK: You said you didn't consider condensation as a source of water in the waste package degradation analysis.

HARDIN: Okay. What we have done for the purposes of this model, we have only considered condensation on the drift wall, because that's where a lot of condensation occurs.

KADAK: I'm asking--

HARDIN: It also occurs on the package itself, based on some of our results here.

KADAK: Okay.

HARDIN: It's not all cases. It's just a few cases. It requires, first of all, that you restrict gas phased communication under and above the drip shield first. It also requires that you use a high invert source condition, as we've postulated in our model, and that was the condition where you have high liquid saturation in the very top layer of the invert. The invert is a porous medium, very permeable, and it's unsaturated. Water will tend to flow right through it into the rock below. On having a high saturation condition in the top of the invert, is a limiting condition really.

KADAK: Well, I'm told that the drift environment is rarely quite humid, 100 per cent, and I'm asking the question at your 10,000 or several thousand years, will you get condensation on the waste package that will be the source of water--

HARDIN: Yes, it is possible.

KADAK: --which will be equivalent to the seepage rate?

HARDIN: No. As you get out to later times, the deltees (phonetic) in the system are smaller. It becomes harder to predict what the condensation and evaporation rates might be from point to point in the system. But, yes, there would tend to be condensation in the cooler places, which might include cold waste packages.

HORNBERGER: Thure?

CERLING: Cerling, Board.

It seems that combining the two talks, that condensation at the end of the drifts then would somehow influence the percolation flux. And, so, can you guys give at least a qualitative description of how much that might be?

HARDIN: Yes. It could be all of the flux that might--it could be 100 per cent, and I showed you that in that figure that showed the distribution of the vapor mass fraction. Do you know which one I'm talking about? It's the two blue humps. 17.

In this case right here, there's no condensation in the emplacement area, because, again, with enough axial transport, the vapor is driven out to the cooler regions where there are no packages. That's a plausible effect. We have other models that predict the same type of response when we put parameters in there to represent this effective axial transport mode.

HORNBERGER: In fact, isn't that more likely than the top one, in terms of realistic boundary conditions?

HARDIN: That's hard to say. I kept in mind the possibility that you could have seepage in the source region, and that it would wet up the invert. It's possible. Whether it deserves equal weighting or not, is open to discussion.

HORNBERGER: Go ahead, John.

PYE: Okay. Pye, Board Staff.

I'd like to compare the diagram in the previous presentation, five, which shows the vapor barrier, and it indicates that vapor transport is away from the drift, compare that to Slide 10 in your presentation, which shows that the vapor moves into the drift. So, conceptually, can you explain why the difference?

HARDIN: Yes, the difference is in the application of boundary conditions to the simulation. In a two dimensional thermal hydrologic simulation, it's a--you do not have transport in the third axis, the inherent limitation, dimensionality of the simulation. And, if you do that, then all the vapor flow has to be outlined.

PYE: Going back to a point that was made earlier about the--the concept there is in the two dimensional things, the heat basically pushes moisture away from the drift to drain, but in this case, that wouldn't be the case. The water would move into the drift and down to the end. So, why do you need--

HARDIN: I think we've said there was margin there.

PYE: Okay.

BIRKHOLZER: I'd like to say something in response. I think even if you open up your drift in kind of a thermal analysis that was presented earlier, you would have part of the vapor go back towards the drift as a boundary condition, but you'll still have part of the vapor going out into the rock. So, having that non-vapor transport along the drift is somewhat conservative, but it's not exclusive.

HARDIN: It's not unrealistic.

BIRKHOLZER: There is a current S&T project that includes that as the transport, and it's seepage and the moisture that should be available in the fractured rock, and it probably could lead us to relax those assumptions a bit.

PYE: Thank you. In Slide 15 and Slide 19, you use the term ventilated and non-ventilated. I think you need to explain that a little more carefully.

HARDIN: Okay. We're talking about this slide here?

PYE: Yes.

HARDIN: The uncertainty is the amount of, the extent of gas phased communication between the air spaces below and above the drip shield. Stated a different way, the amount of gas and water vapor included that would migrate in and out through the joints in the drip shield and through spaces, gaps in the footings.

GARRICK: All right, to avoid me getting into more trouble with the people anxious to get to lunch, I think we'd better truncate the discussion. And, I guess I'm going to suggest that we be back at 1:30.

(Whereupon, the lunch recess was taken.)

AFTERNOON SESSION

GARRICK: I think we will resume and end our recess, and go onto the next item on our agenda. Now, we're going to go onto the radioactive side as well, and get some indication of what happens as radionuclides move through the mountain. So, why don't you introduce yourself, and we'll take off.

SASSANI: Okay. The title of this talk is mass and activity of key radionuclides potentially released from waste forms, waste packages and drifts over time. I'm Dave Sassani. I work with Management and Technical Support Services contractor, providing consulting to the Department of Energy on the technical areas of geochemistry, coupled processes, and waste form. I'll be doing the first portion of this presentation, focusing in on processes occurring in the waste forms that define the source term, and the second half will be done by Rob Howard from Bechtel SAIC Company, covering the transport within the waste package, through the drift, and to the UZ over time.

Next slide, please. So, just a quick overview. The source term presentation, I'll go through concepts on the source-term model, I'll discuss descriptions of these models and the bases for those, focusing specifically on waste form degradation and solubility-limited concentration models.

I'll cover then some additional project studies that are relevant to these areas, and then transition a little bit and try to cover some of the Science and Technology source term targeted thrust projects. This will be a little bit of a transition, and then hand it off to Rob, or we can do questions at that point, whichever.

Before going on, I want to acknowledge that all of the work that I'm going to present here today, in both project models and the program, S&T source-term targeted thrust projects, have been done by numerous investigators and scientists, both within Bechtel SAIC Corporation, and at the national laboratories, as well as a number of universities when we consider the Science and Technology aspects.

So, I'm going to focus in on concepts now. This is a conceptual diagram of the drift, with both processes and materials within the drift shown. Of course, I'm going to focus in my presentation within the waste package on the waste form areas. What else is in the package besides the waste form? Well, there's things like the basket materials, carbon steel, aluminum alloys, which of course form corrosion products after a waste package is breached, which contribute to some of the transport aspects. I'll focus in on the waste forms themselves, and then Rob Howard will follow up with the transport through the system to the UZ.

Next slide, please. This is showing the two primary waste package configurations that we evaluate, one being commercial spent nuclear fuel waste packages, which can contain spent fuel from either boiling water reactors or pressurized water reactors, and co-disposal waste package shown here, which contains both high-level waste, glass canisters surrounding DOE spent nuclear fuel canister, shown in the center.

Today, I'm going to primarily concentrate on the commercial spent nuclear fuel aspects. So, let's go to the next slide, please.

Well, what do we know about degradation of commercial spent nuclear fuel? It's uranium, primarily as uranium dioxide pellets, in the fuel rods, and we have a wealth of information from both natural systems and from laboratory testing on UO_2 and spent fuel itself in terms of what happens to uranium oxides during corrosion and alteration processes that create uranyl phases.

This figure on the left is a photomicrograph of a sample showing some original uraninite here, this grain, and some zonation of the uranyl alteration phases formed during oxidative alteration of that. This yellow phase at the top, the yellow crystals, are schoepite, which is a uranyl oxide hydrate. This sequence in zoning of the alteration is a very common occurrence. It's been observed in numerous deposits going into uranyl silicates with further zones, and the diagram on the right shows, again, the development of these phases, uranyl oxide hydrates, uranyl silicates, at later times or in the outer zones, this time in a case over weeks, which are based on laboratory testing done at Argonne National Laboratory.

So, this information from both natural analogs and from laboratory testing provides a very nice conceptual framework within which to develop models of the alteration of the spent fuel and potential release of radionuclides through time, which I will now go into.

Next slide, please. So, we're now going to talk a little bit about the description and the integration of models and the technical bases for those for the project.

Next slide. This conceptual diagram is fairly complicated, but it shows the connections within the source term model. My presentation is going to focus on these boxes here, which are two of the primary models for the source term, the waste form degradation models, which give the far from equilibrium aspects of the system as it oxidatively alters to produce dissolved concentrations in the waste package. And, then, the closer approach to equilibrium processes represented by this box, dissolved concentration limits, usually defined by solubility limits, of minerals.

These two models are used in conjunction, or meant to be used in concert with the system process determining which one is the controlling limiting factor for various radionuclides through time.

What else I'd like to point out on this slide is these models within the package, and the waste forms have upstream process connections to both EBS chemical environment in terms of gas compositions in the package, and, of course, the in-package chemistry model, which is produced both for CSNF packages and the co-disposal packages with separate chemistries providing inputs.

Downstream, the connections are primarily to the EBS radionuclide transport models, which plays a large role in terms of the flux of these away from the waste form in determining which of these becomes a controlling factor for which radionuclides. Rob Howard will talk more about this end of it, and out through the invert. You can see, again, we do check in the invert, dissolved concentration limitations if there's changes in chemistry to be looked at there.

So, I'm going to go into some detail about commercial spent nuclear fuel degradation models first, and then solubility limits second. So, next slide, please.

Okay, there's two primary portions to the spent fuel degradation model which are shown by these two bullets here. One is a gap and grain boundary instantaneous release fraction of a number of radionuclides based on inventories determined from, for example, Grade 92 for the radionuclides primarily that migrate to the boundaries of UO_2 grains, or migrate to the interface between the uranium dioxide pellet and the cladding. These are represented as triangular probability distribution functions based on the variability of uncertainty in the inventory data. And, you can see for Cesium 137 and Iodine 129, there's about 10 per cent, or about 25 per cent of those as a maximum that can be contained there. Those are instantaneously released whenever that fuel becomes available for alteration.

In addition to this, there's matrix dissolution rates which depend on chemical conditions and temperature, based on the flow through testing that's been done. And, I'm going to talk to some more details on this in the next slide.

So, what I've got here is, again, a conceptual diagram for the CSNF degradation rate model, which is a function of temperature and chemistry. And, in addition, what we're going to look at a little bit here is the cladding degradation model, which determines the mass of exposed fuel based on the cladding failure for that package.

The defective clad is taken as split instantaneously after a waste package breach testing in humid air environments about 175 at Argonne. It took about two years to split the cladding, which on the time frames that we're concerned with is instantaneous. All of the mass of fuel in that split rod becomes available then to alter. And this diagram down here shows more detail of a rod with the cladding split apart, and you can see the unreacted UO_2 here in gray, the alteration rind shown in orange forming, which is generally schoepite, and the cladding itself. This area splits apart as the fuel alters to schoepite, due to a volume increase as this alteration rind forms.

The porosity of that rind is treated as uncertain based on determination of porosity from schoepite, and the alteration rind itself is assumed saturated, which plays a role both, it's used for the water volume into which the radionuclides can dissolve from the altering fuel, and it produces the diffusion transport path for radionuclide transport out. Rob Howard will talk about this a little bit later. I want to reiterate--

KADAK: Just a clarification? This will occur regardless of the--I mean, suppose the waste package remains intact and the canister is sealed and welded?

SASSANI: No, I'm sorry, this instantaneous failure of the cladding and splitting of any defective cladding occurs once there's a breach in a package.

KADAK: Once there's a breach.

SASSANI: So, for unbreached packages, none of this is happening.

KADAK: Okay. And, when you say the package, you're talking about the waste container itself has to be breached, not just the waste package?

SASSANI: The waste container. That's correct.

KADAK: So, you've got two breaches theoretically before this happens?

SASSANI: No, you just need one breach through the package so that the external environment can invade.

KADAK: Okay, I guess I'm confused. What is that cylindrical gray thing? Is that the waste package or is that a container containing spent fuel that you will be putting into the waste package, which I would call the over-package?

SASSANI: Okay, I'll clarify here. All the models that we're going to be talking about today are all for our previous design, which is a waste package, which is a dual layer waste package. Only the outer Alloy 22 layer which we take performance credit for. This is not for the revisions that are designed which would include the TADs.

KADAK: But, even that waste package, wouldn't that be a seal welded probably inerted container?

SASSANI: Yes. And, so, once you get a breach through the container wall, such that the--

KADAK: But, you need two breaches, is what I'm trying to get to, the waste package breach, and then that breach, to get access to the fuel?

SASSANI: I guess I'm confused because that is one container that needs to be breached. There's no inner-container. If this is what you're--you're referring to this up here?

HOWARD: I believe what your question is you believe we've got the Alloy 22 outer waste package, and then we've got the 316 stainless steel inner shell.

KADAK: Correct. Is that 316 inner shell, which will contain these spent fuel elements, is that a sealed and inerted can?

HOWARD: Yes, sir, it is.

KADAK: Okay. So, am I correct in saying two breaches have to occur?

HOWARD: We take no performance credit for that, once the Alloy 22 outer shell is breached, we assume that the inner shell fails rapidly thereafter.

SASSANI: You could add up here, not only does the effective clad split instantly after the waste package breach, but you could clarify that with once the Alloy 22 is breached, the inner stainless steel container is assumed to instantly breach.

KADAK: Thank you.

SASSANI: Sure. So, going back to this alteration rind, it's used both for the water volume and for radionuclide dissolution from the altering fuel and the diffusive transport path. And, just to reiterate, radionuclides are released from the fuel both by the instantaneous gap and grain boundary right upon breach of the cladding, split of the cladding, and also then by the matrix degradation alteration rate, some more details of which are shown in the next slide.

So, the overall rate for spent fuel matrix degradation is constrained in our system under two major conditions. One, under acidic conditions where the rate is a function of both pH and the oxygen fugacity, or oxygen partial pressure, or redox conditions, whichever you prefer, and also under basic or outgoing conditions where the rate is then a function of the oxygen fugacity and the total dissolved carbonate.

These rate equations are both based on the flow-through testing of commercial spent nuclear fuel and UO_2 dissolution under various conditions. An example is shown here in this plot where log of the corrosion rate per unit area is shown as a function of inverse temperature, and the negative log of the oxygen fugacity in the system. The data points are from both CSNF and UO_2 dissolution rate, experiments under various conditions, and the surface is the fit to those data points.

An example calculation of the degradation lifetime based on these rate models is shown here, years on this axis as a function of pH, and this is the low temperature result, going to higher temperatures as the rate goes up at higher temperatures. So, you have longer lifetimes at low temperatures, and you can see no pH dependence in the outgoing region, and a pH dependence here.

So, these provide that radionuclides are potentially available for release, but those masses from the altered fuel are turned into concentrations using the rind water volume for comparisons to limitations based on solubility controls. For something like technetium, there really are no solubility controls that are imposed, so technetium would be limited by the degradation rate of the fuel, its release. But, for something like neptunium, we do have solubility limiting phases, and I'll talk about those now.

On the project, we've considered a number of different models for constraining. Neptunium concentrations based on mineral solubilities. We've looked at both pure phase models, which I'm referring to neptunium oxides here, both Np_2O_5, which is a metastable pentavalent neptunium oxide, and neptunium dioxide, which is the stable tetravalent neptunium oxide. And, these models are based on thermodynamic data reviewed by the NEA, primarily taken from Lameer, et al., 2001.

The solution itself is dominated by neptuneal species. These are pentavalent dissolved species, which indicates that we'd be dealing with a reduction reaction for precipitation of NpO_2, or reduction equilibria. That's not the case for the pentavalent neptunium, which, in fact, precipitates very readily in over saturation studies in simple systems, in which the more stable NpO_2 phase is not observed at very short times in these simple systems.

In secondary phase models that we have considered, which are coprecipitation of neptunium within the uranyl alteration phases, the uranyl silicates and oxide hydrates, it's expected that the neptuneal line would substitute for the uranyl line in these phases based on the theoretical studies from Burns, et al., 1997, which would involve, of course, a coupled substitution to maintain charge balance in the crystal structure.

But, the proximal alteration phases, for example, schoepite, in particular, don't appear particularly effective at even taking up neptunium in any manner. And, so, the project currently does not implement this kind of a model. What the project does do currently, for our expected system, is to use the neptunium dioxide constraint within the waste package for a number of reasons. The kinetic barriers for reduction are not expected to be an issue in this case, primarily based on the numerous and massive reductants that exist within the package itself, primarily the waste form, in which the neptunium resides, and the steels, the carbon steel and the steel alloys, which comprise a very large mass of reductive material inside the package.

In addition, neptunium is expected to be tetravalent within the spent fuel itself, and, in fact, may even be controlled by the corrosion process of the spent fuel at an even lower redox condition than the atmospheric FO_2 that we impose.

And, lastly, for our expected system, it's going to produce a gradual increase of dissolved neptunium concentrations, starting from well under saturation with all of these phases. The first phase that would saturate would be this phase, if the system can even reach saturation with one of those pure phases.

However, outside the package in the invert, which is a more simplified system, does not contain quite the mass of reductants. As a hedge against the uncertain precipitation kinetics for neptunium dioxide, we apply the constrains from Np_2O_5. Now, there are additional studies relative to what the extent of those kinetic rates may be for NpO_2, and they are shown on this next slide.

Finch did a study in 2002, where he formed NpO_2 directly by humid air alteration of neptunium-doped U_3O_8 as an oxidized form of UO_2 at elevated oxidation conditions, hydrogen peroxide was added to the system.

In about three weeks at 150 degrees C, he formed very nicely crystalline neptunium dioxide, and at 16 weeks at 90 degrees C, there was a formation of neptunium dioxide and also Np_2O_5, although this was determined by XRD because there is less overall reaction progress of the starter materials, and, so, very little of the U_3O_8 neptunium had reacted at this point.

In addition, for precipitation of NpO_2 directly from solution, simple solutions without any reductants added, Roberts, et al. in 2003 observed at moderate pHs in less than three months at 200 degrees C, a precipitation formation of nicely crystalline neptunium dioxide.

Well, there is additional information besides these tantalizing pieces of information of relatively short times from a geologic standpoint, or even from a standpoint of decades time frame, relatively short time frame PO_2 formation. In our expected system, I think we would even expect to have a much longer than these periods of time, even at the lower temperatures.

Next slide, please.

LATANISION: Excuse me.

SASSANI: Sure.

LATANISION: The precipitation would effectively mean that neptunium is not mobile, not available for transport?

SASSANI: Well, it would be precipitated out as neptunium dioxide, and it would set the remaining concentration and solution to the solubility and equilibrium with that phase, which is a little bit lower, and I'll show you some of those values here.

LATANISION: Okay.

SASSANI: So, this is a comparison of the spent fuel drip and batch dissolution test data with the solubility models I've just referred to. Now, this has been shown a number of times, this plot is showing lot to the neptunium concentration dissolved in solution as a function of pH. And, there is a couple of things on here. I want to talk about the data points first, which are from batch tests done by Wilson at PNL, where spent fuel was put effectively in a beaker or container and let to dissolve and monitored over time of months to years. And, at Argonne National Lab, the drip testing, where spent fuel pellets had water slowly dripped on them over time for up to a period of nine years, almost a decade.

These data sets represent very nicely empirical, very nicely applicable empirical data for what we should expect from the evolution of spent fuel in our system. The data points are dissolved concentrations of neptunium in those tests. They are not necessarily corresponding to controls by a solubility limiting phase. But, the mechanisms in these tests are all expected to be mechanisms that would apply in the system that we will have, which is dissolution of spent fuel.

The lines on here, the dark line is the NpO_2 model for solubility limits, showing with the uncertainty band around it by the dashed curves. And, the data sets themselves give us a window of insight into the time frames that we have involved in expectation of what kind of times do we have for NpO_2 to precipitate and form.

In almost ten years of testing, these concentrations have not even risen to the level of the solubility of this phase calculated at 25 degrees C. Our calculation is consistently higher than any of these observations, which suggests that we'll have reasonably long times to rise above here and super-saturate that phase, and allow it to precipitate, possibly years or possibly even decades.

In addition to the mechanisms that are occurring in here, a number of recent studies indicate neptunium retention in uranyl solids. The mechanism is not quite certain at this point, but this may offer some explanation to where some of the neptunium might be going, or possibly could go in our system.

The project has done additional studies on neptunium, both in the commercial spent nuclear fuel pellets from these tests, to try to characterize mechanisms, and also to look at specific formation kinetics as a function of temperature for direct precipitation of NpO_2 from solution. I'd like to talk about those now.

These additional studies, the data were supplied from Jim Kinane and Art Gillis and Jeff Fortner from Argonne National Laboratory, and this shows an image of one of the cores taken from a spent fuel pellet. The spent fuel itself is shown in the pink color. The bluish color here is actually the alteration phases, and the corrosion front is right here where this arrow is pointing. I have two diagrams here of analyses, essentially perpendicular to that corrosion front, not in exactly the same location, but going across that.

The spent fuel is effectively to your left of this point right here, as I've lined these diagrams up, and the alteration phases are to your right. And, what we have seen is the x-ray absorption spectroscopy shows the neptunium is tetravalent in the fuel matrix. So, that, we're starting with the reduced form of neptunium to begin with.

Also, looking at this plot, you can see that the neptunium to uranium ratio is peaking just at that corrosion front before dropping off in the mixed valence region, where the uranium transitions from tetravalent from the UO_2 to hexavalent in the uranyl phases shown in red here.

In addition, both neptunium, shown in blue, and plutonium in green, peak in their concentrations right near that corroding surface. And, all of these results indicate that there's evidence that the redox conditions act as corrosion front and near that front are controlling the behavior of neptunium. And, in fact, the neptunium itself may be remaining within the fuel as a possible solid solution of NpO_2, within the UO_2. The structures are very similar.

Additional studies on the next slide are for precipitation from neptuneal pentavalent solutions, kind of after the Roberts, et al. study in 2003, which was done at 200 degrees, and what I'm showing here are two diagrams of x-ray absorption spectra for solids that have been collected from these experiments. These are all preliminary from Argonne National Laboratory. From 200 up to 280 degrees C, the solids are shown here, and for comparison, the tetravalent neptunium standard and the pentavalent neptunium standard, and you can see they correspond very well to the characteristics of the tetravalent species. So, we've got tetravalent solids forming in about three weeks at these temperatures.

However, at 150 degrees C, and I believe this is after about a couple of months, the solids in that system shown here in red have a mixed signature between both the tetravalent and the pentavalent species themselves. So, the expected slower kinetics in this system at lower temperature are being observed, and the work is ongoing to address both homogenous and heterogeneous precipitation at the lower temperatures, and to develop the temperature-dependent rates for these. If these turn out to be favorable, that it might allow us to apply this constraint for NpO_2 across the board, even in the simple parts of the system.

So, I want to switch gears here now, and go onto the next slide, and talk very briefly about the Science and Technology source-term targeted thrust projects, a little bit of overview, a few examples that are extremely relevant to what we are talking about here today.

Next slide, please. The prime directive that was focused on for this is that the research program is focused on the changing conditions over time, identifying the critical processes in each time interval, with attention to the radionuclides that are the major contributors to dose. So, they have the appropriate focus, and, in fact, both Professor Rodney Ewing and Dr. Mark Peters, University of Michigan and Argonne National Laboratory, have put together a very well integrated set of collaborative projects across six national laboratories, seven universities, with international collaboration with the European community also.

Next slide, please. There's four primary areas of research, spent nuclear fuel dissolution mechanisms and rates, formation and properties of uranyl secondary phases, including neptunium incorporation, waste form/waste package interactions, this is more in-package chemistry, and transport of radionuclides through those materials, and then integration of in-package chemical and physical processes. There's been a couple of recently funded modeling studies. Carl Stefert at Lawrence Berkeley National Laboratory, and Udo Becker (phonetic) at University of Michigan just got funded this past year to work on some of this.

I'm going to talk three examples from these three areas, starting out with spent fuel dissolution results next. These data are from Pacific Northwest National Laboratory under the direction of Brady Hanson and co-workers. And, what we're looking at are low pH single pass flow-through testing on commercial spent nuclear fuel dissolution. This is very low pH, pH of 2 for the diagrams on the left, pH of 4 over here on the right, where we have plotted the dissolution rate as a function of time in the top plots, and the cumulative release fractions as a function of time in days on the bottom plots, based on analysis of these constituents in the effluent.

A couple of things to note. In the top plot, you can see that the dissolution rate drops as a function of time in this far from equilibrium dissolution test. That's quite important. In the bottom plot, you can see that the dissolution test goes to about 90 per cent of the entire sample dissolved out here at 120 days, based on looking at the uranium coming out. It's bending over, based on this dropping of the rate.

The really, really interesting thing for me from these data, if you look at both bottom plots, the release of Technetium 99 is much, much lower and it's below 20 per cent in all instances out here, all the way out at 90 per cent of the fuel reactor. This may reflect technetium being bound up in the epsilon phase, which is a separate metal particle phase in commercial spent nuclear fuel containing rhodium, ruthenium, technetium, I think molybdenum also, and some palladium. Those particles tend to be extremely resistant to dissolution, and may represent a mechanism for containing the Technetium 99.

LATANISION: Another question.

SASSANI: Sure.

LATANISION: Are these dissolutions oxygenated?

SASSANI: These are done in equilibrium with the atmosphere, I believe.

LATANISION: So, they're oxygenated.

SASSANI: Yes.

KADAK: Do you put a pellet in a beaker of water, or something?

SASSANI: These are nitric acid solutions. These are column tests where they have liquid flowing through the material in a column, and then coming out the other end and being sampled for analysis for these constituents.

KADAK: How does that relate to the repository?

SASSANI: Well, these are representative of the far from equilibrium dissolution. This is not meant to actually represent the repository overall. It's really focused in on the matrix degradation rate. The far from equilibrium oxidative dissolution of the UO_2, the first step in the process, as one of the major constraints on how does this alter as you react through time. It's not looking at alteration products. This is trying to get after what's the highest rate at which these things can be dissolved as the fuel oxidatively dissolves. So, these are full dissolution tests.

In fact, you can see on this plot that some of the plutonium looks like it's also remaining behind. This may reflect some plating out of plutonium dioxide on the fuel surface, which in these types of tests, you don't want that to happen. You want to get the far from equilibrium rates for the dissolution of the fuel, which is one end of the spectrum that the system is going to analyze. Look at the far from equilibrium dissolution, compare it with other constraints, which are based more on approaches to equilibrium, precipitation of the uranyl phases, precipitation of other phases, like plutonium dioxide or neptunium dioxide.

KADAK: Here's my problem. I'm trying to figure out how does this fit into understanding how spent fuel in casks or waste packages in the repository environment will dissolve or degrade. Tell me how this helps?

SASSANI: Well, this is a very specific piece of the puzzle which allows us to define what the oxidative alteration rate of the UO_2 can be in an oxidizing environment. This is basically the conversion rate to--this is the highest level of conversion rate you could have to form other uraneal phases, because UO_2 itself is oxidatively unstable.

KADAK: Go ahead.

GARRICK: I guess one way of following up with Andy's question, given that technetium is a dose contributor, especially in the earlier years, a few thousand, how does this relate to the possibility of trapping technetium in the degradation of the waste package?

SASSANI: Well, if in fact the technetium is bound within a separate phase, this epsilon phase, in these metal particles within the fuel itself, we currently represented as distributed throughout the UO_2 matrix, which the matrix dissolution is represented by this right here, if it's actually bound up in a separate phase, then in fact that phase, based on some other observations, may undergo dissolution at a much, much slower rate and, therefore, the technetium may be constrained to come out at a much lower rate. This is all fairly preliminary information.

GARRICK: Okay.

LATANISION: Just one additional question.

SASSANI: Sure.

LATANISION: These oxides are going--their solubility is going to be a function of both the pH and the oxidizing conditions and the potential, just as a metal would be. Do you have equilibrium diagrams for these?

SASSANI: I believe we do for the neptunium system and the plutonium system and the uranium system. These tests, in and of themselves, are designed in such a manner that the concentrations in solution, because of the flow-through application, are not high enough to actually saturate with any of those phases. So, in these types of tests, you don't want other phases to form. You want to see what's the forward disequilibrium rate of dissolution.

LATANISION: I'll buy that. But, I'm thinking back to Andy's question. Depending on what conditions arise in a compromised--these conditions may or may not be appropriate.

SASSANI: No, that's correct. That's why at the beginning of the talk I was saying the dissolution rate models are used in conjunction with the solubility limiting models to see what's the controlling factor. In those cases, that's where the solubility limits would kick in and say if you degraded the fuel at these rates, and your fluid was not moving, and you weren't taking it away fast enough, that coupling to the transporter, if it's not being transported away fast enough, the concentration rises to a point where it hits one of those solubility limits and is constrained there to reflect precipitation of those phases. So, those have to be taken as two sides of a coin almost.

The next slide is another set of results. You may have seen this before. This is studies of pentavalent neptunium incorporation into uranyl phases form. It's work by Burns, et al. at Notre Dame University, where they have synthesized both uranyl phases that have interlayer cations and those that do not, and have found that the uranyl phases with interlayer cations actually do take up the neptunium from solution reasonably in proportion to what it is in the initial solution. And, those that don't have interlayer cations, which should facilitate the chart balancing, do not seem to. And, this washes off very readily.

The major point here is not so much the quantitative aspect of it, but it's one of the first demonstrations of the theoretically based estimates from 1997 that were done by Burns, et al. where these minerals actually take it up. What they synthesized here, though, were fine grain masses of material, and, so, they were a little unsure of exactly what the mechanism was, if it was actually in the crystal structure or not.

And, so, they also did some single crystal studies shown in the next slide, where they have synthesized becquerelite in this system, which is calcium/uranyl phase, and there's a photomicrograph of the becquerelite single crystal here. And, then, they analyzed this using laser ablation ICP mass spectrometry, which is shown, the track of the laser ablating, essentially vaporizing the crystal is shown here, and the analysis is shown here with counts versus time. Prior to the laser being turned on, there are no background Neptunium 237 counts. Once the laser ablation starts, you can see about 800 parts per million were measured to be within this crystal, and it demonstrates nicely that that neptunium is reasonably homogeneously distributed throughout the crystal here.

In addition to these studies on spent fuel and incorporation of neptunium, there's also in package work shown on the next slide done at Sandia National Laboratory by Pat Brady, Jim Jergen and Ken Korkup (phonetic) at Pacific Northwest Laboratory, which is evaluating that once we have a breached package and the basket materials begin to corrode, what might these iron oxides do in terms of controlling the redox conditions inside the package, and in particular, maybe occluding the gas phase. You can see that the minerals equilibria for the iron minerals are much more reducing conditions than even the oxidative alteration of the UO_2 itself, and well below atmospheric conditions that are evaluated.

Within these types of systems, they're looking at both the generation of the corrosion products, but they're also looking at interactions of neptunium with those, and technetium via rhenium as an analog, the rhenium ion as an analog for technetium, and they're finding both that neptunium is reduced onto magnetite surfaces, and they are actually finding that a lot of the rhenium is removed from solution when they interact it with these types of corrosion products.

And, the next slide is just a picture, scanning electron micrographs of some of those corrosion products from their coupon studies at low and high magnifications in very dilute effectively tap water, J-13 type solution. And, the only thing to take from here is that they see various types of morphologies of these products, depending upon water chemistry, and sometimes getting silica precipitants also.

These studies are more about the in package chemistry and transport pathways for radionuclides, and the transport aspect is what Rob Howard will cover in his presentation.

That's where I'm done.

GARRICK: Thank you. Questions from the Board? Yes, go ahead, Bill.

MURPHY: That was a very interesting talk, and I'll try to stop asking questions at some point. I am curious about your Figure 6-3 on Page 11, first of all, where you show dissolution rates. And, there's quite a substantial scatter, which doesn't surprise me. I understand these are hard to determine, and the surface areas are difficult to characterize, and I'm just curious in your judgment, whether you think this rather broad range is a consequence of uncertainties in measurements, or is it a function perhaps of other independent variables controlling the rate besides O_2 and temperature, for instance, pH, or other aqueous species?

SASSANI: Well, that's a long question. I'll try to stick to it a little bit. You know, I think if you look at a lot of these studies, the primary variables, things like the temperature aspects, the oxidation state of the system, in particular, you dissolve carbonate under ablating conditions by a very large controlling role. In particular, the carbonate because of uranyl carbonate complexation at very high pH. The uncertainty in these data of course are included in the functions that are derived from these to try to capture that uncertainty. These are shown per unit area.

Surface areas, in fact, for the overall rates are explicitly in the rate relations, and surface areas can vary over a fairly wide range of values. The surface areas that are used vary over about two orders of magnitude for these dissolution rate studies. These reflect $10^{-6\ 1/2}$ meters squared per gram of material for the surface areas there. Those are derived from backing them out of dissolution rate studies by knowing the initial surface areas, and measuring the constituents as they are released. They are also derived from characterization of the geometric surface areas of the fuel. But, of course, the geometric surface area may not be the full story if there are cracks and grain boundaries that play a role in the dissolution rate process, to a large extent.

If you look at things like teller VT (phonetic) measurements of surface areas, nitrogen gas measurements, those surface areas can be even a couple orders of magnitude higher. The surface area effect itself, and the uncertainty in that, the range of that that's used is, for the model itself, one of the larger controls on where these--it goes directly up and down in time frame relative to those.

The actual measurements, some of it is possibly characterization of the surface area, particularly for dissolution tests, you can characterize that surface area when you begin, but it's very difficult to characterize the change in that surface area through time in the test. And, in fact, a number of folks have written some methodologies for getting around that, in particular, some of the PNL co-workers, Brady Hanson, looking at the actual cumulative release of material and trying to normalize the rates in a different manner.

But, in terms of the chemical environment, there might be some effects also from radiation interactions and radiolytic processes, which are sort of rolled in here implicitly from the spent fuel tests, but there's some uncertainty in how you deconvolve that out explicitly, and whether or not we're seeing in a lot of our testing a large effect of this radiolytic process that we're not explicitly representing.

MURPHY: That addresses some of my questions. I guess a rather basic concern is that given this rather broad range of maybe an order of magnitude, or more, is it possible that it reflects differing sets of conditions, and how can you be comfortable with this particular regression plane through the data as a conservative estimate of the rate?

SASSANI: Well, again, these are far from equilibrium conditions. Now, some of those other conditions which might be important in the specific experiments, of course, would be the water chemistry, and in the discussion of the SPFT tests in the S&T area, I talked about do you want to run these experiments in dope form phases on the surface, which would (a) occlude surface area and effectively remove a portion of the mass from reaction in your test. There might be some of that. That's a really hard question to answer.

The types, again, the types of lifetimes, we're getting to the fuel range from hundreds of years to thousands, if not, an extremely long lifetime. I think Tim McCartin referred earlier to we aren't taking an enormous amount of credit for the alteration of the fuel, the front end alteration of it.

MURPHY: I'm curious if the relatively new Hanson, et al. data that you showed in Figure 21, how they compare to these data for rates, and I can't quite compare, the units aren't given in the first figure.

SASSANI: Well, I haven't seen the explicit comparison of these in terms of the actual dissolution rates, and I'd have to sit down and talk with Brady about that to really comment directly. I don't know if Brady has done that comparison or not. I suspect, from talking with him in general, Brady has a very strong feeling from not just these studies, but a lot of studies that have been done on spent fuel, that the degradation rate of spent fuel that we use is probably a bit fast relative to what reality might be.

MURPHY: One additional question concerning Figure 16. You say that neptunium may remain within the fuel while the alteration occurs, and I'm wondering if you attempted a kind of mass balance. Because, it seems to me that there's a substantial quantity of fuel pellet that's been oxidized here, as represented by the blue, and if you would accumulate all the neptunium in that at the background level of neptunium, it certainly would be more than that little peak near the surface, it seems to me.

SASSANI: Right. The results here, and also results in the S&T area, are tantalizing in terms of seeing mechanisms. But, the quantification of these I would say is at a very early stage. In fact, you look at these peaks, and in fact, these are relative concentrations in arbitrary units based on the absorption and spectroscopy that's been done. And, in fact, if you look closely, the neptunium concentrations are a factor, multiplied by .002, and the plutonium .006. So, these are just shown for the qualitative behavior.

I'll say one thing in terms of the cores that have been taken, which were not real easy to do, but these are actually more oriented than the grab samples that were looked at before, Jeff Fortner indicated to me, and I think they discuss it a bit in their report, when they do this coring into these small fuel pellets, the coring device actually wraps some of the bottom material up on top. You can see again here more spent fuel up in this region, and it's unclear probably at this time how much of the alteration phases have been rolled up onto here. So, I wouldn't put too much quantification into that altered zone at this point, and it has not, frankly, yet been done.

I think these types of studies, and these types of measurements, which are not real easy measurements to make, is just the first step in that process.

MURPHY: Thank you.

GARRICK: Okay. Yes, George?

HORNBERGER: So, some of these S&T results you showed, my question is are there plans to move these forward to the TSPA? If so, how do you anticipate that will be done, and over what time period? And, depending upon that answer, how about future results of projects in the S&T program?

SASSANI: The future of the S&T program, I'm not really sure about. Russ could probably answer that better. But, speaking with him, it's not really clear what's going to go on with that at this point in time.

I'll speak to the S&T program as it was set up up to this point, and its intent was actually to investigate and constrain more detailed scientific questions, as opposed to be used for the regulatory process. Now, in fact, that's a good thing, but that's a really basic question to ask, of course, what do you do with this? The project's point of view has been, you know, in fact, if there are results found which would indicate that the project was doing something that was non-conservative, then absolutely, the results would get rolled into a TSPA. I mean, you'd have to go update your models in terms of public health and safety issues.

But, these types of data from the S&T program, from the regulatory standpoint, may never get rolled directly into the TSPA model used for regulatory purposes because they could be pointed to just as demonstrating the point that we're reasonably conservative in the approach, or reasonably cautious. For the regulatory standpoint, that's okay.

Now, from the standpoint of answering detailed technical questions, you'd like to utilize those results also. So, that's where I believe, I mean, which is a long way of saying I don't know.

GARRICK: I have some questions, but I think I'll wait until the next presentation. Andy, you had a question?

KADAK: Yes. I'm trying to get a perspective here of this work and the relationship between the design or analysis of what the repository will really do. What I got from the thrust of your presentation is you are completely focused on trying to understand better the neptunium dissolution rate from a waste package--from a pellet, forget the waste package. We earlier heard you're not taking any credit for the container in which the spent fuel is placed, and yet we also heard this morning that the NRC folks don't want to take credit for the cladding, and you want to take credit for the cladding, and yet you don't want to take credit for the can. Will you help me understand what you're doing in this regard?

SASSANI: I'll clarify a little bit, and Rob may be able to clarify more. But, in terms of the container, there is a lot of credit taken for the container.

GARRICK: No, he's talking about stainless steel--

SASSANI: I'm sorry. Okay.

KADAK: And, you think the cladding is better, apparently, because you're taking credit for it?

SASSANI: There is no representation of the stainless steel liner because it's assumed that once you penetrate the Alloy 22, you have an environment that is potentially corrosive and will corrode reasonably fast relative to Alloy 22 corrosion.

KADAK: Okay.

SASSANI: That's the way I understand it, but I'm not an expert on corrosion.

KADAK: Then, talk to me about cladding then.

SASSANI: Cladding itself, we do have defective cladding included in the modeling. That defective cladding has instantaneously failed upon waste package breach. But, there is a lot of cladding that has not failed, and, in fact, there are cladding models for the corrosion which under the current operation for looking at the tens of thousands of year period, did not produce any other cladding failures. The stainless steel cladding is assumed to be failed. There is no credit taken for it. It's only zircaloy cladding and commercial spent nuclear fuel, and there's a distribution of defective cladding based on looking at the reports and studies of defects in cladding that have been observed when cladding has been moved or handled.

KADAK: So, zircaloy is better in this environment than stainless steel; is that true?

SASSANI: Zircaloy is fairly corrosion resistant. From my understanding, it's pretty much high fluoride environments that attack it, and that's not expected in our case. But, again, I'm not an expert on the corrosion.

KADAK: What we as a Board are interested in, and maybe you could explain when you get up, is how does this stuff actually get out of the fuel pellet into the environment.

SASSANI: That's what he's going to talk about.

KADAK: And, he's going to talk about that? I'm not sure we still got the first part, and that's how we are corroding this fuel pellet to a point where it can get down into the bottom of the can, given the amount of water that we're talking about, and given the dissolution or some kind of a corrosive mechanism. I understand you dissolved it in nitric acid, but I'm hoping that there isn't nitric acid in this bloody repository.

SASSANI: That is just, and I really want to clarify, those experiments are for characterizing properties, not for stainless steel.

KADAK: I understand that. But, my point was how do you take these properties, which I know you're looking at the oxidation potential, but how do you translate that into the repository environment?

SASSANI: Well, there are environment models, which I couldn't go into here today because of the time frame, and those models provide all the chemical parameters that are required to these waste form models, as the repository evolves through time.

GARRICK: You'll get another shot at this after the next speaker. So, thanks a lot. It's time for the next talk.

HOWARD: Good afternoon. I'm Rob Howard with Bechtel SAIC. I want to acknowledge that I did get quite a bit of help from Jim Schreiber on this presentation. He was the lead author for the engineered barrier system, radionuclide transport abstraction AMR, and much of the material that I'm presenting today was derived from that piece of work.

Dave went through this slide already, and I'm not going to dwell on it. We can come back to it if we have questions.

Next slide. Let's talk for a couple minutes about what the key radionuclides are that we're concerned with in the repository. Start off by noting that Strontium 90 and Cesium 137 are the highest contributors to the initial activity in the repository, but they decay off rather rapidly. They both have half lives on the order of about 30 years, and after several hundred years, they are not going to be significant contributors.

Bill Arnold is going to show you a sensitivity study in the saturated zone later on this afternoon that's going to show you some of the effectiveness of the saturated zone with reducing the total repository activity. So, you might want to keep this slide in mind when he goes through that sensitivity analysis.

We also have plutonium and americium, and these two radioelements behave similarly chemically. They're strongly sorbing, and that's important when we talk about transport characteristics. They also are moderately soluble. Technetium 99, we talked a little bit about already today. It's in the GAP inventory. It's fairly soluble under oxidizing conditions, and it persists for a relatively long time. Neptunium 237, Dave went into great detail about its solubility, and it, again, persists for a long amount of time in the repository.

Next slide, please. The uranium isotopes are interesting because they are abundant. They're weakly sorbing. They also have relatively low specific activities. In other words, there's U-238, for example, there's a high-- a large amount of U-238 in the repository, but its specific activity is rather low. Just to give you an idea, I think the activity is about 3.4 times 10^{-7} curies per gram, whereas Carbon 14, for example, is on the order of 4 1/2 curies per gram, and Cesium 137 is on the order of 87 curies per gram. So, we've got a high mass content, but a low activity. We do track all of the uranium isotopes. Iodine 129, we're interested in, has a relatively soluble element. It also has a large fraction in that GAP inventory that Dave discussed earlier.

Next slide, please. The only thing I want to point out on this slide is that we do have some thorium in there that we are concerned about, and that comes from the high-level waste form.

Next slide. I guess this is where I'm going to get some more questions from Andy. Releases from the repository are scenario dependent. So, let's spend a little bit of time going through some of the scenarios and what the modeling implications are for those scenarios.

In the nominal scenario, we have two cases, general corrosion failure case and early failure case. For the first 10,000 years of repository evolution, we don't really see any waste package or drip shield failures due to corrosion mechanisms.

In the early failure case, waste package failure is represented by a poisson distribution, and in that case, the failed waste package, and that's the Alloy 22, and the 316 stainless steel, performs no barrier function. The drip shield is still in place, so you're going to have just diffusive releases out of the waste package, but you're not going to get any barrier performance from that waste package other than the sorptive capacity of the internal materials.

Cladding damage in the nominal scenario is specified by the initial conditions of the repository. There's about 1 per cent of the total inventory that we receive of spent fuel rods is going to be stainless steel clad. We don't take any performance credit for the stainless steel clad. There's a distribution that we get on the initial conditions for the zirclad stainless steel, and that ranges from about zero to 1 per cent failed.

For the igneous scenario, the drip shield and the waste package, neither one of them provide a barrier function. So, the seepage flux that we get into a degraded drift is available to contact the waste form in that case.

The cladding has no barrier function. The high temperatures involved in the igneous case where we assume some magma will contact the waste form, we don't get any credit for the zirclad. We apply basaltic chemistry conditions to the engineered barrier system transport environment, and I'll talk a little bit more about that later.

In the seismic scenario, we've got two cases.

We've got a mechanical damage case and a fault displacement case, and we use the seepage abstraction for degraded drifts in both those cases. And, the mechanical damage case, the drip shield remains intact. It still performs its flow diversion function, so releases from the waste package are going to be diffusive only.

For the seismic fault displacement case, the drip shield loses its functionality when we have an initiating event with an exceedence frequency smaller than 2 times 10^{-7}.

Damage for the mechanical damage case for the waste package, it's a network of tight cracks, they are very tortuous cracks that the waste packages can bang into each other, or bang into the pallet, and get these stress corrosion cracks. No advective flow through those cracks. It's going to be diffusive only. And, the fault displacement case where waste packages that are sitting on a fault can be disruptive, we can get what we characterize as a crimping of the waste package. And, that damage area is uncertain, and it ranges from zero damage to the total surface area of the lid.

You can also get localized corrosion in the seismic fault displacement case, but it's also going to, keep in mind, that the thing has already been damages by the crimping conditions. We do have some cladding performance for the seismic cases, but it's going to be a function of the peak ground velocity. So, it varies with the ground velocity. And, we adjust the temperature and relative humidity for collapsed drifts in both seismic cases.

Next slide, please. Key transport processes within the EBS. We consider transport of dissolved radionuclides, transport of radionuclides that are reversibly sorbed to three types of colloids. We have groundwater colloids. We have corrosion product colloids. And, we have waste form colloids in the high-level waste glass.

We also consider transport of radionuclides irreversibly sorbed onto iron oxyhydroxide colloids, transport of embedded colloids, that's plutonium and americium that are irreversibly attached to those smectite clay-like waste form colloids. And, irreversible sorption of radionuclides, this is just plutonium and americium only, onto stationary corrosion products.

Next slide, please. Okay, in order to solve the mass transport equations within the engineered barrier system, we discretized the EBS into four different domains. We have a waste form domain that's going to consist of the commercial spent nuclear fuel, or the high-level waste glass. We have corrosion product domain, which includes the damaged area to the waste package. The invert, which is crushed tuff, and the EBS/UZ interface domain that Ernie Hardin spoke about already today, that establishes the boundary conditions for the UZ flow and transport model.

In order to solve the mass transport equations, we've got to specify water volume, saturation, porosity, diffusive area, the diffusive path length, diffusion coefficients, what the advective flux is, if any, and these can vary by the waste form type, whether it's commercial spent fuel or high-level waste glass, or whether it's in a dripping environment or non-dripping environment. Another way to think about that is whether or not that drip shield is intact once the waste package is damaged or not.

Next slide, please. For commercial spent nuclear fuel, the way we conceptualize this, the CSNF waste form domain represents the breached and axial splitting of the fuel rod that has degraded into the rind and the alteration products from uranium oxide. And, Dave showed you a couple slides on that.

The rind is assumed to saturate quickly and completely for both dripping and non-dripping environments at temperatures below 100 degrees C. We don't have any releases at temperatures above 100 degrees C.

There's a continuous thin film that we assume exists at all temperatures below 100 degrees C. And, so, that always creates diffusive path length out of the waste form.

The rind water volume is going to be a function of the time dependent fraction of the degraded waste form, its porosity and the saturation.

The rind porosity is epistemic. I won't say any more than that.

The amount of each radionuclide mobilized is going to be a function of the rind water volume, the waste form degradation rate, and the dissolved concentration of that radioelement in water.

There are no colloids present in the commercial spent nuclear fuel waste form domain.

Next slide. The diffusive area is going to be the product of the fuel rod length, the split opening for those rods that have failed cladding, and the number of failed rods that are in the waste package. That's going to vary amongst the different scenarios.

The diffusive path length to the corrosion products is the time varying thickness of the rind, and the diffusion coefficient, we start out with free water diffusion coefficient, and we modify that by porosity and saturation, or water content, using power law to form Archie's Law, and based on experimental data from Concha and Wright in the early Nineties, and some work we did I think out at Low See Road (phonetic) in the late Nineties.

Advection is going to occur in seismic fault displacement and igneous intrusion, so we specify what the advective flux is using the flux splitting algorithm that Ernie went through this morning.

Next slide, please. Co-disposal waste form. The waste form domain here represents the degraded high-level waste glass that forms the clay like alteration product. It saturates quickly. The high-level waste glass is hydroscopic, and that's for both dripping and non-dripping conditions. Again, a thin film exists at temperatures below 100 degrees C.

The rind water volume is a function of the time dependent fraction of the degraded waste form, but we set the porosity and saturation are deterministic values, in this case, not uncertain values or variables. The amount of each radionuclide mobilized is, again, going to be a function of that rind water volume, the waste form degradation rate, and the solubility of the radioelement in water, just like it is for the commercial spent fuel. The alteration of the high-level waste glass does include embedded plutonium and americium. That is different from commercial spent fuel.

Next slide. The diffusive area is the sum of the surface area of the five high-level waste glass logs. That's what we consider. So, Dave showed you a slide of the co-disposal package that's got either four or five logs in it. We use the surface area for the five log case. The path length is the time varying alteration rind thickness.

Diffusion coefficient is handled essentially the same as we did for commercial spent nuclear fuel, although we do reduce it by a factor of 100 for radionuclides that are bound to colloids, and that's based on the Stokes-Einstein relationship.

Next slide, please. Corrosion product domain is the portion of the waste package that has degraded. It's all the internal components, like the basket materials that surround the commercial spent nuclear fuel, and any other components that are internal to the waste package, other than the spent fuel or high-level waste glass.

The degradation of the internals results in corrosion products. They can be a large mass of stationary corrosion products, or they can be colloidal in form. The mass of the corrosion products is going to be a function of the mass of the stainless steel and carbon steel that's available, along with their respective degradation rates. Just to give you an idea, there's about 5,000 kilograms of carbon steel in these commercial spent nuclear fuel waste packages, and on the same order for stainless steel, and that includes that inner shell that was causing so much confusion today.

We don't account for the consumption of water through chemical reactions in this domain. The water volume is going to be the product of the pore volume, and the saturation of the corrosion product mass. The pore volume is a function of corrosion product mass, porosity, and the corrosion product's density.

GARRICK: Is there a flow rate at which accounting for consumption of water would be important? We are talking about very low seepage, and very low flow rates.

HOWARD: I'm trying to think of the best way to answer that. I see Ernie on the edge of his chair.

HARDIN: Ernie Hardin, BSC. He's talking about a degraded waste package where the moisture got in through the gas phase. At least that's the conceptual model. In the case of advective transport of liquid through a breach into the package, you would not be using this particular mode of transport.

GARRICK: All right.

HOWARD: I think that not counting for the consumption of water, there's been some sensitivity studies that I think Jim Schreiber and others have done that looked at this, and you could get upwards of 4,000 or 5,000 years, or even more, of delay if you did account for it. But, we haven't incorporated any of those alternative conceptual models.

GARRICK: Okay, thank you.

HOWARD: Water volume is a product of the pore volume, and the saturation. Pore volume--actually, we went through this already.

Next slide. The effective saturation in commercial spent nuclear fuel is a little bit different. It's based on absorbed water and is a function of relative humidity, and the specific surface area of the corrosion products for non-dripping environments. For dripping environments, for those cases where you have advective flow into a waste package, we just set the saturation at 1.

The diffusion coefficient is handled essentially the same way as we do for the waste form domain.

Next slide, please. Diffusive path length is epistemic and it ranges from the waste package outer shell thickness to the radius of the waste package, whether it's co-disposal or commercial spent fuel.

Sorption. Sorption is important. There's a large mass of iron in here, the 316 stainless steel. Iron content is about 63 per cent. The A-516 carbon steel has an iron content upwards of 97 or 98 per cent. So, there's a lot of iron available here, and we account for that in sequestering radionuclides. So, we have sorption, irreversible sorption to stationary corrosion products, and that irreversible sorption is based on some field observations and experiments that we have done where less than 1 per cent of these radionuclides actually desorbed in the column experiments that we did over a period of several months. So, we have irreversible sorption on the stationary corrosion products, reversible and irreversible sorption onto the mobile corrosion products, and reversible sorption onto groundwater and waste form colloids.

GARRICK: So, you don't take credit for the stainless steel for corrosion resistant, but you do take credit for it as a corrosion product sorption, absorption bed.

HOWARD: Yes, sir, we do. I should have been a little clearer on that. That is correct.

Next slide. The invert domain. I'll touch briefly on this. The concentration is going to depend on the solubility limits. We do check for solubility, as Dave mentioned earlier, in the invert. Colloidal stability, which is going to be a function of pH and ionic strength, the transfer between the corrosion product domain and the invert, so, you know, whatever that difference is in concentrations is going to control the diffusive transport, and the boundary concentrations at the invert/UZ interface.

The advective source fluxes are those ones that were defined earlier by Ernie in the EBS flow abstraction. They generally dominate diffusive releases in the invert domain. We don't account for the transport path along the waste package pallet, so that is a simplifying assumption that we made.

Next slide. Diffusion coefficient is handled essentially the same as they are elsewhere within the EBS.

The diffusive length is simply the average thickness of the invert, and the diffusive area is just the product of the invert top surface and the length of the waste package.

Next slide. EBS/UZ interface domain. Ernie touched upon it earlier. We use a semi-infinite zero-concentration boundary condition and we apply that approximately three drift diameters below the invert. And, we did sensitivity studies to make sure that we were okay there.

The near-field UZ interface is modelled as a dual continuum consisting of overlapping UZ matrix and UZ fracture continua, so that we can account for releases into the fractures and releases into the matrix. And, I think Bruce is going to touch upon that just a little bit.

Next slide. Sorption. Reversible sorption onto stationary corrosion products is not considered. I said earlier that we had some laboratory experiments that suggested less than 1 per cent actually desorbs. Sorption onto corrosion products in the invert path is not considered. So, we do have sorption in the invert on tuff when we use the Kds for crushed tuff, the same Kds that are used in the unsaturated zone. But, any of the steel plates and rails that are in the invert that corrode, we don't account for that because of the high uncertainty of whether or not radionuclides will actually cross that transport path.

Irreversible sorption rate constants for plutonium and americium on corrosion products, whether they be stationary or mobile, is epistemic and it depends on Goethite sorption density. The pictures that Dave showed you earlier was a mixed assemblage of different forms of iron oxide, of hydrous ferric oxide sorption site density, Goethite surface area, the fraction of total iron oxide that is Goethite versus other species, and the amount of high-affinity hydrous ferric oxide sites that are available.

We use Kds for reversible sorption onto crushed tuff, and they're again the same Kds that we use for the unsaturated zone.

Next slide. Colloidal transport. The sorption coefficients for reversible radionuclide sorption for all three types of colloids, we use the Kd approach. For the irreversible sorption, we use the rate constant, just like we do for stationary corrosion products.

For the irreversible sorption of plutonium and americium onto the waste form colloids is going to be a function of ionic strength and pH.

Some processes that we don't consider when we are evaluating colloid transport in the engineered barrier system include physical filtration. We don't take any credit for retardation at the air/water interface, interaction with organics, or settling.

Next slide. To sum it all up and bring you back to the different scenarios, for the early failure modeling case, releases are going to be dependent obviously on the number of early failed waste packages. Again, it's a Poisson distribution. In-package solubility limits for uranium and neptunium. Irreversible sorption onto stationary corrosion products, and the diffusion characteristics of the waste form and waste package.

For igneous intrusion, it's a little bit different. It's the number of waste packages and drip shields that are disrupted by that event. The solubility limits are still important. Irreversible sorption on the stationary corrosion products, and in this case, we may see some more releases because we have that advective flux. The diffusive characteristics aren't that important to us, even that factor of 100 that we reduced the diffusion coefficient by, doesn't seem to matter. And, so, colloidal concentration limits are going to be important for releases out of the EBS in the igneous case.

For the seismic ground motion case, it's essentially the same characteristics as it is for the early waste package failure cases.

All right, Andy?

GARRICK: Let's start with David.

DUQUETTE: Duquette, Board.

I thought I understood what was going on, but now I'm a little confused. Can I walk you through what I think you said, or at least what between the two of you have said? And, that is, in order for the fuel to swell and to cause splitting of the cladding, I have to get water inside the cladding. So, I have to penetrate the cladding somehow, unless it's a damaged cladding.

HOWARD: Right.

DUQUETTE: To get the water inside. And, as long as I don't get water inside, I will not have splitting of the cladding; is that correct?

HOWARD: That's correct.

DUQUETTE: Okay. So, somehow I've got to get the water through whatever you use for the outer casing, whether it be stainless steel or C-22 or some of the new alloys that are being looked at, the water then has to get down onto the cladding surface, has to corrode the cladding surface, and has to enter the cladding, and then it causes the fuel to swell, and everything goes backwards, that is, it now allows the fuel to exit. Is that correct?

HOWARD: That's essentially correct.

ARNOLD: Excuse my interrupting. I asked the question this morning how would the cladding fail in the first place, and the answer came back imbrittlement, seismic activity, various things that had nothing to do with corrosion. There were physical effects.

HOWARD: Yes. And, as I said earlier, it's mainly mechanical damage, either it's the cladding is damaged as it arrives at the repository or--

DUQUETTE: Then, the real question I have is does your model take into account the fraction of bundles, if you will, where the cladding is actually damaged?

HOWARD: When it's received?

DUQUETTE: Yes.

HOWARD: Yes, we don't take credit for the stainless steel, but there is a distribution that we use, it's on the order of zero to 1 per cent that we sample on for the failed cladding, failed fuel that is received at the repository.

DUQUETTE: So, barring igneous or seismic events, it's only about 1 per cent of the clad fuel packages are expected to have failed beforehand. The others probably will not fail at all because these are zirconium alloy is corrosion resistant.

HOWARD: For the 10,000 year cases that we've evaluated. I'd think you wouldn't want to say that for--

KADAK: There's another caveat though. Even if it does fail, the temperature of the pellet has to be below 100 degrees to make any problem relative to corrosion. That's what I also heard you say.

HOWARD: At temperatures above 100 degrees. If the cladding is split, it degrades instantaneously.

KADAK: Instantaneously. But, what about this 100 degree thing, you had a chart that said if it's above 100 degrees, you don't get something going on. I forgot what it was.

HOWARD: Dave's degradation chart? It was water, right.

HARDIN: Would be no transport.

KADAK: So, it degrades, but doesn't transport if it's above 100 degrees?

HOWARD: That's correct.

KADAK: So, we've got a lot of boundary conditions on this stuff actually getting into what we would call the environmental portion of this problem, which is the movement into the water?

HOWARD: Yes, sir.

DUQUETTE: Maybe we're going to hear it this afternoon. Duquette, Board. But, so far, I haven't heard anything about how much of this stuff is going to reach the surface? How much of the radionuclides will reach the biosphere, given 1 per cent of the clad packages will have failed beforehand, only those will split, assuming that water gets into them, and then that most of it's going to be tied up with colloidal absorption?

HOWARD: Again, that's going to be scenario dependent. It's also a TSPA question. We don't have TSPA results.

DUQUETTE: Okay.

GARRICK: Ron?

LATANISION: Latanision, Board.

If we could go to your Slide 6? I just want to make sure I have the perspective clear. In the nominal case in terms of the waste package, am I reading this correctly? You have general corrosion, microbially influenced corrosion and stress corrosion cracking. Are they occurring, but they are not penetrating, so you don't have a failure, or are they not occurring?

HOWARD: They are occurring, but they're not penetrating in 10,000 years.

LATANISION: Okay. And, in the case of a seismic event, you say stress corrosion cracking damage--

HOWARD: It's a function of the peak ground velocity.

LATANISION: Now, why do you think stress corrosion cracking occurs in that instance?

HOWARD: Because the waste packages can bang up against each other end to end.

LATANISION: I mean, how does that implicate stress corrosion cracking as opposed to just mechanical failure of the packages if they're banging into one another? Where does stress corrosion enter into a seismic event?

HOWARD: Well, you have residual stresses in this case, you know, we stress relief the waste packages in their initial conditions. So, in this case, we have now induced residual stresses, which can allow for stress corrosion cracking to occur.

LATANISION: So, you take the position that the fact that these packages are banging into one another does not cause them to fail because of mechanical reasons, but it may in the presence of a corrosive environment?

HOWARD: That's correct. And, we assume that corrosive environment is there and they do crack.

LATANISION: Okay. Yes, that's a good point. I mean, stress corrosion cracking phenomena are called delayed failures because they don't occur instantaneously. There's an initiation and a propagation stage. But, this is obviously part of the conservatism of the process; is that it? I mean, I would not have thought of stress corrosion cracking in a seismic event as being a very realistic likely probability, given the time element, the kinetics that are involved in the propagation, the initiation of propagation of the stress corrosion crack.

HOWARD: Right. It will take some time to propagate, but on repository time scales, you know, it's important to note that there is a lot of aleatory uncertainty in that seismic event. We don't know the timing of the event, when it's going to occur.

LATANISION: One other point. On the next item down, waste package damage due to localized corrosion. The comment there is that it's very unlikely to occur because the drip shields function, but I thought we heard from Ernie earlier today that condensation may actually occur. And, why is that not--

HOWARD: That would be distilled water.

LATANISION: Right. Well, distilled water produced from, you know, the constituents in the water phase, whatever they carry along with it would be carried along with the water, and either the dust that's on the surface of the canisters.

HOWARD: Okay. Ernie?

HARDIN: Ernie Hardin, BSC.

The dust has high nitrate, and there's no reason why the distilled water contacting the dust would go to acidic pH. So, we will not see localized corrosion under those conditions.

LATANISION: Okay.

GARRICK: Howard?

ARNOLD: I'm frustrated by the answers on the cladding. Arnold, Board. I got the impression this morning that you assumed that all the cladding was damaged somehow.

HOWARD: We don't assume that. I think you're talking about the NRC's presentation, Tim McCartin?

ARNOLD: Yes.

HOWARD: Yes. Tim I think could speak to that.

ARNOLD: He did mention that you all had different assumptions. That's right. I do recall that.

The other issue with the stress corrosion cracking, a seismic event, as has been said, is going to be a short time, unless it leads to stress because things are leaning against each other, or something. I don't see how it can be a factor in stress corrosion cracking.

HOWARD: We believe it will lead to residual stresses and it will dimple these waste packages. They can bang up against each other, and they could bang on the pallet. I think Mark Board went through a presentation back in November where that was covered.

ARNOLD: Just one remark. The 1 per cent failure was used as a design specification on our part to design clean-up systems for operating reactors. It's very much higher than the actual experience rate.

HOWARD: Right. And, we use a distribution, so we sample on 1 per cent, but it could also be lower than that. So, that's not a fixed parameter at 1 per cent, up to about 1.2 if I recall correctly. And, that's added to 1 per cent, which is stainless steel.

KADAK: Could you go to Slide 21? It was one slide that you said the things you don't consider. Maybe it's the one after that.

HOWARD: Right before that, Slide 20.

KADAK: 20, yes. All the things that you don't consider I think are pretty significant in terms of potentially holding back or retarding the movement of these colloids, which I understand is one of the major vehicles for movement. So, why did you not consider all those?

HOWARD: They were simplifications that we made to--

KADAK: Are they hard to analyze? I mean, everybody knows that there is some filtration going on.

HOWARD: Yes.

KADAK: All these mechanisms I think people know exist.

HOWARD: Yes. And, they are admittedly conservative.

KADAK: And, they could be significant relative to trying to meet even your compliance standard.

HARDIN: Ernie Hardin, BSC.

Any sort of proof testing that you want to do in the laboratory with colloids is, by its nature, time consuming. And, so, this is a simplification that may have saved us a great deal of effort in the lab.

KADAK: Okay, I'll accept that. But, you spent a lot of time in the lab, nine years dripping something on something, that may or may not be important. So, I don't know where you're going with that.

GARRICK: This morning, the NRC gave us a dose profile for long-term performance that included peak dose, and it was obvious that those of us who also have seen the TSPA/LA results, at least of earlier models, earlier versions, could see that the results were very different from DOE's. And, we asked why, and, of course, the NRC model was an illustrative model, and very well caveated, and we are not supposed to read too much into it at this point. But, still, when asked about the basis of the difference, one of the things that was mentioned was release rate.

And, I guess I would like some elaboration on that, because I didn't think release rate was all that important when you start talking about a million year dose profile. But, if release rate is important for the long-term performance, then it seems that the things that we have been hearing about today relative to the source term need to stabilize some.

And, I guess my real question is when are you going to have a source term that you really have confidence in in the kind of terms that we're very interested in, namely realism? And, we know that you have somebody working on the source term that has been asking that question for ten, fifteen years, Rod Ewing, and now he's working on it, so there's no excuse. So, when can we expect to see some stabilization of the source term work?

And, also, comment on the sensitivity of the long-term peak dose, for example, to release rate from the engineered barrier system.

HOWARD: Well, there were several comments and questions in there. When are we going to have a source term that we have confidence in, the answer to that from our perspective, we have a source term that we have confidence in now, and that's for regulatory purpose.

GARRICK: And, that's where we have the chasm. It may be okay, because, in a sense, you've bounded the problem, and you end up with doses that are in compliance. But, there's still this nagging concern that we have about fundamental understanding of the source term. And, the comment was also made by David, I think it was, that unless the S&T work demonstrates that you're not conservative enough, probably won't impact future TSPA's. And, I guess my reaction to that is supposing it reduces the dose by a factor of 100, are you still--obviously, are you still going to ignore it as a part of the source term? The one you would use in a performance assessment?

HOWARD: Yes, I'm better at predicting the future at 10,000 years rather than 10 years from now, which means I'm not very good at all. Total System Performance Assessments are iterative processes and they are going to change over time. And, when the data becomes available for regulatory purposes, and we've got the validated models and it's amenable to incorporation into the TSPA, you know, it's rational and reasonable that we would incorporate it at that time. But, I'm in no position to tell you when that will be.

GARRICK: I guess the question partly is what's the convergence time here? There must be a time--well, you already answered it in one respect by saying that you have a source term now that you're satisfied demonstrates the compliance.

HOWARD: Okay. Mark?

PETERS: Mark Peters, Argonne.

Rod is not here, so I guess--I've only been working on it ten or fifteen years, but I'm the co-lead for the--we've got a program in the S&T Program and source term looking at not only secondary phases, but also a lot of the package type processes Rob just talked about, and we thought about it in terms of a three to four year program, to give you a time frame. We've got the beginning of a task to start thinking about how we would take all the interesting experiments and put into a modeling framework. It might fit into a downstream TSPA as we iterate on TSPA. But, that's the kind of time frame that Rod and I are talking about in the program. Does that help?

GARRICK: That helps. Do you agree with the observation that was made this morning, though, that the differences, these analysis results, one of the major contributing factors is the release rate of radionuclides from the engineered barrier system?

HOWARD: It makes sense to me.

GARRICK: It makes sense for 10,000 years, but I'm not sure, I guess what I'm getting at is how much sense does it make for your peak dose, which we're going to hear later is maybe on the order of 700,000 years.

HOWARD: And, that again may be scenario dependent as well. You know, there's lots of aleatory uncertainty in these disruptive events and the timing of them. And, how that plays out, until the calculations are set up and run, I'm loathe to speculate.

GARRICK: Okay. Okay, any other questions? Yes, Thure?

CERLING: Just as kind of a follow-on, but if we could go to Slide 1? I really like the title of this talk, and it would make a very nice caption to a figure, and I was wondering will we ever see a figure where you could use that as the caption?

HOWARD: Noted.

CERLING: As long as we could remove potential.

GARRICK: Leon?

REITER: Reiter, consultant.

For the TSPA/SR, you convened an international review team to look at it, and they looked at various things, and they had some strong comments, some positive and some negative about what had been done. One of the strongest negative comments they had was about the assumption of a very thin continuous film of water that always exists that you have, diffusive transport. I see it's still there. Have you looked at that at all? And, what's been your conclusions?

HOWARD: Yes, we looked at that, and part of it is that we use the absorption isotherm as a function of relative humidity. I think really related to that is the idea that we don't consume any water in these chemical reactions as these internals corrode. As I said, you know, we've done some sensitivity cases that would suggest that, you know, you could get on the order of thousands of years of delay if you accounted for that, or if there wasn't a continuous film.

Now, again, that assumption is probably more important in the nominal early failure case than it is in the other cases where you can have advective fluxes. So, we have looked at it, but we still haven't found the basis to move very far beyond where we were with those site recommendation models.

REITER: Just is the basis something that you believe it's there, or you can't find proof that it's not there?

HOWARD: It's a recently cautious assumption.

GARRICK: Andy assures me it's a quick question.

KADAK: Relative to the failed fuel, most of the utilities know where they have failures. Now, I'm not sure exactly what they're doing relative to packaging of these failed fuel assemblies. But, my hope would be that they would put it in either some kind of zirconium clad device that is corrosion resistant, so you'd have the same performance, or some other kind of system. Is that going to happen to reduce your 1 per cent number, because if you go all the way back and you work through all the analysis, it says if you can eliminate that 1 per cent, and 1 per cent of whatever the millions of curies there is, that's a lot of curies. So, what are you doing with canned or spent fuel, packaging them any differently to make them more corrosion resistant?

HOWARD: Well, there's certainly no requirement for the utilities to do anything with them before they package them and send them to us beyond what's in their standard contract. And, we don't anticipate any specific designs where, you know, we segregate them and we bundle them up into a special package. That's why we developed the model the way we did. In fact, you know, if the releases are going to be dominated by those from seismic or igneous, I'm not sure that would be a real effective use of resources to attempt something like that.

KADAK: I think it might be worth checking just to see if it would make a difference, because it's clearly a lot of curies.

GARRICK: Okay, thank you. Thank you very much.

Well, we have a little bit of a dilemma. One of the things I don't want to do is push the public comment item on the agenda into the dinner hour. So, what I guess I would like to do is what I say I would never do, and that is skip the break and suggest that people take breaks individually and as needed, but allow us to go forward. So, I think we'll do that, and I guess that brings Bruce to the podium.

ROBINSON: Okay, I'll get started then. Thank you for inviting me to make this presentation on radionuclide transport in the unsaturated zone. Between myself and Bill Arnold of Sandia National Laboratories, we will describe the radionuclide processes and models that are going to be used in TSPA analyses, taking the radionuclides from the base of the repository through the unsaturated zone--that's my piece--and then Bill will pick it up and talk about saturated zone radionuclide transport.

I would like to acknowledge co-workers on this effort from Los Alamos National Laboratory, Lawrence Berkeley National Laboratory, who did a great deal of work in flow modeling, which I believe the Board has heard about in past meetings, as well as the radionuclide transport modeling, and then finally, BSC folks who helped me also prepare the model that I will be presenting today.

Next. This is an outline. I would like to start with conceptual models for transport of radionuclides through the unsaturated zone, talk about those models, what they are, what their technical basis is, and then briefly describe how we go from those conceptual models to a numerical implementation that's suitable for use in the TSPA analysis. Then, I will talk about results, show some representative results, both in terms of what I call a representative case, and then a series of sensitivity analyses to look at parameters that are uncertain that we need to look at in terms of what impact they might have on radionuclide transport. And, those would include flow model parameters, diffusion processes and parameters.

Then, I would like to show two other simulations, one related to whether or not the radionuclides are released from the EBS system into the fractures versus in the matrix. I'll show you the differences that our models predict based on that. And, then, address the question of spatial variability. Are there differences in transport through the unsaturated zone, i.e. releases to the saturated zone, depending on where those releases take place at the repository horizon.

Next. This is a conceptual model diagram. The repository, as we all know, sits within the Topopah Spring welded tuff units. The diagram here denotes that because of low matrix permeabilities and highly fractured tuffs in the repository horizons, we believe the flow to be principally within fractures of the Topopah Spring tuff that the repository sits in.

When you get down below the basal vitrophyre at the base of the Topopah Spring tuff, you then get into the Calico Hills, which is either non-welded and vitric, or altered to the zeolytic alteration. The main difference there being mineralogic differences, but also hydrologic differences in the properties of the matrix, which has an important impact on the flow processes.

What all this boils down to is that depending on where you are beneath the repository, the flow can be fracture dominated, matrix dominated, with a little role played by the fractures, or something in between. So, to capture that in a numerical model, we use a dual permeability model formulation for both the flow modeling in the unsaturated zone, but also the radionuclide transport modeling as well.

The processes included here in radionuclide transport are advection to flow of water, also diffusion, sorption and colloid-facilitated transport. I'll go into those in a little bit more detail, and also show you sensitivity analyses describing what role they play.

Another aspect to consider here is that in the time scale over which radionuclides are likely to be released from the EBS system, we assume that the ambient flow field that would have taken place in some future climate would not have any residual heat effects. There are still heat effects in the rock itself, but the assumption is that the flow fields within the unsaturated zone, for the purpose of radionuclide transport calculations, can be developed based on the ambient flow conditions, and how they change with climate. And, again, the releases in this model can occur either in the fractures or the matrix.

Next. The scientific basis is basically summarized in this slide, and we can get into more detail if you like. I split it into two categories that I'll touch upon. One is fracture versus matrix flow. The other being what are the flow and transport parameters that one uses? Both of those require a description of the scientific basis.

So, in terms of fracture and matrix flow, we had experiments at the field scale in vitric Calico Hills unit at the Busted Butte site several years ago, which we believe is reasonable to conclude from those experiments that matrix flow dominates in the vitric Calico Hills units.

In contrast, more recent studies in the ESF and Alcove 8, Niche 3, confirm in the Topopah Spring tuff, confirm that the process of fracture flow and matrix diffusion in those units is the right model to use as the fundamental model for radionuclide transport in the units at the repository horizon.

Furthermore, Chlorine 36 results have gone back and forth on this program over a period of a decade or more. Basically, at this point, these results suggest the possibility of fracture-dominated transport, especially of conservative species, through the unsaturated zone. So, given the fact that that can't be discounted at this point, and that there is also other data in the course of those studies that suggests the fracture-dominated transport is a reasonable mechanism, we assume that mechanism for some of the units in the unsaturated zone beneath the repository.

And, then, finally, I would just point out that this model that we've developed really is a combined fracture and matrix flow and transport model. I think it's consistent with lots of studies that occurred in various vadose zone sites beyond Yucca Mountain, and I think there's nothing particularly unusual about any of the model assumptions that we make in this model.

Related to flow and transport parameters, the process flow and transport models are either informed in a formal way through direct calibration using data sets such as the water content of the matrix or the matrix potential, pneumatic studies, et cetera, or in a softer fashion by consistency checks, where you do your model calibration and then you check to see whether other data sets, typically chemical data sets, are consistent with those flow model results, and those are the types of data sets that would go into developing the technical basis for this model.

Next slide. Okay, that's the technical basis. Now, how do we go from that to a numerical model that's suitable for use in a Total System Performance Assessment. I mentioned it's a dual permeability model for flow. The particle tracking model that we use in this regard is also a dual permeability model. It accounts for sorption and diffusion via a probabilistic travel time delay type of approach, which I could go into with more time, or if anybody is interested.

There is a full decay chain capability in the model whereby those decay products are tracked through the simulation. Particle release locations are related to upstream in the modeling sense, upstream analyses of the engineered barrier system, which say that I have a given radionuclide release rate at a certain location at a certain time. That is implemented or put into the UZ system via particles that are released across the repository horizon. So, that spatial variability piece is included in the model, as well as whether the fracture or matrix continuum receives the radionuclides.

Then, once particles travel through the system in a TSPA type calculation, it's then turned back into radionuclide mass flux, which is then passed to the saturated zone model.

A word on validation of this model. This is, in a sense, or in reality, an abstraction model. We abstract from the UZ transport process model, thereby inheriting the model validation steps that were undertaken to validate or build confidence in that process model.

The model I'm describing was compared directly to that one, and also to 1, 2 and 3 dimensional calculations for validation purposes.

HORNBERGER: Am I right that there is no drift shadow in any of this?

ROBINSON: There is no explicit modeling of the drift shadow, but as I will show, there are drift shadow-like effects that come into the model for radionuclides if they are released into the matrix.

This is a list of the radionuclides. The reason I put this up is not to run through them all, but basically to say that I'm going to be showing conservative, typical conservative radionuclide result Technetium 99, as well as a strongly sorbing radionuclide Plutonium 242. Colloid-facilitated transport is also included. I will go into that in a little bit of detail in a second.

Next slide. First, advective transport. We use directly the 3D steady state, dual permeability flow fields that are developed from the UZ flow model. The uncertainty in infiltration is captured in a discrete fashion by a median low infiltration and a high infiltration case. The climate-related variability is treated by kind of going along in this direction, unfortunately somewhat out of order. You start with the present day, go to monsoon, and then glacial transition.

So, the uncertainty piece is handled along here by discretely, sampling from three discrete flow fields. The variability in time piece is captured by using flow fields that are simulations of future climate scenarios.

There is this assumption of instantaneous transition of the flow field from one climate state to another. The water table also rises during those future wetter climates.

I just mentioned the uncertainty from infiltration model. We do a lot also of sensitivity analyses in which flow model parameters that really aren't pinned down that well in flow model because of a lack of relevant data to pin them down, we do explore those through sensitivity analyses what impact that might have on our uncertainty in transport.

Next slide. Okay, the transport parameters beyond simply flow are summarized in this slide. We basically are incorporating probabilistic stochastically defined parameters to propagate the uncertainty through the model.

Sorption, that would be the Kd. Bill Arnold will talk more about sorption in his part of the presentation. Diffusion parameters are also stochastically defined. The parameters themselves are based on diffusion coefficient distributions derived from laboratory measurements. It's not just the diffusion coefficient, it's also geometric and other parameters related to the--basically, what it boils down to is the surface area available for diffusion between the flowing fracture and the matrix. That relates to the aperture of the fracture spacing, and other parameters which end up controlling that surface area.

GARRICK: You may have said this, Bruce, but are all the fractures connected?

ROBINSON: No. They are all connected, but they don't all flow. And, some of our parameters relate to basically boiling down to how many of the fractures, of all the fractures that are in there, how many of them are actually flowing. And, that's a model that I will describe in a moment, at least I'll show sensitivity analysis for it that relates to that.

There is also conceptual model uncertainty for how that diffusive transport between the fractures and matrix occurs, as well as colloid transport, which I will describe in the next slide.

This is a schematic that shows the various forms that radionuclides can take. You have radionuclides in the aqueous phase, or sorbed to the rock. But, then, the purpose of this slide is to describe how we capture colloid-facilitated transport.

There are reversible sorption type colloids in which you have a partitioning coefficient which says that a strongly sorbing, typically radionuclide, will sorb the colloids as well as to the rock surface. So, if that occurs reversibly, that can be an enhanced transport mechanism, depending on how the parameters themselves play out. The parameter then is the partitioning coefficient Kc that we talk about, which is the product of the colloid concentration, and the Kd to the colloid, Kd for the colloid and the radionuclide.

The reason this is colloid-facilitated transport is that if that's occurring in a fracture, then during the time when it's sorbed to the colloid, it can't diffuse, effectively cannot diffuse into the rock matrix.

There's also irreversible sorption type colloids. And, what this is meant to denote are radionuclides that are bound to colloids either so strongly or intercollated into a colloid to the point where they basically are irreversibly attached to that colloid. So, then, for those types of colloids, you have advective transport without diffusion into the rock matrix, size exclusion which prevents the transport of those colloids into the matrix if the pore size distribution is such that they wouldn't be able to go into the matrix.

We also have two flavors of this type of colloid, one in which we assume that there is reversible filtration. The colloids move through the fractures. They are slowed via straining processes, but it's assumed to be a reversible process in this model. And, then, we also take a small fraction of the colloid inventory and assume it to be transported without retardation. This is the so-called fast fraction colloids.

Next. So, I'm going to present results now. I want to describe what you're going to be looking at, and how I'm displaying the performance of the unsaturated zone. Basically, I'm showing normalized breakthrough curves. So, what that means is we are looking at the model-predicted distribution of arrival times at the water table.

So, for most of these analyses, we release at time zero, we call time zero the moment at which we release these particles into the unsaturated zone. We do that over the entire repository footprint, except when I tell you that I did a point release. But, for most of the simulations, it will be over the entire repository footprint. We introduce the particles at time zero, and then the breakthrough curves that you will be seeing are the cumulative number that arrive at the water table over time.

So, you can look at this as the response to the system to a step change in concentration spread over the entire repository, and that's the way to think about these results. They are normalized to 1, all radionuclides. So, we're not looking at specific concentrations of individual radionuclides. We're looking at a normalized response for each radionuclide that I'll show.

The other point that I would like to make is that the decay chains are included in the next slide that I'm going to present, but they are introduced both as the radionuclide and also as a parent, which is decaying. And, so, when you see in one of these simulations a breakthrough curve going above 1, it's because it was put in as the radionuclide itself, and also as the parent. So, therefore, that's basically because of the choice of how to perform these simulations, not a feature of the model per se, but just in terms of explaining what you're about to see.

A few of the radionuclide breakthrough curves go above 1, and it's because of essentially the combination of putting them in at the repository, and then also gaining those radionuclides via decay of the parent.

Next slide. This is a series of breakthrough curves normalized. There is the 1 right there. So, it's normalized breakthrough versus time. Glacial-transition climate, this is the climate state that we expect for the majority of the 10,000 year simulation, the mean infiltration scenario. That's mostly what I will show you today.

Walking from left to right in terms of earliest breakthrough to latest, the colloidal special that we're showing here have the earliest breakthrough. And, remember, they are introduced into the fractures, they're travelling through fractures, they are unable to get into the rock matrix, and, so, you get short travel times.

You would have to actually combine this with how much of the inventory is in that state to really get a sense for whether or not that's an important contributor to dose. This is the normalized breakthrough curve.

Conservative and sorbing species have the ability to interact with the rock matrix, and either just diffuse in there and be slowed down that way, or actually sorb to the rock matrix. And, so, as you go from left to right, Technetium 99 is in there, neptunium, and then you get into the plutonium and some of the other species that actually have longer travel times due to matrix diffusion.

That also results in a very much broader arrival time distribution, which probably would filter through and have some impact on concentrations downstream.

So, when these curves don't go to 1, it's because of radioactive decay, which is included in these simulations. So, if the UZ is able to hold up certain radionuclides long enough for decay to occur, then it becomes a barrier for that radionuclide, a significant barrier.

HORNBERGER: The ones that go above 1 are due to, again, the decay chain product?

ROBINSON: Yes. This is this business of putting in the radionuclide as the parent and also the species itself. It's more or less an artifact at how the calculation was performed rather than something that would be in the TSPA analysis itself.

This is a slide that shows sensitivity to the infiltration scenario. Technetium breakthrough versus time for three different scenarios, the lower, mean and upper infiltration scenarios. I want to point out two things here. The first is that a comparison of the red and the black curve shows that the abstraction model being used for TSPA represents or reproduces the model results from the process model, which is a vastly different numerical scheme, thereby providing confidence that the abstraction model is actually valid by inheritance of the fact that the--the abstraction model is valid because the process model is valid, and it compares well to it.

With respect to infiltration itself, this basically shows what we probably knew already, but that the infiltration scenario, uncertainty in infiltration has a very large impact on the transport of all species, including conservative species.

Next slide. This is an examination of a parameter uncertainty related to the flow model, and that is the active fracture model parameter. I mentioned that not all fractures flow, and that there's a surface area available for diffusion between the flowing fracture and the matrix. This parameter controls that. It's an uncertain parameter because we don't have good data to really hone in on what that parameter is, and, so, this type of analysis lets us determine how conservative or optimistic our models are with respect to that parameter.

As a general statement, we do this with a lot of different flow model parameters, and most of the flow model parameters really don't have great--the transport doesn't have great sensitivity to those uncertainties. This is an example of one where we call it basically moderate amount of impact on the breakthrough curve. As you go to greater and greater diffusive areas for release into the fractures, you get less early time breakthrough. The curves typically converge at later times.

So, the TSPA models in the case of this parameter use flow parameters that are on the conservative, but reasonable. End of that.

Next slide. This is a similar look at the active fracture model parameter, but for a sorbing radionuclide, Plutonium 242. It also has impact on plutonium. Other flow model parameter results is that basically, a lot of parameters have kind of a low to moderate impact on these breakthrough curves.

Next slide. I'm going to move on to diffusion at this point. Now, I show breakthrough curves for Technetium 99, the orange, and Plutonium 242 on the same plot. And, what we're varying in the series of three simulations for each radionuclide is the diffusion coefficient. This shows that the diffusion coefficient has a rather large impact on the breakthrough curves. Basically, the higher the diffusion, the more ability the radionuclide has to diffuse into the rock matrix, where flow rates are much lower. They do eventually reach the water table, but with a delayed breakthrough curve. And, those effects, in general, are more dramatic for sorbing species than they are for conservative species.

Now, that uncertainty in the diffusion coefficient is captured in the TSPA model as a direct parameter, stochastic parameter that's varied in the TSPA.

Next slide. This is a slide which gets at the question do we have the right conceptual model for this fracture/matrix interaction. So, I have two sets of curves here. In this case, they are all Technetium 99, but in the one set of curves, they are the curves I showed previously, but with kind of the base case, dual permeability flow and transport model, which basically has one computational grid cell in the matrix for every one in the fracture. So, the gradient that you capture from fracture to matrix is basically--the gradient on which the diffusive flux from fracture to matrix is based is very coarse. It's basically a quasi steady model for diffusion versus the discrete fracture model, blue curves, which basically do a better job at capturing the gradients of concentration between the fracture and the matrix.

So, what's well known about this type of a comparison is shown here, and that is that although the two sets of models converge at the later times, early time behavior is predicted to be longer breakthrough curves for this discrete fracture model than for the base case model basically that we're calling the base case model, which is the dual-k model. That's for technetium.

The next slide shows the same results for plutonium. When you also combine that with sorption in the rock matrix, it has quite a dramatic impact which of these conceptual models you choose. So, that plutonium transport for the dual-k conceptual model are these curves. When you go to the discrete fracture model, it gives you dramatically increased delays through the unsaturated zone.

Next slide. This is a simulation which gives you a feel for what is the variability in the breakthrough curve if you have the releases occurring into the fractures at the repository horizon versus the matrix. And, basically, it's a common sensical result, but it's quite a dramatic one in which if you release the radionuclide into the fractures, you get much shorter travel times than if it has to actually diffuse out of the matrix and get to a flowing fracture before it can transport down.

There's other aspects to this simulation in the sense of the effect of the diffusion coefficient itself has the opposite effect for the matrix releases, i.e. if you have smaller diffusion coefficients, you will push these curves out even further in time, which is the opposite of the fracture release case.

Next slide. This is a simulation, my final one, and it gets to the question of spatial variability. This is a depiction of the repository footprint. The nodes in the numerical model are each of the points. And, what we're doing here is we are releasing at a point, at each point across the repository, we're releasing radionuclide, in this case, Technetium 99, and we're saying what is the travel time to the water table. But, we're doing that for the case of diffusion into the rock matrix, and all those things.

So, this brings me to the point of what we're plotting here, and doing in color coding here, is the median travel time. So, we have a breakthrough curve, this is just a small reproduction of the previous slide, we have a median travel time as the measure that we're using to plot here, and it's basically the 50 per cent travel time on one of these breakthrough curves.

This shows a rather dramatic variability across the repository in the median travel time, depending on basically location where that release takes place. And factors that enter into this are the percolation flux variability, but more importantly, hydrogeologic variability. The extent of fracture flow from repository to water table really controls these results. So, they go from, in the case of the red, less than 10 year median travel times, to locations in which the predictions yield travel times of greater than 1,000 years. So, that gives you a sense for spatial variability, which is one of the questions you had.

A point I'd like to make on this is the way to think about how this plays out in a TSPA calculation depends on the release, the release term. If only a few waste packages in limit one, let's say, fails, then this type of variability is really an uncertainty in the sense that the travel times through the unsaturated zone are vastly different for that one failing waste package here versus down here. However, if most of the packages fail, the way this plays out is in a spread in the distribution of arrival times, and all of the simulations that I showed you, with the exception of one, previously include this spatial variability as part of the breakthrough curves. So, I wanted to point that out just to give you a better perspective on spatial variability.

Next. Conclusions. I have described the transport processes. We feel we have a reasonable model that considers the relevant transport processes for the UZ. The uncertainties that I showed to be most important are infiltration rate, diffusion model parameters, and diffusion conceptual model.

We also looked at certain flow model parameters, and they basically have a low to moderate impact on the travel times. And, in those cases when we don't directly propagate those uncertainties through TSPA, we take a reasonably conservative approach for those uncertainties that are not actually explicitly represented via uncertainty distributions in the TSPA.

Then, for matrix releases, they quite clearly yield much longer travel times. But, if you have fracture releases, a lower diffusion coefficient enhances transport, gets it to the water table quicker, but for matrix releases, that same lower diffusion coefficient will give you much longer travel times. So, where the release occurs, fracture versus matrix, has an important impact on how to interpret some of the uncertainties, things like diffusion coefficient.

And, then, finally, I just showed you the spatial variability of travel times, which results from the different percolation fluxes that we have across the mountain, represented in the model, and also very importantly, the hydrogeology across that part of the model.

Thank you.

GARRICK: Okay, Ali?

MOSLEH: Mosleh, Board.

Bruce, start with a couple of questions, starting with your conclusion, Bullet Number 4. What sort of factors influence the decision on carrying certain uncertainties parametrically or using a conservative approach?

ROBINSON: There's a variety of factors. Basically, parameters that are kind of determined early on to have a low to moderate impact based on logical technical arguments tend to be set off into the sensitivity analysis world, if you will, rather than propagating uncertainties that really aren't going to matter much through the TSPA. That's a computational burden that we don't want to place on the TSPA analysis because it might not allow you to look at uncertainties as well as you could for things that really do matter.

Another category that gets to your question of this decision is in the way the models are set up, some parameters are very much more simple to propagate that uncertainty through directly in the TSPA than others. Now, that's not a very great criterion, just because something is difficult to propagate, you wouldn't want to leave it out of the TSPA, and we don't do that. What we do is we take, for the ones that we take that are kind of in that range where there's really not a very big impact based on sensitivity analysis, we choose to leave those ones out of the TSPA analysis, and handle them differently, handle them with sensitivity analysis instead.

MOSLEH: And, then, maybe you can explain what you have on 18. I think it was a modeling choice--well, actually, I'm sorry, on 17.

ROBINSON: I was going to say that would not be a good example. But, we actually have that in the model for fracture and matrix releases. So, that's in the model.

MOSLEH: How about 17?

ROBINSON: This is an example of when you're building these models, the scientific, and also the quality assurance, criterion is that the models be "valid." In other words, you've done your best to build confidence that the models are consistent with all the available data, and the model, which was developed first, that was made, calibrated and made consistent with all the available data, is the dual-k model.

More recent analyses, and the ability to do this type of modeling better has led us to a point where we believe that this model could also be made "valid" in the sense of comparing it to data that's available from the site, et cetera. However, that work hasn't been done yet, and, so, just from a procedural, if nothing else, standpoint we go with this type of model, the dual-k model, with the anticipation being that as time progresses, and things develop further, we would tend to try to also take other reasonable models that we believe are consistent with the data, and use those as well.

GARRICK: Andy?

KADAK: This was an excellent presentation, by the way, it was very impressive.

ROBINSON: Thank you.

KADAK: A couple of observations, and maybe you could help me understand this as well. How well, I know you're doing a lot of sensitivities, analyses, and the last chart showed a whole bunch of different zones in the repository regime. How well do you know that to be able to do a better job of predicting what the flow will actually be of the radionuclides?

ROBINSON: Well, I think the thing to keep in mind kind of at the outset, is there is no full up repository that we can compare to, and if we could, we couldn't wait long enough to do a direct simulation. So, everything is based on experiments that are possible to do, which tend to be smaller in scale, to try to get what some of the fundamental processes that occur are, and then a mechanism for coping with the scale issues, the scaling in time and a spatial scaling. So, we can do experiments that go over 20 meters transport distances, and time scales that last the average time for graduate student. Five years.

And, so, basically, that gives you a picture for the nature of the problem that we're dealing with in terms of how well we know mountain scale radionuclide transport. To account for the fact that you basically have that fundamental uncertainty, that leads you to needing to go to parameter distributions and models that are reasonable, but not necessarily overly optimistic. It tends to push you, unreasonably perhaps.

KADAK: Towards the cautiously?

ROBINSON: Towards the cautiously reasonable; right. Can you go to the sensible way to inform a--

KADAK: We heard that in spades last time. Can you go to the next to the last slide there where you showed the repository in multi-colors? There you go.

Now, that's somebody's best estimate of the let's call it the geological structure, whether it's fracture or matrix type of rock, is that what we're seeing here displayed as results for travel time?

ROBINSON: That is a very strong factor in what you're seeing here.

KADAK: Okay.

ROBINSON: Let me talk to you about Busted Butte and the matrix flow in Busted Butte. When you're talking about releases in this portion of the repository, our geologic and hydrogeologic studies have shown that below the Topopah Spring tuff, you have large sections of vitric Calico Hills where it's probably matrix dominated flow, based on the field experiments that we did. And, so, therefore, the way that plays out in this type of a simulation is longer travel times because there's not as much fracture flow. And, the opposite is true in the areas, in the more northerly areas of the repository.

KADAK: So, are you saying you can capture the geology well enough to run your models to be able to predict radionuclide flow in this unsaturated zone? Is that what I'm hearing you say?

ROBINSON: Yes, for the purposes of performing a TSPA analysis to get at what the UZ system does for dose, or does or doesn't do for dose, yes, that would be the data.

KADAK: Earlier, our NRC friend said that they assume a fifty-fifty split between fractured and matrix type rock. I think that was like an arbitrary selection. You're more sophisticated than that in terms of being able to identify where the fractured rock is relative to the sort of diffusion type rock?

ROBINSON: I can only speak to this model and what that's based on is a developed over a large period of time, a representation of the hydrogeology and the stratigraphy of these rocks, based on bore hole measurements.

KADAK: Here's the bottom line question. The bottom line question is why do you have such short travel times compared to the NRC which has relatively long travel times, given all the sophistication?

ROBINSON: Again, I, I guess, wasn't smart enough from the presentation I saw today to know what those travel times were in the unsaturated zone. Keep in mind my travel times that I'm presenting here are once a release takes place. So, any precursor delay in that, I don't know about in that other model.

KADAK: Well, if you look at these curves, it's fairly steep and over 100,000 years.

MC CARTIN: Tim McCartin, I could give a quick explanation. I mean, it's all fractured rock in the unsaturated zone. In terms of the geology, there are places where the Calico Hills vitric unit pinches out and might be very thin, on the order of a meter or less. And, roughly speaking, where the Calico Hills vitric unit, which has a very high matrix permeability, so the water will be flowing in the matrix rather than the fractures, it's about a fifty-fifty split in our model where it's thick enough that we have included it in our model, versus other areas where it's very thin and we have not included it for purposes of the calculation.

And, so, where it's not present, you have a Calico Hills zeolytic unit, a very low matrix permeability and the flow is primarily in the fractures.

Now, the travel time distance differences I was talking about is primarily obtained due to retardation in the saturated zone alluvium. In terms of in our model, if I had to give you a best guess, where it's fracture flow primarily, it's on the order of tens of years, and we essentially typically take no credit for that, and just assume it's instantly transported to the saturated zone.

Where the Calico Hills vitric unit is, and its matrix flow, travel times are on the order of hundreds to possibly a couple thousand years. And, if it's retarded, clearly, the nuclides would take much longer. But, that's half of the repository. But, in the curves you saw, that was primarily saturated zone, the delay time.

ROBINSON: Observation would be that's quite consistent with what I'm showing here.

GARRICK: Okay, Bill, and then Ron.

MURPHY: Bill Murphy, consultant.

You mentioned that the fraction of fractures that flow in your active fracture models, one of your big uncertainties, they're a hard question to answer, and I wonder if you could tell me if the Chlorine 36 bomb pulse data could be used as a measure of which fraction the fractures flow?

ROBINSON: There are a lot of reasons for the answer of no.

HORNBERGER: First of all, he's from Los Alamos.

ROBINSON: There's a lot of reasons no. Let's leave aside whether or not the Chlorine 36 observations indicate that bomb pulse even got to--let's leave that aside, please. Even if you do that, you take for granted that the measurements are valid, the original measurements by Los Alamos. That's not really getting at what is going to control radionuclide migration from the repository because (a) it's under a different climate scenario than these simulations, and, in fact, the simulations that TSPA will be at, that's one reason. Another reason is that flow in these major features, if they really exist, may be the ones we can see with Chlorine 36 and not the totality of what the flowing fractures in the unsaturated zone might be.

I think generally, this model would say that the flowing fracture spacing is much less, much smaller, in other words, there are more flowing fractures than would be represented in the Chlorine 36 data of Los Alamos, even if you take that data at face value. So, I just think there's too many uncertainties, including the well publicized one about, you know, reproducibility of that data, to use that data set in that manner.

MURPHY: Well, you already answered my follow-up question concerning different climates. But, I wonder what other sorts of field data you would make use of to try to determine that?

ROBINSON: I think it's something that we're going to have inherent uncertainty and basically will need to quantify that uncertainty. I think it's a very tough question. You can do intentional inputs of water, like we've done for seepage and have done for other reasons, and try to test that, look at just what is the, I don't know what the word would be, topography of how that water moves through fractures, and some of that has been done. But, it boils down to having to make a prediction in a climate scenario that we don't have today, and that's a very fundamental uncertainty that's difficult to get around.

MURPHY: Okay, I have one other very brief comment, and it has to do with the figure you showed a moment ago, Number 17. And, here, you have a case where you have a strongly sorbing radionuclide, and you've maximized your fracture matrix interaction in the curves to the right, and you've included your explicit fracture model to really capture it as well as possible. And, the difference turns out to be 20,000 years between one end member of this graph and the other. So, that seems like a short time scale in general in the context of a million year repository. So, can I draw a conclusion from this that the unsaturated part of the transport pathway doesn't contribute much?

ROBINSON: Not really. And, the reason is that a million year transport calculation is only for radionuclide that sticks around for a million years. So, if a barrier like this one predicts travel times which are large compared to the half life of the radionuclide, then, of course, it is a barrier, regardless of--you know, you can go to 10 million years, and if the radionuclide is not around anymore, I mean, I'm stating the obvious, but I'm placing a caveat on what you said, it's not always a million year. It's radionuclide by radionuclide, a comparison of travel times to half life that really will enter into the peak dose analysis. Does that make sense?

MURPHY: Thank you. Sure.

GARRICK: Okay, Ron?

LATANISION: I have a generic question and I think if we look at Slide 11, that's the best. Many of these trancients appear to reach a steady state, some do not, and I'm looking in particular at the green squares, which is neptunium, and the yellow triangles, which looks like it's uranium. Is there something in the simulation that distinguishes those two from the rest of the set?

ROBINSON: Here's the way we think of it. The fact that they're going above 1, it means basically in the limit, they're going up to 2 because they're being produced from a--now, that's just an artifact of the way this calculation was performed. But, basically, if they are still going up--well, here's one that's down here that's still going up--it's because some fraction of that radionuclide that's been placed there at time zero hasn't gotten to the water table yet. So, there is delay in the unsaturated zone, maybe not for the most rapidly moving part of the radionuclide inventory, but for some of it.

Another reason for plateauing is that radioactive decay. Had this radionuclide had a longer half life, it would have been going up like the others, but it flattens out when the half life is essentially short enough to where there's no additional arrivals at the water table at longer times.

LATANISION: Okay. So, would you expect at some point, you're going to see a plateau in the others, or not?

ROBINSON: Yes, they would plateau--this is for a unit release. You picture this as a constant release over the entire simulation. That's the way to think about these simulations. And, furthermore, if you're able to do numerical convolution in your head, these are transfer functions, essentially, that for any input of radionuclides can be convolved to get what the breakthrough at the water table would be.

LATANISION: Thank you.

GARRICK: Bruce, is the bottom line to your presentation that the process models provide confidence in the abstractions used for the TSPA?

ROBINSON: That would be a conclusion, and that the TSPA representation of UZ, i.e. the abstraction model, which is what I'm presenting here, is a realistic representation of how the UZ would behave, given all the uncertainties that we also build into the model.

HORNBERGER: Just a quick follow-up. So, I remember from the last TSPA reading AMRs and PMRs and a lot of this looks very similar to me, have you presented anything that we haven't seen before, except for the sensitivities?

ROBINSON: The sensitivities are new. The model is probably the same model that you've seen before, but there are, for example, the median travel time is a function of space, location in the repository, same model, new analysis to try to provide you what you asked for.

GARRICK: Speaking of models that we've seen before, in an earlier TSPA/LA, there was an appendix that delineated conservatisms, and I'm curious as to whether or not these are still conservatisms, or they have been addressed. Let me just address a couple. One of the conservatisms was the dual permeability grid is used rather than refined gridding methods, such as the Mink method.

ROBINSON: Remember the plutonium simulations? That would essentially be an examination of what the differences are with the dual permeability model versus a more discrete fracture model. So, that gives you a sense that, yes, the dual permeability model is a significant conservatism that's currently in the TSPA model.

GARRICK: And, there's another one, the active fracture parameter is set to a high value.

ROBINSON: Yes, I showed the active fracture parameter simulations, I called it moderate impact in terms of the uncertainty. There was this family of curves that kind of had a whip that kind of goes from this one down to about that one, that kind of an uncertainty.

GARRICK: Right. And, then you had indicated a water table varying with climate. And, in the past, it was they fixed it at a specific height, and claimed that as a conservatism.

ROBINSON: That is still in the model. It's changeable, but at the present time, it's changeable because you can vary quite easily what the lower boundary of the model is, and basically tell the particles when they're to leave and be in the saturated zone. But, the simulations here and the ones that we anticipate to be in TSPA will have the 120 meter water table rise, which is I believe what you're referring to.

GARRICK: Yes. And, we already mentioned this one, but the dual permeability representation of fractures assumes complete interconnectivity, and you addressed that earlier. Is that potentially something that could have a significant effect? I'm trying to get a sense of the conservatisms that still exist and whether or not they are important.

ROBINSON: If we're talking about within a hydrogeologic unit like the Topopah Spring tuff--

GARRICK: Right.

ROBINSON: --there's lots of fractures, there's a place for water that's percolating down those fractures to go in an interconnected way, so I wouldn't characterize that, if that's what you're referring to, as a conservatism. I think it's realistic, that particular unit, which is where a lot of this uncertainty kind of plays itself out. It's in the Topopah Spring that a lot of this type of uncertainty really lies.

GARRICK: I guess I'm still troubled by--well, if I were a hydrologist, I wouldn't be troubled, I'm sure.

HORNBERGER: That's right, you wouldn't.

GARRICK: I really set you up for that. But, the physical aspects of this whole unsaturated zone, the problem that disturbs me is that there's very little water involved. Very little water involved. And, there's a massive amount of material and surface area and high residence time, and these fast pass-throughs, I just have a hard time conceiving it, as if there was no absorption capability whatsoever in the fractures.

ROBINSON: I will say that our model mutes those rapid travel times through the diffusion mechanism that I showed there.

GARRICK: Yes, but on the other hand, you also say that this model doesn't impact much the way in which the TSPA's abstractions have been made.

ROBINSON: Well, this is the abstraction model. I wouldn't say that this model basically says that the UZ is no barrier. I think you have to go radionuclide by radionuclide and do the type of analysis that we walked through earlier, you know, talking about what is the travel time compared to the decay half life to really decide on a radionuclide by radionuclide basis whether it's a barrier.

GARRICK: Well, I thought that's what we wanted very much to do, yes. Okay.

All right, any other questions? Yes, David?

DIODATO: Thanks for the presentation, Bruce. I just wanted to address one comment you made regarding Slide 19. I was pleased, in Slide 19, to see your recognition that hydrogeologic variability and percolation can make the difference in terms of travel times. So, that's an interesting result. And, you made the statement that you thought, well, if you only have a few packages, or one package, then maybe the spatial variability really represents some spatial uncertainty function that you imply on that. But, that's assuming that the role of water in, say, waste package degradation is, you know, it's not a factor.

However, if the zones where the reds occur are corresponding with zones where the environment of the waste package might be more hostile to waste package corrosion, then you couldn't separate out.

ROBINSON: That's an interesting point. I appreciate that comment. I didn't mean to simply leave it at this, frankly. But, the bottom line is it's in the TSPA model, that type of coupling. In other words, if part of the reason for this rapid travel time is high infiltration rate, there's probably an increased high percolation rate at the repository, there's probably a coupled effect whereby packages may fail more frequently in that location, and that's in the model.

GARRICK: Okay. Any other questions?

(No response.)

GARRICK: Excellent presentation. Thank you, Bruce.

All right, now we're going to hear from Bill Arnold.

ARNOLD: Okay, thank you. I'll be talking about transport through the saturated zone. So, next slide, please.

This is the outline of the talk. I will give a quick summary of the flow and transport modeling approach and abstraction approach used for the saturated zone. I will go through some of the key processes that affect releases, some examples of the technical bases for our understanding of those processes, and some sensitivity studies to try to illustrate them for the Board. We will talk about key assumptions and key uncertainties associated with those processes, and then, finally, talk about the affects of spatial distribution of releases from the unsaturated zone analogous to what Bruce presented. This is to address the specific question posed by the Board.

So, next slide, please. The general approach for saturated zone flow and transport abstraction in the TSPA has not changed significantly within the last few years. Consequently, I will summarize it at a fairly high level. The primary basis for this is the three dimensional saturated zone site-scale flow and transport models, and these are used to simulate radionuclide transport to the accessible environment. We defined four source regions beneath the repository, so we do take into account that kind of variability in transport through the unsaturated zone. And, those sources are modeled at point sources, however, that point is varied from realization to realization.

We use the convolution integral method to couple radionuclide source term from the unsaturated zone with saturated zone transport simulations that are done ahead of time.

Downstream from the saturated zone, radionuclide concentration in the groundwater is calculated by dividing the total radionuclide mass crossing the boundary of the accessible environment by the representative groundwater volume of 3,000 acre feet per year.

Climate change is incorporated into these analyses by scaling the radionuclide mass breakthrough curves that are simulated, and they're scaled in proportion to our estimated changes in the flux of the saturated zone.

And, finally, there's a separate model, an abstracted one dimensional transport model that's used for radioactive decay chains in the TSPA calculations.

Next slide. This figure shows this three dimensional site-scale saturated zone flow model domain. The upper part of the figure shows the topographic relief with a satellite image draped on it. This is Highway 95 right here.

The lower surface shows the simulated potentiometric surface that's contoured, and the red lines are the simulated particle paths through the system. This domain is 30 kilometers by 45 kilometers. It goes to a depth of about 2,700 meters below the water table .

The numerical methods that are used here include a particle tracking method that includes radionuclide transport processes of advection, dispersion, matrix diffusion in fractured volcanic units, and sorption.

I should back up one step here and explain that the flow model that forms the basis for the transport model is calibrated to site data, including numerous water level measurements within the model domain. It's also calibrated to estimates of ground water flux along the lateral boundaries from the regional scale groundwater flow model.

To get back to the transport, these simulated flow paths in the repository generally occur in the upper few hundred meters of the saturated zone, and they cross the boundary to the accessible environment approximately 5 kilometers west-northwest of the highway intersection at Amargosa Valley. So, these particle paths roughly simulate particle paths flow underneath the lower Forty Mile Wash across Highway 95 right here.

Next slide, please. To go further in the abstraction process, the uncertainty analysis is accomplished using the Monte Carlo method, and we sample all of the uncertain parameters in both flow and transport parameters. The result is multiple simulations using the saturated zone site-scale transport model.

A total of 200 realizations have been performed for the TSPA. So, these radionuclide transport simulations consist of radionuclide mass breakthrough curves, assuming a unit mass input. And, this is similar to what Bruce showed for his example sensitivity runs in the unsaturated zone.

And, these breakthrough curves result in a library of breakthrough curves that will be used by the TSPA model for the probabilistic risk assessment analyses via the convolution integral method, which is really just a numerical short-cut that is a computationally efficient method that can be used in the TSPA.

This brings us to the first process that I wanted to discuss, and this is advection of groundwater. And, I want to start with our conceptual model of how this process affects radionuclide transport. Advective flow of groundwater is conceptualized to occur through the relatively limited volume of fracture networks in the volcanic rocks in the saturated zone.

These flow paths also go through the alluvium, and in the alluvium, our conceptual model is that flow is more uniformly distributed in this porous medium with some channelization occurring in the more permeable strata.

Another feature of the system is that simulated groundwater specific discharge generally increases along the flow path from beneath the repository to the boundary of the accessible environment, and this is due to the convergent nature of the groundwater flow system at the site-scale.

And, the specific discharge varies from point to point in space, and it's a function of local hydraulic gradient, permeability, the anisotropy in permeability in the fractured volcanic units, and, to a lesser extent, temperature. This really only applies deeper in the model, where the viscosity of water is a function of temperature.

Next slide, please. This is some information that forms the technical basis for our conceptual model of advective flow in the saturated zone. On the left, is an example of this information for the volcanic units. What's shown here is a flowmeter survey in one of the C-wells. On the left is depth in meters. These are the geologic units in the bore hole. And, these curves here are the flowmeter survey, and there are two different survey methods used here, a spinner survey and an oxygen activation survey.

What they show is a high degree of channelization of flow within the well when the well is pumped. So, these are cumulative flows. So, 21 per cent of the cumulative flow occurs in this zone in the Tram tuff, 6 per cent here, 62 per cent in this zone and the Bullfrog, another 4 per cent here, I'm not sure I can read that, 8 or 9 per cent up here. And, these zones in which significant flow occurs are separated by significant distances, on the order of tens to hundreds of meters.

KADAK: I'm sorry. Could you explain that again? What is it that you're measuring? Are you pumping water, or what?

ARNOLD: Yes. The well is being pumped, and a spinner log, or a log is being taken of the flow rate through the bore hole.

KADAK: To some other place, or are you just pumping water at a certain depth?

ARNOLD: Well, just the pump the well, or down the well bore in this case.

KADAK: You're sucking it?

ARNOLD: Yes. And, this is the cumulative amount of flow as you go up the well.

KADAK: And, those regions are specific regions where there's apparently more water than others; is that right?

ARNOLD: That's right. These are the regions where water is flowing into the well.

KADAK: I see.

ARNOLD: Here is something like 62 per cent of the entire water being produced by the well is flowing in this region right here.

KADAK: Okay, thank you.

ARNOLD: On the right, is just a picture of the alluvium. It has the visual appearance of a classic porous medium. However, examination of the outcrops like this, and testing at some of the Nye County wells, reveals that there is significant variability in the hydraulic conductivity of this material, and that is accounted for in our uncertainty in the parameters in the model.

Next slide, please. Okay, now I'm going to show some breakthrough curves to illustrate the effect of advection of groundwater and our uncertainty in advection of groundwater. And, Bruce Robinson went through an explanation of what these breakthrough curves are, but just to remind you, this is relative mass as a function of time that's produced at the downstream end of the saturated zone at the boundary of the accessible environment.

This is assuming a unit input at the upstream end from the unsaturated zone. For these breakthrough curves, this is for glacial transition climatic conditions, and there's no decay and no sorption for these breakthrough curves.

The black curves are the breakthrough curves for all 200 realizations of the system. So, each is a separate realization. These embody uncertainty in all of the uncertain parameters that go into the analysis.

Now, I've also conducted a couple of other runs just for sensitivity here. If we set the advective groundwater flow rate at its 90th percentile value, we get the solid red curve here. If it's set at its 10th percentile value, we get the dashed red curve here.

So, the conclusion is that our uncertainty in advection encompasses a significant portion of the overall uncertainty in radionuclide transport rates for non-sorbing radionuclide, anyway. And, this conclusion is borne out by other sensitivity studies that indicate that our uncertainty in groundwater specific discharge is one of the most significant uncertainties we have in the saturated zone.

Next slide, please. The next key process I want to discuss is radioactive decay. Now, these are similar breakthrough curves. This is for Technetium 99, so this is non-sorbing radionuclide, and most of the breakthrough curves that I'm going to show here do not include decay primarily for the purpose of better illustrating or isolating the processes I'm trying to illustrate in the sensitivity analyses. And, such is shown on the left here. So, this is the entire suite of 200 realizations with no decay.

For Technetium 99, it has a half life of 213,000 years, and you can see the effects of decay only occur at very long transport times. And, you saw this on the breakthrough curve shown for the unsaturated zone. The breakthrough curves flatten out here at very long times.

Next slide, please. And, it's good to remind ourselves of how dramatic this effect can be, as Bruce pointed out, for a radionuclide such as Cesium 137, which is highly retarded in the saturated zone. On the left, are the simulations with no decay, and on the right, shown with decay, which of course is essentially zero breakthrough because Cesium 137 has a half life of only 30 years, which is very short relative to the simulated transport time.

GARRICK: I don't know why you show that curve. It's so obvious, I mean, nobody is worried about Cesium 137.

ARNOLD: Yes, this is kind of an end number.

GARRICK: Let's get to the ones that are important.

ARNOLD: Okay, next slide, please.

Okay, another key process is climate change. This is done in a fairly straightforward manner. We have scaling factors of groundwater specific discharge in the saturated zone for monsoonal and glacial transition climate states, and these have values of 2.7 and 3.9 respectively.

These scaling factors are based on the ratios of the average infiltration in the unsaturated zone site-scale flow model for these climate states.

In the case of the glacial transition climate, this is also corroborated by steady-state flow simulations using the Death Valley regional groundwater flow model for glacial climatic conditions.

Next slide, please. The next key process is matrix diffusion, and Bruce spent quite a bit of time discussing this process in the unsaturated zone. I thought I'd summarize here what we do know in the saturated zone. We have an extensive database of laboratory-scale studies and measurements of effective diffusion coefficients in the volcanic rock matrix.

In the saturated zone, we also have a field scale demonstration of the matrix diffusion process in cross-hole tracer tests at the C-wells.

However, there are significant uncertainties in this process. We have uncertainties in the degree of channelization of groundwater flow in the fractured volcanic units, as illustrated by that flowmeter survey that I showed earlier. We have uncertainty in the flow porosity in the fractured volcanic rocks, and we have uncertainty in the effective diffusion coefficient due to uncertainties in chemistry and uncertainties in the specific rocks encountered along the flow path in the saturated zone.

Next slide, please. This is an illustration of the kind of experimental data that we have to support these conclusions. What's shown on the left here are some breakthrough curves from cross-hole tracer tests at the C-well complex in the Prow Pass tuff. This is a normalized concentration of the tracers used in the test, pumped over a distance of approximately 30 meters between the wells, versus the time here in hours.

On the right is a laboratory diffusion cell experiment for the Prow Pass tuff, the same rocks through which the tracer test was conducted. This is a concentration versus time within this diffusion cell experiment. And, let's start on the figure on the right. What this shows is two different tracers or solutes of bromide and PFBA. These have different diffusion coefficients in the tuff, and they differ by about a factor of three. The diffusion coefficient for bromide is higher than the diffusion coefficient for the PFBA because it's a larger molecule.

Now, if we go back to the results from the C-well tracer test, this is the breakthrough curve for the PFBA. This is the breakthrough curve for the bromide, and this is the breakthrough curve for lithium, which also sorbs on the tuff matrix.

So, two points here. First of all, there's a significant offset in the peak between the TSPA and the bromide concentrations, the breakthrough curves. Really, the only way to account for this offset is by the process of matrix diffusion from the groundwater flowing in the fractures between the two wells, and the relatively immobile groundwater in the tuff matrix.

Now, lithium is even lower, and this is a demonstration that the sorption process is also operating here, where diffusion into the matrix is providing access to the matrix for sorbing radionuclide and then it is also then sorbing, and the peak is lowered even more for the sorbing radionuclide.

A couple of other indications are that there were flow interruptions in the tracer test, and the concentrations of all of these tracers rebounded during the flow interruption. This is a diagnostic characteristic of matrix diffusion. It also occurred at this point here.

Next slide, please. Now, some sensitivity runs to show you the impacts of matrix diffusion. Again, a breakthrough curve, a relative mass versus time. The solid breakthrough curve here is our expected case for a non-sorbing species. By expected case, I mean that we're using the expected value for most or all of the uncertain parameters used in the analysis in the transport model.

The short dashed line here is our minimum diffusion case. This is with the diffusion coefficient set very low, so there's essentially no matrix diffusion occurring. So, all of the transport time out to this short dashed curve is accounted for by transport through the alluvium, or delay through the alluvium.

The long dashed curve is the maximum diffusion case. This is with the diffusion coefficient set high enough that there's essentially full diffusion into the rock matrix. So, this is the envelope of behavior that can occur with regard to matrix diffusion for a non-sorbing species in the model. And, what you should note here is that the expected value case includes significant matrix diffusion, but a majority of the mass arrives at times closer to the minimum diffusion case.

And, looking at the entire range of uncertain parameter values that are used in the transport simulations, we actually span this behavior for minimum diffusion to maximum diffusion among those 200 realizations.

Next slide, please. This is a similar sensitivity run conducted to illustrate the impact of sorption in conjunction with matrix diffusion. The solid black line again is our expected case for a non-sorbing species. The solid blue line is using the same parameters but for the sorption coefficient of neptunium in the volcanics. So, this is the amount of additional retardation due to sorption in the volcanic matrix only.

And, then, the dashed blue line is neptunium breakthrough curve for sorption in both the volcanics and the alluvium. So, for these expected parameter values, the sorption for neptunium in the alluvium provides the majority of the retardation for neptunium.

Next slide, please. To look at an even more highly retarded radioelement like plutonium, this is a similar sensitivity study. Again, the expected case breakthrough curve--now, this is for plutonium that is reversibly sorbed onto colloids, so this is a colloid-facilitated model. Here, the solid blue curve is for sorption in the volcanic matrix only, not in the alluvium, and the dashed blue curve is sorption both in the volcanics and in the alluvium.

So, for expected parameter values for neptunium, sorption in the alluvium provides additional retardation of plutonium, and that's particularly true for the mass that arrives before the 50 per cent of the breakthrough value.

Next slide, please. Now, I wanted to describe next, the next key process is sorption, and our conceptual model for sorption. Our conceptual model is that we have local equilibrium between radionuclides in the aqueous phase and the aquifer material. In other words, the sorption reactions are rapid and reversible.

We have a linear relationship between radionuclide mass on the solid phase and the aqueous phase. This is the Kd approach, linear Kd approach. And, we recognize that sorption reactions are influenced by the local chemical conditions. And, this includes water chemistry, such a pH, Eh, concentration of carbonates, and so on. It's also influenced by the rock types, the mineralogy of those rock types, devitrified versus zeolitic tuffs in the alluvium, and potentially influenced by radionuclide concentrations.

So, these factors and scaling considerations are taken into account when we assess the uncertainty in sorption coefficients in setting our uncertainty distributions for sorption coefficients in the transport modeling.

Another point is that oxidizing conditions are assumed in the saturated zone. This is a topic that we have discussed with some members of the Board recently, and I will have more on that later.

Next slide, please. This is just an example of some of the sorption data. There is a large amount of laboratory sorption data that are site specific that are available for Yucca Mountain. This example is for neptunium onto devitrified tuffs, where we have the neptunium Kd, these are batch sorption experiments, as a function of experiment duration. This gives some idea of how long you have to conduct these experiments to achieve equilibrium. And, you can see that we're talking about sorption coefficients in the range of 1 to 10 milliliters per gram for neptunium. This is under oxidizing conditions, open to the atmosphere.

The other example is for plutonium onto devitrified tuff. Again, plutonium Kd is a function of experimental duration. Now, we're talking about sorption coefficients approximately in the range of 100 to 1,000 milliliters per gram for plutonium.

Next slide, please. This is an example of some of the data for sorption in the alluvium from samples obtained from the Nye County Drilling Program. These are for neptunium and for uranium. You can see that there is some variability within the samples that have been taken from the alluvium. But, we do have a good technical basis for our uncertainty distributions for neptunium and uranium in the alluvium.

Next slide, please. Now, to look at a similar sensitivity analysis to get an idea of the relative importance of neptunium sorption. You can compare this to the sensitivity study that I showed for specific discharge. So, these are our breakthrough curves. This is the entire suite of 200 breakthrough curves for neptunium in the saturated zone, shown with the black curves. And, then, the red curve is using the expected value for all other parameters, and the 10th percentile value of neptunium sorption coefficient.

The red dashed curve is using the 90th percentile of the neptunium sorption coefficient. This is in both the alluvium and the volcanic units.

So, our uncertainty in neptunium sorption encompasses really only a moderate portion of the overall uncertainty in the radionuclide transport rates. And, this is because we have a pretty good data set on neptunium sorption, and we have a fairly high degree of confidence in what those neptunium sorption coefficients are in the system.

Next slide, please. Now, to go through some of the key assumptions. Our current information forms the basis for the following key assumptions with regard to radionuclide flow and transport, groundwater flow and radionuclide transport in the saturated zone.

Steady-state groundwater flow in the saturated zone. We assume an instantaneous change in saturated zone groundwater flux with climate change, and no change in the flow paths for the saturated zone.

Matrix diffusion occurs from uniformly spaced, parallel fractures in the fractured volcanic units, as implemented with the analytical solution of Sudicky and Frind. This is an obvious simplification of the system.

Equilibrium, linear sorption occurs in the tuff matrix and the alluvium. There is no sorption of solutes on fracture surfaces or coatings. Radionuclide mass from the fractures and matrix flow in the unsaturated zone is input to the saturated zone in the fractures. So, the sum of the radionuclide mass arriving at the water table in these two continua in the UZ model is put into the fractures in the saturated zone for transport.

Next slide, please. We assume oxidizing conditions in the saturated zone with regard to sorption coefficients and solubility limits of redox-sensitive radionuclides, such as Technetium 99 and Neptunium 237. And, we do have some indications that local reducing conditions may exist in the saturated zone. These reducing conditions can have a dramatic impact on both the solubility and the sorption coefficients for Technetium and Neptunium.

We have a high degree of uncertainty regarding the distribution of those redox conditions in the saturated zone, but as I said, this is something we have discussed with some of the Board members in more detail.

For transport of radionuclides reversibly attached to colloids, we assume that there's local equilibrium among the colloids, the aqueous phase, and the aquifer material. For radionuclides irreversibly attached to colloids, it is assumed there will be no desorption of radionuclides from the colloids. So, we assume that this attachment is permanent, at least for the duration of the residence time in the saturated zone.

And, colloids are subject to attachment and detachment from the mineral grains in the aquifer, and are, thus, retarded in their transport through the saturated zone. But, there is no permanent filtration of colloids in the saturated zone.

Next slide, please. This is just a list of the parameter uncertainties in the saturated zone abstraction modeling. Here, I categorized these into uncertainties in groundwater flow and geological uncertainty, and transport uncertainty. We have uncertainty in groundwater specific discharge, the degree of horizontal anisotropy in permeability in the fractured tuff. We have geologic uncertainty with regard to the alluvium/tuff contact in the subsurface, although this uncertainty has been significantly reduced with the Nye County Drilling Program.

With regard to transport, parameters that influence matrix diffusion, we have uncertainty in the flowing interval spacing, or the spacing between fractures that conduct significant amount of groundwater in the saturated zone, effective diffusion coefficients in the matrix, flow porosity in the tuff. Then, of course, sorption coefficients, dispersivity, effective porosity in the alluvium, the source location beneath the repository, colloid retardation factors, sorption coefficients onto colloids, and groundwater colloid concentration.

Next slide, please. This is an example of one of the uncertainty distributions for this key uncertain parameter, which is specific discharge, or this is really the multiplier in specific discharge as it's applied in the abstraction model.

What this is is cumulative probability. So, this is CDF, and this is the log of the specific discharge. So, our median value has a log value of zero, so it's a multiplier of 1. Values greater than zero represent a multiplier greater than 1, and less than zero, represent a multiplier of less than 1. A devisor.

So, uncertainty in specific discharge is based on the results of the saturated zone expert elicitation and on more recent well testing in the alluvial tracer complex. And, the discrete cumulative distribution function has 80 per cent of its probability between a factor of one-third and 3 for the specific discharge multiplier.

And, this is really the range in uncertainty that was derived from the tracer testing in the alluvium. However, we have residual uncertainty with regard to specific discharge along the entire flow path, and that uncertainty is accounted for in the tails of the distribution and these tails are taken from the saturated zone expert elicitation.

Next slide, please. And, finally, this is that sensitivity study to address the impacts of uncertainty in the source location beneath the repository. So, what I looked at here, here's the repository outline. I conducted transport simulations for non-sorbing species for four locations near or beneath the repository. These correspond to the center of those four source regions that are used in the saturated zone abstraction model. The colors here, which

I know don't show up on your black and white copies, correspond to the four breakthrough curves shown on the right here.

So, these blue flow paths correspond to the blue breakthrough curve for source sort of in the northeast corner of the repository. Then, the green is the northwest corner, and this other blue and purple, magenta, correspond to sources in the southwest and southeast parts of the repository. And, some things to note here. Generally, the transport times are a little bit longer for source release to the further north in the repository. That's because the transport distance is longer from these locations to the boundary of the accessible environment. And, also, releases on the western side of the repository seem to transport somewhat faster than on the eastern side of the repository, particularly the northeastern side of the repository. This is due to the flow path which is further to the west, and it encounters less alluvium along the flow path and, thus, has shorter transport times.

It's interesting to note that there's kind of a reverse correlation with the pattern of transport times that Bruce Robinson showed for the unsaturated zone. In the unsaturated zone, the longest transport times occurred in the southern part of the repository down here. But, overall, there is not a high degree of sensitivity to the source location underneath the repository with regard to transport time, simulated transport times in the saturated zone.

Next slide, please. So, in conclusion, I just described how the calibrated three dimensional site-scale flow and transport models form the basis for the abstracted radionuclide transport simulations for use in the TSPA. We examined several key processes and their impacts on the releases of radionuclide mass from the saturated zone. Of these, our uncertainty in advection of groundwater is probably the most important with regard to our uncertainty of transport and release rates from the saturated zone.

And, also, spatial variations of releases to the saturated zone did not have large impacts on the simulated releases from the saturated zone relative to other uncertainties.

So, thank you.

GARRICK: Thank you. Dave?

DUQUETTE: Duquette, Board.

I've been seeing these curves for about as long as I've been on the Board, about three years, and maybe I'm finally starting to understand them somewhat. Am I correct in assuming that basically you pick up where Bruce left off, that is, they're simply additive. Your time zero is when he has breakthrough from the unsaturated zone into the saturated zone?

ARNOLD: That's correct.

DUQUETTE: Okay. And, if I take a look at your Figure 9, or Slide Number 9, at least some of the breakthrough occurs in less than ten years. And, he also showed some breakthrough in less than ten years. Can I make the assumption that you expect some nuclides to be transported to the biosphere in about a 20 year period?

ARNOLD: You have to be careful how you use the word expect here, because you actually have to look at this entire suite of breakthrough curves, and what we're seeing here is basically our probabilistic assessment, our uncertainty in these. So, one way to look at this would be that just for the saturated zone for these simulations, it looks like there are about four breakthrough curves that have a median breakthrough of less than ten years. That's four out of 200. So, you might say that we have a 2 per cent probability that transport through the saturated zone for glacial transition climatic conditions could occur in less than ten years.

DUQUETTE: Yes, that's what I thought I understood with the way the curves are to be used. Doesn't that make it even more important, even with a 2 per cent probability, that the containment of the radionuclides in the engineered barrier becomes increasingly more important?

ARNOLD: I don't think you can make that direct comparison between these results and the relative importance of other components of the overall system.

GARRICK: Yes, Thure.

CERLING: Cerling, Board.

If we could go to Slide 23? So, now, where would you put your effort if you could try to improve on the spread that Dave just alluded to in the previous slide 9, or whatever it was, but where do you think your most important area is in this long list of things that are the parameter uncertainties that you could narrow down that very wide range of horse tails.

ARNOLD: Well, to do that, it's a pretty subjective exercise, and I can give a couple of ideas here, but I can't necessarily back them up, you know, quantitatively. It's kind of a balancing act between how much difference could additional understanding for that particular parameter mean with regard to our transport simulations, and how amenable would this parameter be to investigation through further work.

I would say that this is an important parameter of flowing interval spacing. The degree of channelization of groundwater flow in the saturated zone has a high degree of uncertainty based on the field observations that we have at this point, and there are ways in which that uncertainty could be reduced.

I think another one is specific discharge. Our uncertainty in the flux of groundwater through the system is an area in which we could reduce our uncertainty also. Those would probably be the highest priority parameters that I would choose out of this list.

GARRICK: Bill, Andy and Ali.

MURPHY: Bill Murphy, consultant.

I was fascinated by the data from the C Number 3 well, the flow, the channelized flow. I hadn't seen that before. And, I added up the percentages, and it looks as if 100, actually 102 per cent of all the flow is in the channels. Does this imply that there really is no flow in the large intervening spaces?

ARNOLD: Well, there are limitations to the way in which this assessment was made. These spinner logs, I think in particular the oxygen activation survey, have limitations with regard to resolution. And, again, there are other techniques that have a higher resolution that could detect flow from fractures at a much smaller percentage than this. So, this is not definitive, but these are the data that we have to work with now.

Another thing to keep in mind that I sort of glossed over with regard to, and this is on Slide Number 7, if you could put that up, is that these features that are producing groundwater into the well bore are in actuality probably dipping at a fairly high angle. You know, I think the average dip angle for fractures and fracture zones is over 60 degrees. So, the actual perpendicular distance between these is not as great as the apparent distance shown by the separation and the well bore. So, that's another consideration here. We did account for that in our analysis.

MURPHY: That's a very important point, and I'm glad you pointed it out. It starts to address a related question I have. You said in your particle tracking models, the transport is in the upper two or few hundred meters. And, I'm wondering to what extent the particle tracking model accommodated the details of this channelized flow that's recognized in the bore hole?

ARNOLD: Yes, the particle tracking itself uses a continuum representation of the flow field. So, those flow paths do not include the complexities of this channelization that we're inferring from this information here. The transport simulations do account for it in the dual porosity transport simulations. So, there is kind of a difference here. The flow paths don't have the kind of detail that's shown here, but the transport simulations do account for this in an implicit fashion.

MURPHY: Would you expect, given the channelization that's evident here, that flow is confined to a narrower top part of the saturated zone, or would the dip on these channels mix at hundreds of meters?

ARNOLD: It would not necessarily be shallower or necessarily deeper. It would probably be not as smooth as it is represented in the particle tracking.

MURPHY: One final question. Are there comparable flow data for the alluvium showing potential preferential flow paths, or channels?

ARNOLD: Again, in the alluvium--in the particle tracking, the alluvium is represented as a homogeneous medium. So, the particle paths are fairly smooth.

HORNBERGER: He's asking about data.

ARNOLD: Okay, with regard to data, there are data that indicate that there would be channelization, and we do have some preliminary data from cross-hole tracer tests that suggest that the effective porosity of the alluvium is lower than the total porosity of the alluvium. This would be an effect of channelization. And, that hydraulic conductivity can vary significantly between strata within the alluvium.

MURPHY: Thank you.

GARRICK: Thanks. There's a number of us that have questions, but we have just simply run out of time, and I think we need to move on. And, as a matter of fact, as a courtesy to the public members, I want to sandwich in at this time the public comments, rather than having the last speaker, if the last speaker will just be patient, we will come to that after we have heard from those who have signed up to make the statements.

I have two names here. They are familiar names. Is Sally Devlin in the audience?

DEVLIN: Good evening, everybody. And, thank you all for coming to Nevada. I hope next time you'll come to Pahrump and have your meeting, in Nye County, the host county where I live, and I'm from Pahrump, and my name is Sally Devlin and I've been doing this for 13 1/2 years. And at my very first meeting, John Cantlon, who was the chairman then, he said you're an idiot, go back to school. So, we did. And, thanks to school, and I say that you have really made my ego go to the moon, because all you did today was talk about my colloids, which I introduced in '95, and the microbes.

And, of course science has really blossomed these last five years with microbiology and all the rest of the stuff, and I really didn't know how far it would go, but I see that almost every report has one or the other, or both, in them. And, this is wonderful, and I really do compliment you on it because the science of Yucca Mountain has improved so in the last five years, it's been just simply wonderful. And, John, of course, has been a wonderful leader. He's my pet. But, anyway, it's so nice to see so many familiar faces, as well as so many new faces on the Board. And, I knew there would be quite a bunch new ones.

But, anyway, what I have to say is basically this.

I want to thank one of the presenters for saying something nice about Nye County wells. When Nick Stellasoto (phonetic) was alive, and he did a hydrology report back in '99 in Amargosa on Yucca Mountain, it's one of the best meetings I've ever attended, and he did actually go in the mine and put his equipment in there, and, of course, it disintegrated. And, I always remember that for those of us who were there.

The other thing, you keep talking about the TSPA and that will come, and what have you. We all know that the licensing is off for another two years, and I know you will be preparing another million papers. You were kind enough to send me a thousand pages on twelve topics, and you got my report on it, and I said basically the same thing I've been saying for 13 1/2 years. I love your reports. They're very informative. I love the science that you are producing. I love the sophistication with which you present your programs, as the toastmaster, which I told everybody to go and join, I give you wonderful evaluations, very few ahs, very few pauses, and it's been a delight. But, again, you have done what you always do, and that is you are modeling.

And, I don't know how long, Mark said you're going to do his thing for another five, six years, and so on. Now, I'm 76 and I'm on borrowed time and this is my 13th and a half year here, and I'm saying to myself I might not be around for those five or six years, and then who's going to come and yell at you, as I always do. Because I really resent the modeling. I want some reality. I want to see a canister. You haven't a sign of a canister. That's a lynch pin component. With the Alloy 22, my bugs ate it immediately. I have all those reports. So, there's the bugs. You have nothing for a canister, and that's number one.

Number two, you have no design for the mine. You have no transportation, and so on and so forth. But, mainly the canister. If you have nothing to put the waste in that will last less than 100 years, and I'll even give you 50 years, I'm very generous, I'll even give you another three, Russ. Hear that? And, I do mean that, and it bothers me as the public because as the public, and there are very few of us who have really taken the interest, who are deeply affected, when we hear nothing but modeling and not reality, reality is real, and the 16 billion, which is the last figure that I saw for the monies spent on Yucca Mountain, I think that is thoroughly unacceptable for continuing modeling.

I understand that you're doing the geography and the water flow, and all that wonderful stuff. But, that doesn't make a canister. It doesn't make the metallurgy that is really needed to be implemented and put in the mountain and tested. And, that is number one with me. So, that's what I'm going to leave you with. You'd better do a canister, and you'd better have a better alloy than 22 that my bugs will eat. And, I don't know where it's going to come from, because the steel won't work, I don't care whose steel it is, zirconium or whatever. Remember my diamond? I thought zircaloids were diamonds. But anyway, I did, they were zircons; right? Same family.

But, anyway, I'm just saying that this science has got to stop that you're doing now and do some real stuff. Do at least some kind of test on some kind of thing. All these labs have been getting rich over the years. You all, I hope, have had a very comfortable marvelous living, but as I say, I want to be here to see something really in fruition, and I don't get that feeling.

So, that's about my statement. I hope you heard it, because those of us who have been on this project, as I say, for a very, very long time, we're not going to be here. Our children are not going to be here to fill up a Yucca Mountain with 77,000 metric tons of waste. It will take 100 years, and there will be a need for the second Yucca Mountain, and I'm looking at the other reports that I get from my wonderful friends at NRC about on-site storage, 34 plants are putting it on site, and so on and so forth. Will there be a need for Yucca Mountain? That's another question. And, where will it go?

I heard President Bush last night talk about Yucca

Mountain, and whether it was positive or negative, I don't know, but he had an open mind, and I think the open mind was hurry up, guys, you've spent too much money, we don't have the money. We're in serious financial straits, and we'd better get something done.

So, my suggestion is work on the canister immediately, start testing in the mine, or something, or if you really don't have a lynch pin component to do Yucca Mountain, then I think it should stop. And, so, anyway, we are all sitting on banana peels and we've got to do something positive, and I think it's wonderful that you model, but I want reality.

Thank you.

GARRICK: Thank you, Sally. We always enjoy your comments.

Our next speaker will be Grant Hudlow. Grant?

HUDLOW: Hi. I'm Grant Hudlow. For those of you that don't know me, I'm a chemical engineer with nuclear engineering training and experience. And, I have some good news and I have some bad news. The only reason I have bad news is because we have new members on the Board that I think need to hear it. The others have all heard it. And, I have some very good news.

You have a valuable resource in John Garrick. He breathes the rarified air that only industrial turnaround experts can access.

The bad news, we have 20 years of junk science, and Congress jumped all over DOE in the Nineties, and it just goes on and on and on.

Investigation into the murder of Paul Brown shows that gangsters got a large amount of the Yucca Mountain money. What they did was they bid on projects, did a phony report, got paid, and then went and did another one. And, the whistle blowers are now confirming that sort of nonsense.

Number three, there is no known material on the planet that can withstand neutron imbrittlement for over 100 years. And, number four, the California rate payers want a refund because of fraud.

Well, the good news is that you can run Yucca Mountain as a warehouse and replace the waste packages every few years. The other good news is that industry has now solved the waste problem by building high-rise dry storage facilities. And, as a result, they are building new nuclear facilities to generate electricity.

But, just a brief summary, I know we're out of time, and thank you for bringing your brains to the backwoods of Nevada. I always appreciate seeing you again.

GARRICK: Thank you. Thank you, Grant.

Are there any other comments? Yes.

TREICHEL: I have one question. Judy Treichel, Nevada

Nuclear Waste Task Force. Can I get either the backup or the additional slides that were on Ernie Hardin's presentation. He had additional slides from what were in the package.

GARRICK: Sure. Any other comments?

(No response.)

GARRICK: All right, I guess we'll go into our final presentation on the Management and Technical Support Peak Dose Sensitivity Analysis by Mark Nutt.

NUTT: It always seems that I get the opportunity to talk to the Board when I get the pleasure to develop simplified models. All these other people are talking about very complex processes, but my pleasure is one about five years ago that I got to talk, and then today about simplified models we put together. So, maybe my thought would be let's not do anymore simplified. We've got to keep making more complexity into it.

In my overview, I'm going to talk about the objectives and limitations of the analysis we put together. I'll give a quick overview of the model approach, talk about the features, events and process evaluation we went through to come up with this sensitivity analysis, the key assumptions, the modeling approaches, and finally, results.

Next slide. Before I get into it, it's the big caveat. The objective was to develop a scoping-level simplified model to identify those factors that influence the repository performance over the period of peak dose. This is not a compliance model. The results are informative in nature only, and should not be used to compare to any proposed or final regulations.

This effort was initiated well in advance of the issue of the proposed rules 40 CFR 197 and 10 CFR 63. In fact, it was started well over a year ago, we started looking into this effort. No attempt has been made to reconcile the approaches, methods and the sensitive analysis with those proposed revisions. And, an assessment of post-closure repository performance for demonstration and compliance with those rules will be done once they are finalized.

I've been told early that the Board is somewhat disappointed at the lack of result curves that are in here. To generate the results, or to satisfy the objectives we were trying to meet with this effort, we did have to generate result curves. They're in the report that's publicly available. I invite anybody to go look at them. But, the reason I didn't include them is I wanted the emphasis not to be on the curves, but the differences between them in the sensitivities we found. And, I'll be happy to talk to, and I will at the end of the talk, to what we found out regarding the sensitivities, what are the key factors related to repository performance over this period.

Next slide, please. In the overview, part of the reason we started doing this and we looked into this is we, as the Board recalls, or has seen, we've done estimates for the FEIS over the period of the peak. We hadn't done anything since then, as you've seen all the models we talked about today refer to 10,000 year period. So, the question came up given the new changes we have had, what could be the potential impacts over the period of the peak? So, we started doing this.

In doing it, we considered the FEPs, the features, events, the processes, that were evaluated over the 10,000 year period. We developed a simplified model that includes representative FEPs that could potentially affect repository performance over the period of the peak dose.

FEPs that either have a minor or no effect on the peak dose were either not included in the model or were included in a bounding representation. And, even though some of the FEPs may have an influence on the repository performance over the 10,000 years, they may not have or don't have an influence over the period of the peak, and can be excluded or be included in the model in what we call the bounding representation. And, that's that we took no performance credit for those FEPs or series of FEPs, and I'll talk to some of those later on.

Next slide, please. We used documentation current as of early 2005 in this effort. We also used historical information to essentially bridge the gap between the 10,000 year analyses that were put together that you have seen today, and the period of the peak. As Russ Dyer mentioned this morning, we are looking at revising some of those models, and some of the inputs that were used in this analysis may be revised.

This piece of work won't be revised. It's essentially served its purpose and it's done.

This sensitivity analysis used the simplified model, was similar to a performance assessment in 10 CFR 63. It's a fully integrated system-level model, but what it is not, it's not a full TSPA. We didn't include the level of detail in the various processes that you would see in a full TSPA. And, you will see some of that later on when I start talking through the model approaches we took.

Next slide, please. In order to look at the factors that were important over the period of peak, it was necessary to go back and look at the features, events and processes, the FEPs. So, we began evaluating the exclusion arguments that were conducted over a 10,000 year period against a longer time frame.

We did, when you find the report, or look at it, you won't see an exhaustive evaluation blow by blow, FEP by FEP. We just didn't do that. We looked through and made some judgments and I'll talk to those later.

But, prior to talking about that, we want to look at what we call slow and infrequent processes over the period of repository performance. Those are essentially of secondary importance to the primary degradation modes that are the most significant to repository performance over the period of peak. These less significant role of slow and infrequent processes, if they were to occur, would actually tend to spread the release rate over time. They'd be mechanisms that may give smaller releases from the EBS that would--we'd have a total amount of inventory. You'd be pulling it back further in time, rather than leaving it out or calculating it out at the time of the peak.

These types of FEPs are also not likely to occur over longer time periods due to cooling of the repository environment. And, we feel that such processes can reasonably be excluded from the post-10,000 year assessment, based on low consequence.

Next slide. I'll give a few examples. One is the slow degradation processes of the engineered features, the drip shield, the waste packages and the pallet, and these types of processes include, but are not limited to creep, thermal sensitization, hydride cracking, consolidation, and non-seismic induced mechanical degradation. They are very slow degradation processes that are accelerated at higher temperatures or require higher temperatures to initiate.

If they are either not initiated, they're in the 10,000 year period, or they are sufficiently slow over that 10,000 year period that they won't have an effect over the 10,000 years. But, as you go out further in time, the temperatures will tend to slow down, or tend to go down below the 10,000 year--the values that are over 10,000 years, so these processes, the rates will tend to decrease or they won't initiate, and therefore, you could argue that since they don't play a role in 10,000 years, they're not going to play a significant role beyond 10,000 years.

As I mentioned earlier, even if they did affect the degradation rate, the modes generally result in small holes and cracks in engineered features, which would result in slower diffusion or less of a release than would occur when the engineered features are essentially grossly breached by the corrosion processes, the general corrosion that occurs at the time of the peak dose.

Next slide, please. Slow stress-induced degradation processes of the emplacement drifts that could affect engineered barrier system performance. These include, but are not limited to, drift collapse, consolidation induced by drift collapse, and included in these processes is the creep and static fatigue of the rock. And, this mainly is dealing with the non-lithophysal units of the repository horizon.

And, again, the rates of these processes decrease with lower temperatures and, therefore, they become more stable with the repository environment. They are more significant at earlier times when the repository is thermally perturbed and you end up with a thermal perturbation and mechanical stress modifications. And, if they can be reasonably excluded from a 10,000 year period, the decreased rate of deformation over the long period of time, we feel we can reasonably exclude them for the longer period.

Next slide, please. There's also infrequent stress-induced degradation processes of emplacement drifts affected by seismic events. And, the drift over the period of the peak dose is expected to be affected by seismic events, and they can induce stresses that lead to deformation and degradation of the emplacement drifts.

Because this is the most significant effect of seismic, it would be such degradation as drift collapse. This degradation mode should be considered included in a seismic scenario evaluation.

There's degradation processes initiated by seismic and volcanic event sequences. We feel that evaluating the risks associated with such events can be reasonably approximated by continuing the 10,000 year assessments to a longer time period. And, we tend to believe in how we approach it that this analysis is evaluated in an uncoupled fashion would tend to maximize the likelihood--or minimize the likelihood of diluting the risk. Essentially, again, if you look at them uncoupled, you are not getting early failure to pull inventory release out in time. You're waiting and letting it all come at a later period of time when the event occurs. You are not distributing the EBS type releases over a long period of time.

Next slide. So, using that kind of background, we went through the FEP evaluation, again, looking at all the FEPs that were in the various reports that are out there that covered the 10,000 year period, and feel that the vast majority of the screening justifications applicable to the 10,000 year period are appropriate over a time period that covers the peak dose.

This tends to result from screening justifications being made on either time invariant or on an annual probability basis, or low consequence basis that is not affected by time. Essentially, we've made the FEP justification that doesn't have an aspect of time in it, and those tend to stay out.

What we found is those that can be excluded from a 10,000 year post-closure performance assessment can be and were excluded from this sensitivity analysis. Those that need to be included in a 10,000 year post-closure performance assessment were included in the sensitivity, simplified sensitivity model either explicitly or implicitly through a bounding approach.

Next slide. The remaining FEPs essentially fall into three categories, and the first one was that the screening justifications for several FEPs that could be excluded from a 10,000 year post-closure performance assessment can continue to be excluded with some augmentation. But, in this sensitivity study, we continue to exclude them, and in the report that's publicly available, you can see the augmentation to the arguments that were made.

A few of the FEPs related to seismic effects that can be excluded from the 10,000 year post-closure performance assessment were considered appropriate for inclusion into the sensitivity analysis, and they were, and we'll talk to those in a little bit.

Some of the FEPs, as I mentioned before, that need to be included in a 10,000 year post-closure performance assessment can be excluded from analysis conducted over the peak dose based on low consequences, and if they have a negligible effect on the measure of the peak dose.

Next slide. The key assumptions we made in the sensitivity analysis, and I'll go into more detail of these later, so I'm going to walk through them pretty quick. We assumed that integrated long-term average climate state, which resulted in a slightly larger infiltration rate than the glacial transition climate.

We assumed the repository percolation flux was equal to the average infiltration rate. We assumed collapsed drift conditions for seepage all throughout the repository. We assumed the seismic activity will result in drift collapse throughout the entire repository, and I'll show the impact of that in a bit.

We did not consider diffusive radionuclide transport within this model. We looked at previous TSPA analyses, the FEIS, the TSPA for the site recommendation, the supplemental science and performance assessment analyses, and tended to demonstrate that the engineered barrier system radionuclide releases via advection were several orders of magnitude larger than diffusive releases at the period of the peak dose.

And, since that large of a difference, and we found that the peak dose tended to be dominated by these advective releases, that we didn't consider diffusive releases. And, if you make that assumption, the degradation mechanism results in you not having to consider stress corrosion cracking, which we believe right now is a diffusive release mechanism, given the tightness of the cracks and the tortuosity.

This also tends to maximize the inventory, the radionuclide inventory available when gross breaching occurs, and you get these advective transport pathways. It leaves everything in the analysis and in the model until you get the gross breaching, and then you can let it all come out under much more I'll call them aggressive conditions.

Next, please. So, general corrosion is the only corrosion that we considered in the sensitivity analysis. The key aspect of controlling the peak dose is the formation of large openings in both the drip shield and the waste package, leading to these advective transport pathways. And, again, not considering these other smaller breaches, such as stress corrosion cracking, will maximize the inventory available when the gross breaching occurs.

This is the one that goes back to Tim McCartin's talk this morning. We assumed instantaneous degradation of the waste forms. The period over which the waste forms from our modeling efforts and data are small as compared to the time frame of the peak dose. We also, based on the results of our seismic consequence work, I felt over the period of the peak, the cladding is likely to be completely degraded by seismic activity. So, even if the waste package may not fail, or it may be able to withstand the seismic activity, you're going to shake it enough to assume all the cladding was breached. So, in this model, you will see there is no instantaneous waste form degradation with no cladding.

For simplicity, we assumed immediate transport through both the unsaturated zone and through the fractured volcanic aquifer region in the saturated zone. I'll talk to this a little bit more later.

Next, please. It's a little more detail in the approaches. Again, we used the long-term average infiltration rates for the various climate states to produce the time-integrated average rate for the long-term average climate. Essentially, as Tim McCartin mentioned this morning, he took the climate model we had that had these immediate transitions, and did a time-integrated average, and for the medium infiltration case, you will see the value he mentioned this morning of 26.6. So, I had an independent verification. It was nice.

You also see the range that we--it's a slightly lower than what the NRC proposed. We also assumed the average percolation flux equals the average infiltration rate, and recognized that the spatial variability in the percolation flux at the repository horizon has a pretty significant effect on seepage properties and advective transport rates.

So, you see right here is a curve out of one of our model reports, the UZ flow models and submodels. That curve gives a normalized cumulative percentage of the repository as a function of normalized flux, you can take this curve, use these values, and come up with those curves. So, these are actually the distributions of percolation flux over the repository horizon that were used in the model. The inset shows the glacial transition climate. So, you can see we're actually somewhat larger in terms of percolation flux than that one.

Next, please. Why is that variability important? It drives the drift seepage model, and as I've mentioned, we used the collapse drift seepage model. If you go back to Jens' presentation, he has a comparison of both a slide of the non-collapsed and the collapsed drift, and they are significantly different. You get significantly more seepage for a given percolation flux for the collapsed drift seepage.

So, we applied the distribution of percolation flux to these curves, essentially had lookup tables to calculate both a fraction of waste packages that would see seeping, and the average value of--the average seepage rate over those waste package that experienced seepage. It's a much simpler approach that's used in the seepage abstraction model. We essentially did a spreadsheet approximations of that, did comparisons back to that and realized we had to do some adjustments for spatial variability and flow focusing, which we did. It's documented in the report.

And, this figure shows you kind of the ranges, and you can see between the collapsed drift and non-collapsed drift a fairly significant increase in seepage fraction, and the amount of water seeping into the drifts and contacting those waste packages. You can see you get up a mean of 88, 74 to 88 on these number of waste packages contacted. So, with that collapsed drift seepage flux, we get an awful lot of waste packages seeing water under this end. There's quite a significant amount of water, which plays a role later on.

Next, please. Another important piece of this is the in-drift environment, and I'm going to focus on the thermal hydrology piece. We did use some long-term chemistry from the engineered barrier system physical and chemical environment model, and used very long-term in-drift chemistry to calculate both partial pressure of CO_2 and the invert, the pH in the invert.

More importantly is the thermal response to the repository and how that's used. Essentially, this is out of the thermal hydrology model, multi-scale thermal hydrology model, and this shows the range of temperatures waste packages will go through for about 20,000 years. And, you see they will go through a cool range and they will go through a hot range, and there's actually a wide variation within here.

We took this data and used the historical information that showed at about 100,000 years, everything went to about 21 degrees C, and just did a simple extrapolation. And, what you're seeing here is kind of a tabulated value of the low and the high, and that will play a role in the waste package degradation approach we took that I'll talk to I believe on the next slide.

Before waste package degradation, we'll talk how we treated the drip shield. We used the probability distributions of general corrosion rates from the long-term test facility measurements, and those were determined in the analysis of them to represent uncertainty. There is both weight loss and crevice specimens, and consistent with the modeling approach that's being done for the 10,000 year case, we applied the weight loss to the underside and we applied both the weight loss and crevice specimen to the outer surface to calculate kind of an average corrosion rate of this drip shield. And, just calculated how long it would take given a realized value of the corrosion rate to penetrate, and what you're seeing on this curve is a distribution of time that the drip shield fails. And, since this all represents uncertainty, this is a time that they all fail completely, and that's how we handled it in this model. So, roughly 50 per cent at 20,000, 30,000 years. I can't quite see that.

So, how the model would work is the drip shield, we would calculate the corrosion rate at a given point in time, it would be completely gone with respect to a barrier for advective transport.

For the waste package degradation due to general corrosion, we did a simplified approach to represent the results that are described in the WAPDEG Analysis of Waste Package and Drip Shield Degradation analysis and model report. Again, we applied the probability distribution of general corrosion rates from the long-term test facility. They represent variability, and essentially are applied in this approach as variability in corrosion rates across the waste package surface. They have roughly 1,000 patches that are sampled, and they sample over a probability distribution.

There's also a temperature dependence that's applied, and we applied it down to a limit of 45 degrees C. So, as the temperature decreased, the corrosion rate decreased until we hit 45 degrees C, and then we held it. So, even though the temperature of the repository is going down to 21, we held it at 45 degrees. We did an analysis of what would happen if we let the thermal, the temperature dependence go all the way down to 21 degrees, and I'll talk to that in a little bit.

So, we determined the time for the initial waste package general corrosion penetration, the very first failure for the coolest and hottest waste package. Essentially took that curve, and used the temperature dependence, used the sampled corrosion rates across the repository surface, found out the first one that would fail, and then using the temperature dependence, came up with a range of time for initial waste package for the coolest and hottest.

We then assumed a uniform distribution of failures between the coolest and hottest waste package. And, that gives these curves here. What you're seeing is the percentiles of the curves, kind of like what Tim McCartin was showing earlier, they are very steep, you know, several tens of thousands of years, but there's considerable uncertainty and you can imagine you're moving along, and then you're going to follow up one of these curves, you're going to start the first waste package failure due to general corrosion, and then the last one would be right there. But, that period of time moves around.

ARNOLD: Is it correct or a slip of the tongue that the upper curve shows a 50 per cent at 2 or 300,000 years, not--

NUTT: Sorry, thank you. I'm trying to read at a slant the fuzzy chart. Thank you.

We also adjusted the average number of general corrosion breaches on a penetrated waste package. This model has a method for calculating how many, of the waste packages that are failed, what is the average number of general corrosion breaches on it. And, it's calculated in this report. They have an example calculation that goes down to the 21 degrees C. We adjusted it upwards due to limiting this temperature variability, and that has an effect on the

EBS transport model.

Real quick on the chemistry. We essentially, in order to drive the solubility models later, we needed to come up with estimates of in-package chemistry. Essentially took two approaches. We realized that looking at the results of some of the historical documents, that it looks like it's about a fifty-fifty chance with our modeling results of will the waste package initially breach by stress corrosion cracks or will it initially breach by a general corrosion penetration.

What we assumed is when it cracks, there will be enough time for the internals to fully degrade when you'll get the first general corrosion breach. So, we wanted to apply those chemistry conditions at that point in time. When we assumed that the other 50 per cent of the time, we said well, it's going to be a general corrosion breach that initially degrades the waste package, so we wanted to apply waste package internals chemistry that was more along the lines of what would be controlled by internal waste package degradation.

And, you will see that in here. There is not a difference in the low value of the pH. There is some difference as you head up into these lower CO_2 fugacities where you will get a higher pH value. And, in the end, this really wasn't an important parameter.

Next, please. We used the results of past TSPAs. This is not a full inventory case. You won't see technetium. You won't see iodine. We basically used the results of past cases to kind of come up with somewhat of a limit. It didn't turn out to be too limited. We used the TSPA-FEIS to point out these radionuclides we considered. EPRI pointed out in their report probably Thorium 229 and Uranium 233, and we got the significant daughter products in there, and additional radionuclides in the decay series. And, this shows the example of the inventories and the radionuclides we used.

Next, please. Radionuclide release from the engineered barrier system. Again, we considered only radionuclide transport out of the engineered barrier system via advection. So, as I pointed out, there's a fraction of the waste packages that don't see water. In a sense, those were not considered. They did not have, since they were diffusive transport pathway, we did not consider them. We only looked at the ones that were flowing water and would calculate the dose based on their advective transport.

We used a 1-D transport using mixing cells, very similar to the approach that's documented in the Project's documentation, essentially a waste form, a waste package, internals and invert, both what Dave Sassani mentioned and what Ernie and Rob had talked about earlier. The only real difference is we're only using one column. We're not discretizing across the repository surface or area.

We applied solubility limits in each mixing cell, and full dose out of the dissolved concentration limits AMR that Dave had mentioned. Again, we used NpO_2 as the controlling phase, unlike that AMR and as part of the difference is things have evolved since early spring 2005, as we used it in both the invert and in the waste package, unlike currently where this is in the waste package, and Np_2O_5 in the invert.

There's others we looked at in sensitivity analysis that I'll talk a little bit. As I mentioned early, it was a function of the in-package environment. We looked at reversible sorption on both the waste package internals and the invert. This is another difference. As Rob pointed out, they aren't looking at reversible sorption on the internals anymore. But, they are looking at reversible and that's not considered in ours.

Next, please. Natural barrier system beneath the repository. Mainly for simplicity, we assumed immediate radionuclide transport through the unsaturated zone and through the fractured volcanic aquifer in the saturated zone, essentially took it right up to the alluvium. And, as Bruce had mentioned, this does depend heavily on radionuclide specific.

For moderately sorbing radionuclides the UZ breakthrough is comparable to the waste package failure time. So, the assumption may not be too bad. You essentially end up with a steady state where the release of the moderately sorbing radionuclides, where the mass flux out, essentially becomes equal to mass flux in. Also saw, and this is a conservative assumption that we made, and probably may not be appropriate for things like plutonium, if you look into our report, you will see that Plutonium 242 is one of the dominating radionuclides. That may not be the case if we had done a full UZ or when a full UZ is done.

We saw the saturated zone breakthrough on the order of several tens to hundreds of thousands of years for strongly sorbing radionuclides. Most of the delay was in the alluvium. If you refer back to some of Bill's talks, you can kind of see where the alluvium got you. So, it results in a significant retardation of protactinium, plutonium and thorium. And, we also expect significant retardation of them in both the UZ and the fractured volcanic portion.

That's part of the reason that we are looking at these results, is to look at the comparisons. Don't look at the magnitude, don't do any comparisons to the absolute magnitude of the numbers, is things like that are not in it.

We used the biosphere dose conversion factors for the glacial transition conditions with the new guidance. We used what is in the current rule for the dose conversion factor. So, it's updated. I know you were having a discussion tomorrow about the--this morning about the differences between the two.

Next, please. Treatment of seismic disruptive events. This is where the fun kind of began for me. As you heard earlier, the waste package damage from vibratory ground motion is expected to result in an increased susceptibility to stress corrosion cracking. And, over the 10,000 years, the waste package is protected by the drip shield from rock fall. The consequence of rock fall on the waste packages haven't been analyzed because of the protection of the drip shield over the 10,000 year period.

Well, we have analyzed the consequences of rock fall on the drip shield and, again, it is an increased susceptibility to stress corrosion cracking. So, you can make the extrapolation, I'll say, of the likely consequence of rock fall on a waste package would be increased susceptibility to stress corrosion cracking. Again, this is mainly in the non-lithophysal unit where you can get the real big rocks to come falling down.

So, although we feel that stress corrosion cracking is likely to be the dominant failure mode, and it will result in those diffusive transport mechanisms which are of lower importance than the establishment of the advective transport mechanisms, we did a sensitivity analysis where we assumed that both vibratory ground motion and rock fall resulted in gross breaching of the waste packages.

We essentially considered multiple seismic events over 1,000 years. We let them occur to a poisson process. We randomly sampled the magnitude of the event, the peak ground velocity, and then calculated or estimated the amount of gross breaching as a function of magnitude. And, how we estimated that, the amount, was we took the area that these models calculated as being damaged on a drip shield, and said that area, instead of being a stress corrosion crack, is gone. So, we essentially just added up and assumed very gross breaching, and then accumulated consequences as we went along. So, the waste package could see multiple events and get an accumulation of damage with more and more holes.

Next, please. The treatment of the igneous disruptive events, we did consider two cases of that, and there was simplified representation based on the approach described in the igneous consequence AMR.

For the igneous intrusion, we assumed one event every realization, with the timing and magnitude of that event uncertain. So, it occurred anywhere within the time frame of the million year period we were considering. We calculated the number of dikes that crossed or intersected the waste packages, failed those waste packages completely. Let the rest of them kind of follow the general corrosion mechanism. So, in a sense, I want to call it the nominal scenario, but we used our base scenario of general corrosion going on for those waste packages that weren't intersected by the event.

We sampled the uncertain parameters of how many waste packages would be intersected, and calculated what the dose would be. And, for the volcanic eruption, this was more of a confirmation. As Tim McCartin mentioned this morning, the radionuclides are going away. We wanted to do a simplified reproduction of volcanic eruption to see if any other radionuclides might be building in out in the period of time.

So, we assumed, generated very similar to the approach being taken, recommended for the performance assessment, essentially generated 1,000 realizations with each realization producing a dose history, with a series of eruptive events, essentially erupting one each time step, and then back weighted the probability out to come up with a risk curve that's similar to what you've seen before.

The results. Again, I really want to focus the discussion on the sensitivities and not the magnitude and timing of the peak annual dose. And, like I said before, those curves were calculated to get at what we're really after, is what is important, what's driving things.

So, what we found is the peak annual dose really depends on what I'll call the nominal degradation processes. The gradual degradation of the engineered barriers and subsequent release of radionuclides contained within them. In particular, the dose depends on the timing and rate of waste package failure due to general corrosion processes, and the rate the water transports radionuclides out of the EBS. That's the source term. To me, it's not surprising that our waste package performance out at that period of time has a role. If they don't fail significantly and grossly, you won't have a significant release.

We feel seismic events will occur, and although the seismic induced mechanical damage may influence the annual dose prior to the onset of significant waste package failure due to general corrosion, it is not expected to have a significant effect on the peak annual dose, either the mean or the median.

And, what we mean by that is it will tend to control--it can affect the magnitude in the transient phases, but it will be when you get up to the period where you get the large scale general corrosion breach of the waste packages, where we really see the magnitude of the peak dose go up. And, that's even with that very what I feel conservative model where we did the--every seismic event caused some sort of gross breaching, gross damage to the waste package.

Igneous intrusions are the same way, although, again, it's a low probability event over the period of the peak dose. Unlike a seismic event, which we will have over that million year period, the likelihood of getting an igneous is still a low probability disruptive event.

Again, what we saw is it may control the curve, the risk/dose curve over the period prior to the onset of significant gross breach in the waste packages due to corrosion. And, we don't believe it will have a significant effect on the measure of the peak annual dose, mean or median.

We also, again, I said we looked at the volcanic eruption and found that no, there are no radionuclides that are building in. The decrease that you've seen in the past in terms of the volcanic eruption will continue onward, and essentially the risk of this event will be most significant during the 10,000 year period.

Next, please. The sensitivity analysis. This is more what we're looking for. We found that the infiltration rates and percolation rates through the repository have what I will call a minor effect on the magnitude of the peak dose. Some of these minor or significant, I'm going to claim are my judgment. The report has values of the changes that we saw. I'll invite you to judge for yourself whether it's significant or not.

Over the range of repository average infiltration rates that were--this is true over the range of repository average infiltration rates that were representative of this long-term climate scenario when we considered collapsed drift seepage. We did the infiltration, kind of looked at the high value and the low value and see what the differences were, and we did that for this collapsed drift seepage.

We found that emplacement drift seepage has a significant effect on the magnitude of the peak dose, and how we accomplished that was just to switch to non-collapsed drift seepage, and there was a fairly significant, about a 70 per cent drop in the mean, about an 86 per cent drop in the median, and it tended to move things further out in time.

Again, no surprise, we saw the waste package performance and in particular, general corrosion rates had a significant effect on the peak dose, both the magnitude and timing. Essentially, we ran a series of sensitivity studies. We look at increasing the general corrosion rates of both the drip shield and the waste package by a factor of 5. That caused both the magnitude to increase, and the peak to move closer in time. It, again, affects how some radionuclides decay, but it also affects the rate that the waste packages are failing, and releasing material.

We also did the full temperature dependence. We let the temperature dependence go down to 21 degrees C, and saw about a 30 per cent reduction in the dose. But, more importantly, it moved the measure of the peak way out in time, well past a million years.

The results of the sensitivity model indicated that the choice of a controlling solubility phase for neptunium dissolved concentration limits didn't really have a significant effect. And, again, that was for the range of parameters included in the model. The high drifts, in particular, the collapsed drift seepage. Even if you would invoke solubility limits, you are flowing so much through that you can deplete the inventory fairly quickly in a lot of the realizations.

We did see that if we lowered the seepage rates, we did the same scenario where we went back to the non-collapsed drift seepage rates, and rechecked the two different solubility approaches. This one, the secondary phase solubility control when we did this one.

The lower seepage rates did have an effect. You saw more of the solubility was controlling and having a role in the release rate.

We found that the drip shield performance had a minor effect on the peak dose. It is important early on, it does provide, in the earlier time periods, a barrier for water to get to the waste package, also provides a barrier, a very protective barrier for the waste package against rock fall.

But, at the period of the peak, it tends to be for the most part completely degraded. Yes, there are realizations where it will be there if you look back at that curve, but for the most of the realizations, it tends to be gone by the time we get the general corrosion breach of the waste package, and it doesn't play a significant role.

We feel that the natural barrier system below the repository, mainly the alluvial portion of the saturated zone, is an effective barrier for several key radionuclides. And, again, it kind of goes back to what Bruce and Bill were saying that strongly sorbing radionuclides could be significantly retarded, which the sensitivity analysis in there where we said, well, we're just going to assume the I'll call it ludicrous assumption of no alluvium, and just to test the model and see what would happen, and there's a significant increase in the very highly sorbing radionuclides, the protactinium, these long-lived, highly sorbing.

Next, please. On that, I'm not going to repeat this slide to say more than you saw at the beginning. But, I really want to caution you on what you and anybody else who uses these, what these results mean. They are not to be compared to any proposed standard.

GARRICK: Thank you very much, Mark.

Because of the late hour, and the fact that the Board has other commitments this evening, I think I'm going to not ask questions at this point. So, I think we will end it.

But, on the other hand, I do want to make a couple of comments. The first comment is, of course, to thank the presenters and the briefers. Every time I sit through one of these, I envy much of the work that's done, and that I can't be an intimate part of it. And, I was very impressed with the quality of the individuals that made the presentations. And, that's the good news.

The bad news is that I'm afraid if I had to have another meeting like this, you would force me to retirement, because in my opinion, what we have been through is unacceptable for two reasons. One, we didn't get what we wanted in terms of really getting an understanding, a fundamental understanding of the mass and curie balance of the repository.

Now, I understand full well why we didn't, and I know they did the very best they could do. But, the point is the Board didn't get what they wanted. And, until we do, we're not going to be able to evaluate, as we're supposed to do, and convince ourselves that there is a fundamental understanding of the issue having to do with the radionuclide transport through the repository. And, as I say, we know why there are extenuating circumstances. There's budget issues. There's legal issues, and a number of factors that have contributed to the constraints that the DOE is having to work for. And, my comments apply to the DOE presenters.

The second thing that I want to comment on is that we failed miserably to comply with what this Board has come to invoke, namely the fifty-fifty rule, that for the allotted time for the presentations, half of it is supposed to be the presentation, and half of it is supposed to be for the Board to be able to ask the questions it asks. And, we have had to unduly truncate questions from the Board all day because of that, and that is completely unacceptable.

I don't know what happened to that. The last meeting, it worked very well. It worked very well today for the NRC presenters. They followed the fifty-fifty rule, but nobody else did. This last presentation, which was an outstanding presentation, and the work is really important and good, but if you think in terms of the time that was allotted for the presentation, which was 25 minutes, and you invoke the 50 per cent rule, it should have been 13 minutes, and the presentation was three times as long as it should have been. And, we've got to do something about that if we're going to be effective in our ability to implement our mandate. And, it's just not happening.

So, this is why I'm unhappy, even though as far as the quality of the presentations and the quality of the material and the circumstances under which it was presented was exemplary, and I just hope that in our next meeting, we are able to do a better job of putting ourselves in a position to do our job, because I certainly don't think we were in that position this time.

And, I guess I leave a note of frustration about that, but on the other hand, I think that it's important for us to communicate directly to DOE how we feel, and when we don't feel we're getting what we want, we so say it, and we didn't get what we wanted in this meeting.

And, number two, you didn't, the whole approach didn't allow us to have the kind of exchanges that I think we absolutely have to have in future meetings.

Now, are there any other follow-ups or comments from other members of the Board?

And, I know we're all very tired, and we will I'm sure rectify the situation and we'll see you again soon, and we'll look forward to that, and we will now adjourn.

(Whereupon, at 6:11 p.m., the meeting was adjourned.)